PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE,

TWENTY-FIFTH MEETING,

HELD AT

BUFFALO, N. Y.,

AUGUST, 1876.

SALEM:
PUBLISHED BY THE PERMANENT SECRETARY.
1877.

EDITED BY

FREDERICK W. PUTNAM,

Permanent Secretary.

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TABLE OF CONTENTS.

COMMUNICATIONS.

SECTION A. MATHEMATICS, PHYSICS, AND CHEMISTRY.

I. MATHEMATICS.

II. PHYSICS.

III. TITLES

IV. PERMANENT SUBSECTION OF CHEMISTRY.

V. TITLES

SECTION B. NATURAL HISTORY.

I. GENERAL BIOLOGY.

II. GEOLOGY.

III. BOTANY.

IV. ZOOLOGY.

V. TITLES

VI. PERMANENT SUBSECTION OF ANTHROPOLOGY.

VII. TITLES

EXECUTIVE PROCEEDINGS.

CORRECTION.

Page 154, line 15, *insert* "of the antennæ" *before* "of butterflies."

OFFICERS OF THE BUFFALO MEETING.

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WILLIAM B. ROGERS, of Boston.

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* Not present.

OFFICERS OF THE SECTIONS.

SECTION A.

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H. WHEATLAND, of Salem; W. C. KERR, of Raleigh;

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ANTHROPOLOGY.

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O. T. MASON, of Washington, *Secretary.*

SUBSECTION F.

GEOLOGY.

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THEO. B. COMSTOCK, of Ithaca, *Secretary.*

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OF

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Secretary:—JOHN GEORGE MILBURN.
Treasurer:—GEORGE P. PUTNAM.

SUBCOMMITTEES.

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JOSIAH LETCHWORTH, DAVID GRAY,
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JOSIAH LETCHWORTH.

On Excursions.

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On Railroads.

CYRUS CLARKE, *Chairman.*

W. H. GLENNY, Jr. H. L. LYMAN.

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Hon. A. S. BEMIS, President of the Common Council of the City of Buffalo. E. C. Sprague, John B. Greene, E. S. Hawley, O. H. Marshall, Hon. E. G. Spaulding, Prof. George Hadley, M.D., Joseph Warren, James N. Matthews, David Gray, James O. Putnam, Rev. A. T. Chester, D.D., Dennis Bowen, Wm. H. Greene, Hon. James M. Smith, Hon. James Sheldon, Wm. C. Bryant, F. Sidway, Dr. George E. Hayes, Henry Chandler, Henry S. Sprague, Leon F. Harvey, M.D., Frank Hollister, David F. Day, William Thurston, H. M. Kent, Dexter P. Rumsey, S. S. Guthrie, George Howard, S. K. Worthington, A. Altman, Walter T. Wilson, Cyrus P. Lee, William H. Pitt, H. M. Clay, Chas. D. Marshall, Ottomar Reinecke, Dr. M. B. Folwell, Henry Ware Sprague, Sheldon T. Viele, Everard D. Palmer, Joseph Churchyard.

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1. *Permanent Committee on Weights, Measures and Coinage.*

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Wolcott Gibbs, of Cambridge,
B. A. Gould, of Boston,
Joseph Henry, of Washington,
J. E. Hilgard, of Washington,
John LeConte, of Oakland, Cal.,
H. A. Newton, of New Haven,
Benjamin Peirce, of Cambridge,
William B. Rogers, of Boston,
J. Lawrence Smith, of Louisville,
E. B. Elliott, of Washington.

2. *Committee to Memorialize the Legislature of New York for a New Survey of Niagara Falls.*

F. A. P. Barnard, of New York,
Charles P. Daly, of New York,
James Hall, of Albany,
G. W. Holley, of Niagara Falls.

3. *Committee to Memorialize Congress and State Legislatures regarding the Cultivation of Timber, and the Preservation of Forests.*

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Asa Gray, of Cambridge,
G. B. Emerson, of Boston,
J. D. Whitney, of Cambridge,
J. S. Newberry, of Cleveland,
L. H. Morgan, of Rochester,
Chas. Whittlesey, of Cleveland,
W. H. Brewer, of New Haven,
E. W. Hilgard, of Berkeley.

B. NEW COMMITTEES.

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William B. Rogers, of Boston; James Hall, of Albany; J. W. Dawson, of Montreal; J. S. Newberry, of New York; T. Sterry Hunt, of Boston; C. H. Hitchcock, of Hanover; Raphael Pumpelly, of Owego; in connection with Dr. Otto Torrell, of Stockholm; Dr. E. H. von Baumhauer, of Haarlem; Prof. T. H. Huxley, of London.

Committee to Audit the Accounts of the Permanent Secretary and Treasurer.

Henry Wheatland, of Salem,
S. H. Scudder, of Cambridge.

OFFICERS OF THE ASSOCIATION

ELECTED FOR

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EDWARD C. PICKERING, of Cambridge.

VICE PRESIDENT, Section B.

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N. T. LUPTON, of Nashville.

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R. H. WARD, of Troy.

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GENERAL SECRETARY.

AUGUSTUS R. GROTE, of Buffalo.

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H. CARRINGTON BOLTON, of New York.

SECRETARY OF SECTION B.

WILLIAM H. DALL, of Washington.

TREASURER.

WILLIAM S. VAUX, of Philadelphia.

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The President, Vice Presidents, Secretaries and Treasurer of the Meeting.

From the Association at large.—(Six Fellows to be elected on the first day of the meeting.)

LOCAL COMMITTEE

FOR THE

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Chairman:—His Excellency JAMES D. PORTER.
Treasurer:—Hon. MARSH T. POLK.
Secretary:—Dr. J. BERRIEN LINDSLEY.

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Telegraph and Mail:—G. W. TRABUE, *Chairman;* J. S. Carels, A. C. Ford.

Microscopes:—T. O. SUMMERS, jr., *Chairman;* G. S. Blackie, Alex. Winchell.

MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION OF GEOLOGISTS AND NATURALISTS.

MEETING.	DATE.	PLACE.	CHAIRMAN.	SECRETARY.	ASSIST. SEC'Y.	TREASURER.
1st	April 2, 1840,	Philadelphia,	Edward Hitchcock,*	L. C. Beck,*		
2d	April 5, 1841,	Philadelphia,	Benjamin Silliman,*	L. C. Beck,*	B. Silliman, jr., C. B. Trego, J. D. Whitney, M. B. Williams,	
3d	April 25, 1842,	Boston,	S. G. Morton,*	C. T. Jackson,		
4th	April 26, 1843,	Albany,	Henry D. Rogers,*	B. Silliman, jr.,		John Locke.*
5th	May 8, 1844,	Washington,	John Locke,*	B. Silliman, jr., O. P. Hubbard,		Douglas Houghton.*
6th	April 30, 1845,	New Haven,	Wm. B. Rogers,	B. Silliman, jr., J. Lawrence Smith,		Douglas Houghton.*
7th	Sept. 2, 1846,	New York,	C. T. Jackson,	B. Silliman, jr.,		E. C. Herrick.*
8th	Sept. 20, 1847,	Boston,	Wm. B. Rogers,†	Jeffries Wyman,*		B. Silliman, jr.

*Deceased.

†Professor ROGERS, as chairman of this last meeting, called the first meeting of the new Association to order and presided until it was fully organized by the adoption of a constitution. As he was thus the first presiding officer of the new Association, it was directed at the Hartford meeting that his name be placed at the head of the Past Presidents of the American Association for the Advancement of Science.

MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

MEETING.	DATE.	PLACE.	PRESIDENT.	VICE-PRESIDENT.	GENERAL SECRETARY.	PERMANENT SEC'Y.	TREASURER.
1st	Sept. 20, 1848.	Philadelphia, Pa.,	W. C. Redfield,*		Walter R. Johnson,*		Jeffries Wyman.*
2d	Aug. 14, 1849.	Cambridge, Mass.,	Joseph Henry,		E. N. Horsford, 1		A. L. Elwyn.
3d	Mar. 12, 1850.	Charleston, S. C.,	A. D. Bache,* 2		L. R. Gibbs, 3		St. J. Ravenel.* 4
4th	Aug. 19, 1850.	New Haven, Conn.,	A. D. Bache,*		E. C. Herrick,*		A. L. Elwyn.
5th	May 5, 1851.	Cincinnati, Ohio,	A. D. Bache,*		W. B. Rogers, 5	S. F. Baird,	S. F. Baird. 6
6th	Aug. 19, 1851.	Albany, N. Y.,	Louis Agassiz,*		W. B. Rogers,	S. F. Baird,	A. L. Elwyn.
7th	July 28, 1853.	Cleveland, Ohio,	Benjamin Pierce,		S. St. John, 7	S. F. Baird,	A. L. Elwyn.
8th	April 26 1854.	Washington, D.C.,	J. D. Dana,		J. Lawrence Smith,	Joseph Lovering,	J. L. LeConte. 8
9th	Aug. 15, 1855.	Providence, R. I.,	John Torrey,*		Wolcott Gibbs,	Joseph Lovering,	A. L. Elwyn.
10th	Aug. 20, 1856.	Albany, N. Y.,	James Hall,		B. A. Gould,	Joseph Lovering,	A. L. Elwyn.
11th	Aug. 12, 1857.	Montreal, Canada.	Alexis Caswell,* 9	Alexis Caswell,*	John LeConte,	Joseph Lovering,	A. L. Elwyn.
12th	April 28, 1858.	Baltimore, Md.,	Alexis Caswell,* 10	John E. Holbrook,*†	W. M. Gillespie,* 11	Joseph Lovering,	A. L. Elwyn.
13th	Aug. 3, 1859.	Springfield, Mass.,	Stephen Alexander,	Edward Hitchcock,*	William Chauvenet,*	Joseph Lovering,	A. L. Elwyn.
14th	Aug. 1, 1860.	Newport, R. I.,	Isaac Lea,	B. A. Gould,	Joseph LeConte,	Joseph Lovering.	A. L. Elwyn.
15th	Aug. 15, 1866.	Buffalo, N. Y.,	F. A. P. Barnard,	A. A. Gould,* 12	Elias Loomis, 13	Joseph Lovering,	A. L. Elwyn.†
16th	Aug. 21, 1867.	Burlington, Vt.,	J. S. Newberry,	Wolcott Gibbs,	C. S. Lyman,	Joseph Lovering,	A. L. Elwyn.†
17th	Aug. 5, 1868.	Chicago, Ill.,	B. A. Gould,	Charles Whittlesey,	Simon Newcomb, 14	Joseph Lovering,	A. L. Elwyn.†
18th	Aug. 18, 1869.	Salem, Mass.,	J. W. Foster,*	O. N. Rood,	O. C. Marsh,	F. W. Putnam, 15	A. L. Elwyn.†
19th	Aug. 17, 1870.	Troy, N. Y.,	T. S. Hunt, 16	T. S. Hunt,	F. W. Putnam, 17	Joseph Lovering,	A. L. Elwyn.†
20th	Aug. 16, 1871.	Indianapolis, Ind.,	Asa Gray,	G. F. Barker,	F. W. Putnam,	Joseph Lovering,	W. S. Vaux.
21st	Aug. 15, 1872	Dubuque, Iowa,	J. Lawrence Smith,	Alex. Winchell,	E. S. Morse,	Joseph Lovering,	W. S. Vaux.
22d	Aug. 20, 1873.	Portland, Me.,	Joseph Lovering,	A. H. Worthen,†	C. A. White,	F. W. Putnam,	W. S. Vaux.
23d	Aug. 12, 1874.	Hartford, Conn.,	J. L. LeConte,	C. S. Lyman,	A. C. Hamlin,	F. W. Putnam.	W. S. Vaux.†

1. In place of Jeffries Wyman, *not present.*
2. In place of Joseph Henry, *not present.*
3. In place of E. C. Herrick, *not present.*
4. In place of A. L. Elwyn, *not present.*
5. In place of E. C. Herrick, *not present.*
6. In place of A. L. Elwyn, *not present.*
7. In place of J. D. Dana, *not present.*
8. In place of A. L. Elwyn, *not present.*
9. In place of J. W. Bailey, *deceased.*
10. In place of Jeffries Wyman, *not present.*
11. In place of Wm. Chauvenet, *not present.*
12. In place of R. W. Gibbes, *not present.*
13. In place of W. P. Trowbridge, *not present.*
14. In place of A. P. Rockwell, *called home* 1st *day.*
15. In place of Joseph Lovering, *in Europe.*
16. In place of Wm. Chauvenet, *too ill to be present.*
17. In place of C. F. Hartt, *in Brazil.*

* Deceased. † Not present at the meeting.

MEETINGS AND OFFICERS OF THE ASSOCIATION (*Continued*).

MEETING.	DATE.	PLACE.	PRESIDENT.	VICE PRESIDENT, SECTION A.
24th	Aug. 11, 1875,	Detroit, Mich.,	J. E. Hilgard,	H. A. Newton,
25th	Aug. 23, 1876,	Buffalo, N. Y.,	William B. Rogers,	Charles A. Young,

VICE PRESIDENT, SECTION B.	CHAIRMAN OF PERMANENT SUBSECTION C, CHEMISTRY.	CHAIRMAN OF PERMANENT SUBSECTION D, ANTHROPOLOGY.	CHAIRMAN OF PERMANENT SUBSECTION E, MICROSCOPY.
J. W. Dawson,	S. W. Johnson,	L. H. Morgan,	——— ———
Edward S. Morse,	George F. Barker,	L. H. Morgan,	R. H. Ward,

PERMANENT SECRETARY.	GENERAL SECRETARY.	SECRETARY OF SECTION A.	SECRETARY OF SECTION B.
F. W. Putnam,	S. H. Scudder,	{ S. P. Langley, T. C. Mendenhall,	E. S. Morse,
F. W. Putnam,	T. C. Mendenhall,	A. W. Wright,	Albert H. Tuttle,

SECRETARY OF PERMANENT SUBSECTION C, CHEMISTRY.	SECRETARY OF PERMANENT SUBSECTION D, ANTHROPOLOGY.	SECRETARY OF PERMANENT SUBSECTION E, MICROSCOPY.	TREASURER.
F. W. Clarke,	F. W. Putnam,	——— ———	W. S. Vaux.
H. C. Bolton,	O. T. Mason,	E. W. Morley,	W. S. Vaux.

COMMONWEALTH OF MASSACHUSETTS.

IN THE YEAR ONE THOUSAND EIGHT HUNDRED AND SEVENTY-FOUR.

AN ACT

To Incorporate the "American Association for the Advancement of Science."

Be it enacted by the Senate and House of Representatives, in General Court assembled, and by the authority of the same, as follows:

Section 1. Joseph Henry of Washington, Benjamin Pierce of Cambridge, James D. Dana of New Haven, James Hall of Albany, Alexis Caswell of Providence, Stephen Alexander of Princeton, Isaac Lea of Philadelphia, F. A. P. Barnard of New York, John S. Newberry of Cleveland, B. A. Gould of Cambridge, T. Sterry Hunt of Boston, Asa Gray of Cambridge, J. Lawrence Smith of Louisville, Joseph Lovering of Cambridge and John LeConte of Philadelphia, their associates, the officers and members of the Association, known as the "American Association for the Advancement of Science," and their successors, are hereby made a corporation by the name of the "American Association for the Advancement of Science," for the purpose of receiving, purchasing, holding and conveying real and personal property, which it now is, or hereafter may be possessed of, with all the powers and privileges, and subject to the restrictions, duties and liabilities set forth in the general laws which now or hereafter may be in force and applicable to such corporations.

Section 2. Said corporation may have and hold by purchase, grant, gift or otherwise, real estate not exceeding one hundred thousand dollars in value, and personal estate of the value of two hundred and fifty thousand dollars.

Section 3. Any two of the corporators above named are hereby authorized to call the first meeting of the said corporation in the month of August next ensuing, by notice thereof "by mail," to each member of the said Association.

Section 4. This act shall take effect upon its passage.

House of Representatives, March 10, 1874.
Passed to be enacted,
John E. Sanford, *Speaker.*

In Senate, March 17, 1874.
Passed to be enacted,
Geo. B. Loring, *President.*

March 19, 1874.
Approved,
W. B. Washburn.

Secretary's Department,
Boston, April 3, 1874.
A true copy, Attest:
David Pulsifer,
Deputy Secretary of the Commonwealth.

CONSTITUTION

OF THE

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Incorporated by Act of the General Court of the Commonwealth of Massachusetts.

Objects.

Article 1. The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of America, to give a stronger and more general impulse and more systematic direction to scientific research, and to procure for the labors of scientific men increased facilities and a wider usefulness.

Members, Fellows, Patrons and Honorary Fellows.

Art. 2. The Association shall consist of Members, Fellows, Patrons and Honorary Fellows.

Art. 3. Any person may become a Member of the Association upon recommendation in writing by two members or fellows, nomination by the Standing Committee, and election by a majority of the members and fellows present in general session.

Art. 4. Fellows shall be nominated by the Standing Committee from such of the members as are professionally engaged in science, or have by their labors aided in advancing science. The election of fellows shall be by ballot and a majority vote of the members and fellows present in general session. But all persons who may be members at the time of the adoption of this constitution may become fellows by signifying their desire to this effect before the first day of August, 1875.

Art. 5. Any person paying to the Association the sum of one thousand dollars shall be classed as a Patron, and shall be entitled to all the privileges of a member and to all its publications.

ART. 6. Honorary Fellows of the Association, to the number of ten for each section, may be elected; the nominations to be made by the Standing Committee and approved by ballot in the respective sections before election by ballot in general session. Honorary Fellows shall be entitled to all the privileges of fellows and shall be exempt from all fees and assessments, and entitled to all publications of the Association issued after the date of their election.

ART. 7. The name of any member or fellow two years in arrears for annual dues shall be erased from the list of the Association, provided that two notices of indebtedness, at an interval of at least three months, shall have been given; and no such person shall be restored until he has paid his arrearages or has been reëlected.

ART. 8. No member or fellow shall take part in the organization or business of both sections at the same meeting.

OFFICERS.

ART. 9. The Officers of the Association shall be elected by ballot in general session from the fellows and shall consist of a President, two Vice Presidents, a General Secretary, a Permanent Secretary, a Treasurer, a Secretary of Section A, and a Secretary of Section B; these, with the exception of the Permanent Secretary, shall be elected at each meeting for the following one, and, with the exception of the Treasurer and the Permanent Secretary, shall not be reëligible for the next two meetings. The Permanent Secretary shall be elected at each fifth meeting.

ART. 10. The President, or, in his absence, one of the Vice Presidents, shall preside at all general sessions of the Association and at all meetings of the Standing Committee. It shall also be the duty of the President to give an address at a general session of the Association at the meeting following that over which he presided.

ART. 11. The Vice Presidents shall be the presiding officers of Sections A and B, and of the Sectional Committees, and it shall be part of their duty to give an address, each before his respective section, at such time as the section shall determine. The Vice Presidents may request their respective sections to appoint temporary chairmen to preside over the sessions of the sections, but shall not delegate their other duties.

ART. 12. The General Secretary shall be the Secretary of all general sessions of the Association, and of all sessions of the Standing Committee, and shall keep a record of the business of these sessions. He shall receive the records from the Secretaries of the Sections, which, after examination, he shall transmit with his own records to the Permanent Secretary within two weeks after the adjournment of the meeting. He shall receive proposals for membership and bring them before the Standing Committee. He shall give to the Secretary of each Section the list of papers assigned to it by the Standing Committee.

ART. 13. The Permanent Secretary shall be the executive officer of the Association under the direction of the Standing Committee. He shall attend to all business not specially referred to committees nor otherwise constitutionally provided for. He shall keep an account of all business that he has transacted for the Association, and make annually at the first meeting of the Standing Committee, a report which shall be laid before the Association. He shall attend to the printing and distribution of the annual volume of Proceedings, and all other printing ordered by the Association. He shall issue a circular of information to members and fellows at least four months before each meeting, and shall, in connection with the Local Committee, make all necessary arrangements for the meetings of the Association. He shall provide the Secretaries of the Association with such books and stationery as may be required for their records and business, and shall provide members and fellows with such blank forms as may be required for facilitating the business of the Association. He shall collect all assessments and admission fees, and notify members and fellows of their election, and of any arrearages. He shall receive, and bring before the Standing Committee, the titles and abstracts of papers proposed to be read before the Association. He shall keep an account of all receipts and expenditures of the Association, and report the same annually at the first meeting of the Standing Committee, and, at the close of each year, shall pay over to the Treasurer such unexpended funds as the Standing Committee may direct. He shall receive and hold in trust for the Association all books, pamphlets and manuscripts belonging to the Association, and allow the use of the same under the provisions of the Constitution and the orders of the Standing Committee. He shall receive all communications addressed to the Association during the interval between meetings, and properly attend to the same. He shall at each meeting report the names of fellows and members who have died since the preceding meeting. He shall be allowed a salary which shall be determined by the Standing Committee, and may employ a clerk at such compensation as may be agreed upon by the Standing Committee.

ART. 14. The Treasurer shall invest the funds received by him in such securities as may be directed by the Standing Committee. He shall annually present to the Standing Committee an account of the funds in his charge. No expenditure of the principal in the hands of the Treasurer shall be made without a unanimous vote of the Standing Committee, and no expenditure of the income received by the Treasurer shall be made without a two-thirds vote of the Standing Committee.

ART. 15. The Secretaries of Sections A and B shall keep the records of their respective sections, and, at the close of the meeting, give the same, including the records of subsections, to the General Secretary. They shall also be the secretaries of the sectional committees.

ART. 16. In case of a vacancy in the office of the President, one of the Vice Presidents shall be elected by the Standing Committee as the President of the meeting. Vacancies in the offices of Vice President, General Secretary, Permanent Secretary and Treasurer, shall be filled by nomination of the Standing Committee and election by ballot in general session. A vacancy in the office of Secretary of a Section shall be filled by nomination and election by ballot in the section.

ART. 17. The Standing Committee shall consist of the past Presidents, the President, the Vice Presidents, the four Secretaries, the Treasurer, with the above named officers of the preceding meeting, and six fellows elected by ballot after open nomination at the first general session. The members present at any regularly called meeting of the Committee, provided there are at least five, shall form a quorum for the transaction of business. The Standing Committee shall meet on the day preceding each annual meeting of the Association, and arrange the programme for the first day of the sessions. The time and place of this first meeting shall be designated by the Permanent Secretary. Unless otherwise agreed upon, regular meetings of the Committee shall be held in the committee room at 9 o'clock, A.M., on each day of the meeting of the Association. Special meetings of the Committee may be called at any time by the President. The Standing Committee shall be the board of supervision of the Association, and no business shall be transacted by the Association that has not first been referred to, or originated with, the Committee. The special business of the Committee shall be: to receive and assign papers to the respective sections; to examine and, if necessary, to exclude papers; to decide which papers, discussions and other proceedings shall be published, and to have the general direction of the publications of the Association; to manage the financial affairs of the Association; to arrange the business and programmes for general sessions; to appoint general sessions for the evening; to suggest subjects for discussion, investigation or reports; to nominate members and fellows; to receive and act upon all invitations extended to the Association and report the same at a general session of the Association.

ART. 18. The Nominating Committee shall consist of the Standing Committee, and four members or fellows elected by each of the sections. It shall be the duty of this Committee to meet at the call of the President and nominate the general officers for the following meeting of the Association. It shall also be the duty of this Committee to recommend the time and place for the next meeting. The Vice Presidents and Secretaries of the Sections shall be recommended to the Nominating Committee by sub-committees consisting of the Vice Presidents and Secretaries, and the four persons elected by each section under the first clause of this article.

MEETINGS.

ART. 19. The Association shall hold public meetings annually, for one week or longer, at such time and place as may be determined by vote of the Association, and the preliminary arrangements for each meeting shall be made by the Local Committee, in conjunction with the Permanent Secretary and such other persons as the Standing Committee may designate.

ART. 20. General Sessions shall be held at 10 o'clock, A. M., unless otherwise ordered, on every day of the meeting, Sunday excepted, and at such other times as may be appointed by the Standing Committee.

SECTIONS AND SUBSECTIONS.

ART. 21. The Association shall be divided into two Sections, namely: **A** (*Mathematics*, *Astronomy*, *Physics*, *Chemistry* and *Mineralogy*) and **B** (*Geology*, *Zoology*, *Botany* and *Anthropology*). Either Section may, at its pleasure, form temporary or permanent subsections for the reading of papers.

ART. 22. Immediately on the organization of a Section there shall be three fellows elected by ballot after open nomination, who, with the Vice President and Secretary, and the Chairman and Secretary of the subsections, shall form its Sectional Committee. The Sectional Committees shall have power to fill vacancies in their own numbers. There shall be no sectional meeting during a general session.

ART. 23. When any Subsection organizes, it shall elect a Chairman and Secretary and report the result to the Secretary of its Section. The Secretary of a Subsection shall, at the close of the meeting, transmit his records to the Secretary of the Section. Any Permanent Subsection may elect its Chairman for the ensuing meeting.

ART. 24. No paper shall be read in any Section or Subsection until it has been placed on the programme of the day by the Sectional Committees.

SECTIONAL COMMITTEES.

ART. 25. The Sectional Committees shall arrange and direct the business of their respective sections. They shall prepare the daily programmes and give them to the Permanent Secretary for printing at the earliest moment practicable. No titles of papers shall be entered on the daily programmes except such as have passed the Standing Committee. No change shall be made in the programme for the day without the consent of the Sectional Committee. The Sectional Committees may refuse to place the title of any paper on the programme; but every such title, with the abstract of the paper or the paper itself must be returned to the Standing Committee with the reasons why it was refused.

ART. 26. The Sectional Committees shall examine all papers and abstracts referred to the sections, and they shall not place on the programme any paper inconsistent with the character of the Association; and to this end they have power to call for any paper, the character of which may not be sufficiently understood from the abstract submitted.

PAPERS AND COMMUNICATIONS.

ART. 27. All members and fellows must forward to the Permanent Secretary, as early as possible, and when practicable before the convening of the Association, full titles of all the papers which they propose to present during the meeting, with a statement of the time that each will occupy in delivery, and also such abstracts of their contents as will give a general idea of their nature; and no title shall be referred by the Standing Committee to the Sectional Committee until an abstract of the paper or the paper itself has been received.

ART. 28. If the author of any paper be not ready at the time assigned, the title may be dropped to the bottom of the list.

Reports of Meetings.

Art. 29. Whenever practicable, the proceedings and discussions at general sessions, sections and subsections shall be reported by professional reporters, but such reports shall not appear in print as the official reports of the Association unless revised by the secretaries.

Printed Proceedings.

Art. 30. The Permanent Secretary shall have the Proceedings of each meeting printed in an octavo volume as soon after the meeting as possible, beginning one month after adjournment. Authors must prepare their papers ready for the press and forward them to the Permanent Secretary within this interval, otherwise only the abstracts or titles will appear in the printed volumes. The Standing Committee shall have power to print an abstract only of any paper. Whenever practicable, proofs shall be forwarded to authors for revision. If any additions or substantial alterations are made by the author of a paper after its submission to the Secretary, the same shall be distinctly indicated. Illustrations must be provided for by the authors of the papers, or by a special appropriation from the Standing Committee. Immediately on publication of the volume, a copy shall be forwarded to every member and fellow of the Association who shall have paid the assessment for the meeting to which it relates, and it shall also be offered for sale by the Permanent Secretary at such price as may be determined by the Standing Committee. The Standing Committee shall also designate the institutions to which copies shall be distributed.

Local Committee.

Art. 31. The Local Committee shall consist of persons interested in the objects of the Association and residing at or near the place of the proposed meeting. It is expected that the Local Committee, assisted by the officers of the Association, will make all essential arrangements for the meeting, and issue a circular giving necessary particulars, at least one month before the meeting.

Library of the Association.

Art. 32. All books and pamphlets received by the Association shall be in the charge of the Permanent Secretary, who shall have a list of the same printed and shall furnish a copy to any member or fellow on application. Members and fellows who have paid their assessments in full shall be allowed to call for books and pamphlets, which shall be delivered to them at their expense, on their giving a receipt agreeing to make good any loss or damage and to return the same free of expense to the Secretary at the time specified in the receipt given. All books and pamphlets in circulation must be returned at each meeting, or the value of any not so returned paid to the Permanent Secretary. Not more than ten books, including volumes, parts of volumes, and pamphlets, shall be held at one time by any member or fellow. Any book may be withheld from circulation by order of the Standing Committee.

Admission Fee and Assessments.

Art. 33. The admission fee for members shall be five dollars in addition to the annual assessment. On the election of any member as a fellow an additional fee of two dollars shall be paid.

Art. 34. The annual assessment for members and fellows shall be three dollars.

Art. 35. Any member or fellow who shall pay the sum of fifty dollars to the Association, at any one time, shall be exempt from all further assessments, but this payment shall not entitle him to the publications of the Association, and all money thus received shall be invested as a permanent fund, the income of which shall be used only to assist in original research.

Art. 36. All admission fees and assessments must be paid to the Permanent Secretary, who shall give proper receipts for the same.

Accounts.

Art. 37. The accounts of the Permanent Secretary and of the Treasurer shall be audited annually, by two auditors appointed by the Standing Committee.

Alterations of the Constitution.

Art. 38. No part of this Constitution shall be amended or annulled, without the concurrence of three-fourths of the members and fellows present in general session, after notice given at a general session of a preceding meeting of the Association.

ORDER OF PROCEEDINGS

IN

ORGANIZING A MEETING.

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1. The retiring President introduces the President elect, who takes the chair.
2. Formalities of welcome of the Association as may be arranged by the Local Committee.
3. Report of the list of papers on the register and their reference to the Sections.
4. Other reports.
5. Announcements of arrangements by the Local Committee.
6. Elections to complete the Standing Committee.
7. Election of members.
8. Election of fellows.
9. Unenumerated business.
10. Adjournment to meet in Sections.

This order, so far as applicable, to be followed in subsequent General Sessions.

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.[1]

PATRON.[2]

Thompson, Mrs. Elizabeth, Stamford, Conn. (22). (Entitled to annual volume.)

LIFE MEMBERS.[3]

Case, Leonard, Cleveland, Ohio (15).
Elwyn, Alfred L., Philadelphia, Pa. (1).
Lyman, Benj. Smith, care Smith, Archer & Co., Yokohama, Japan (15).
Robertson, Thomas D., Rockford, Ill. (10).
Stephens, W. Hudson, Lowville, N. Y. (18). (Entitled to annual volume.)
Vaux, William S., 1702 Arch St., Philadelphia, Pa. (1). (Entitled to annual volume.)
Warner, James D., 199 Baltic St., Brooklyn, N. Y. (18).

MEMBERS.

Abbe, George W., 32 East 20th St., New York (23).
Abbot, Miss Elizabeth O., 16 Clark St., Cincinnati, Ohio (20).
Acheson, Edward G., Lock Box 82, Pittsburgh, Pa. (25).
Adcock, Prof. Robert J., Monmouth, Warren Co., Ill. (21).
Aikin, Prof. W. E. A., Baltimore, Md. (12).
Ainsworth, Frank B., Sup't Ind. House of Refuge, Plainfield, Ind. (20).

[1] The numbers in parentheses indicate the meeting at which the member was elected. The Constitution requires that the names of all members two or more years in arrears shall be omitted from the list, but their names will be restored on payment of arrearages. Members not in arrears are entitled to the annual volume of Proceedings.

[2] Persons contributing one thousand dollars or more to the Association are classed as Patrons, are exempt from the annual assessments and are entitled to the annual volume.

[3] Any Member or Fellow may become a Life Member by the payment of fifty dollars. The money derived from Life Memberships is invested as a fund, the income of which is to be used only to aid in original research. Life Members are exempt from the annual assessment, and an additional payment of ten dollars entitles a Life Member to the annual volume.

Albert, Augustus J., Baltimore, Md. (12).
Alexander, Prof. Stephen, Princeton, N. J. (1).
Allen, B. Rowland, Insurance Agent, Hartford, Conn. (23).
Allen, H. R., Indianapolis, Ind. (24).
Allen, J. M., Hartford, Conn. (22).
Allen, Stephen M., 28 State St., Boston, Mass. (23).
Allyn, Mrs. Clarence (22).
Andrews, Dr. Edmund, 6 Sixteenth St., Chicago, Ill. (22).
Armsby, Henry P., Box 273, New Brunswick, N. J. (23).
Armstrong, Rev. John W., D.D., State Normal School, Fredonia, N. Y. (24).
Atwater, Mrs. Samuel T., 166 Washington St., Chicago, Ill. (17).
Austin, Mrs. E. P., Cambridge, Mass. (24).

Babbitt, Henry S., M.D., Columbus, Ohio (23).
Babcock, Prof. Samuel S., Mt. Clemens, Macomb Co., Mich. (24).
Bailey, E. H. S., Bethlehem, Pa. (25).
Baird, Lyman, 90 La Salle St., Chicago, Ill. (17).
Baker, Prof. Arthur L., Lafayette College, Easton, Pa. (24).
Baker, Prof. T. R., Millersville, Lancaster Co., Pa. (22).
Balch, David M., Salem, Mass. (22).
Bandelier, Ad. F., Highland, Ill. (25)
Barber, Edwin A., Westchester, Chester Co., Pa. (25).
Barnard, Jas. M., Boston, Mass. (18).
Barnard, Wm. Stebbins, Canton, Ill. (24).
Barry, Sir Redmond, President Public Library of Melbourne, Victoria, Australia (25).
Bassett, George W., M.D., Vandalia, Ill. (20).
Bassnett, Thomas, Jacksonville, Fla. (8).
Bassnett, Mrs. Thomas, Jacksonville, Fla. (24).
Bastian, Dr. David I., Clinton, Worcester Co., Mass. (25).
Batterson, J. G., Hartford, Conn. (23).
Beach, Charles M., Merchant, Hartford, Conn. (23).
Beach, J. Watson, Merchant, Hartford, Conn. (23).
Beach, William H., Beloit, Wis. (21).
Beal, Prof. Wm. James, Agricultural College, Lansing, Mich. (24).
Becker, Dr. Alexander R., 265 Benefit St., Providence, R. I. (22).
Becker, Hon. Philip, Buffalo, N. Y. (25).
Bell, James D., Office of Daily Graphic, New York (20).
Bell, John J., Exeter, N. H. (22).
Bemis, Hon. A. S., President of the Common Council, Buffalo, N. Y. (25).
Benjamin, E. B., 10 Barclay St., New York (19).
Bessey, Prof. C. E., Agricultural College, Ames, Iowa (21).
Bickmore, Prof. Albert S., Am. Mus. of Nat. Hist., Central Park, N. Y. (17).
Bill, Charles, Springfield, Mass. (17).
Blackham, Dr. George E., Dunkirk, N. Y. (25).
Blake, Clarence J., M.D., Hotel Berkeley, Boston, Mass. (24).

Blake, W. P., Geologist, New Haven, Conn. (2).
Blakeslee, B. F., Stock Broker, Hartford, Conn. (23).
Blatchford, Eliphalet W., 375 North La Salle St., Chicago, Ill. (17).
Bliss, Porter C., Journalist (18).
Blois, John T., Jonesville, Hillsdale Co., Mich. (24).
Boadle, John, Haddonfield, N. J. (20).
Bohrer, Charles J., Lafayette, Ind. (25).
Bolles, Rev. E. C., Salem, Mass. (17).
Bowen, Edmund S., Gen'l Supt. Erie Railway, N. Y. (25).
Bowen, Lemuel W., care D. M. Ferry & Co., 203 Woodward Ave., Detroit, Mich. (24).
Boynton, Miss Susan P., Box 150, Lynn, Mass. (19).
Breneman, A. A., Lancaster, Pa. (20).
Brevoort, J. Carson, Brooklyn, N. Y. (1).
Briggs, Albert D., 7 Fort Block, Springfield, Mass. (13).
Brigham, Rev. Charles H., Ann Arbor, Mich. (17).
Bristol, Eugene S., New Haven, Conn. (17).
Broomall, Hon. John M., Media, Delaware Co., Pa. (23).
Brown, Mrs. Ashley, 115 Wilkinson St., Dayton, Ohio (25).
Brush, Dr. Edward N., Buffalo N. Y. (25).
Bryan, Oliver N., Accokeek P. O., Prince George's Co., Md. (18).
Buckhout, W. A., State College, Centre Co., Pa. (20).
Bull, Dr. Alexander T., 98 Swan St., Buffalo, N. Y. (25).
Burbank, L. S., Woburn, Mass. (18).
Burgess, Edward, Sec'y Nat. Hist. Society, Boston, Mass. (22).
Burnham, S. W., 52 Vincennes Avenue, Chicago, Ill. (25).
Bush, Stephen, Waterford, N. Y. (19).

Cadman, Charles C., Merchants' & Manufacturers' Bank, Detroit, Mich. (24).
Campbell, Mrs. Mary H., Crawfordsville, Ind. (22).
Canby, William M., 1101 Delaware Ave., Wilmington, Del. (17).
Canfield, Rev. Eli H., D.D., Arlington, Bennington Co., Vt. (25).
Capen, Miss Bessie T., Smith College, Northampton, Mass. (23).
Carpenter, Prof. G. C., Simpson Centenary College, Indianola, Iowa (22).
Carpenter, Lieut. W. L., Dunkirk, N. Y. (24).
Carr, Lucien, Cambridge, Mass. (25).
Carrington, Col. Henry B., U.S.A., Prof. in Wabash College, Crawfordsville, Ind. (20).
Case, L. B., Richmond, Indiana (17).
Cassino, S. E., Naturalists' Agency, Salem, Mass. (25).
Cattell, William C., President Lafayette College, Easton, Pa. (15).
Chamberlin, T. C., Beloit College, Beloit, Wis. (21).
Chandler, Henry, Lewis Block, Buffalo, N. Y. (25).
Chanute, O., care Erie Railway, New York (17).
Chapman, F. M., Danforth, Iroquois Co., Ill. (17).

Chase, Prof. Pliny E., Haverford College, Haverford, Pa. (18).
Chase, R. Stuart, 16 Merrimack St., Haverhill, Mass. (18).
Chase, Theodore R., Detroit, Mich. (24).
Chesbrough, E. S., Chicago, Ill. (2).
Chickering, Prof. J. W., jr., Deaf Mute College, Washington, D. C. (22).
Choate, Prof. Isaac B., Buchtel College, Akron, Ohio (25).
Clark, Simeon T., M.D., Lockport, Niagara Co., N. Y. (25).
Clinton, Hon. George W., Buffalo, N. Y. (25).
Coe, Miss Emily M., 44 East 43d St., N. Y. (25).
Coffinberry, W. L., Grand Rapids, Mich. (20).
Cogswell, Dr. George, Bradford, Mass. (18).
Collin, Prof. Alonzo, Cornell College, Mount Vernon, Iowa (21).
Colman, Henry, M.D., 1483 Washington St , Boston, Mass. (25).
Colton, G. Woolworth, 172 William St., New York (22).
Comstock, Theodore B., Ass't Prof. Geol. Dep't, Cornell University, Ithaca, N. Y. (24).
Connerly, Prof. D. C. B., Principal Huntsville High School, Huntsville, Ala. (23).
Cook, Prof. A. J., Agricultural College, Lansing, Mich. (24).
Cook, Dr. Charles D., 133 Pacific St., Brooklyn, N. Y. (25).
Cooke, Caleb, Peabody Academy of Science, Salem, Mass. (18).
Cooley, Prof. Le Roy C., Vassar College, Poughkeepsie, N. Y. (19).
Copes, Dr. Joseph S., care Copes & Ogden, New Orleans, La. (11).
Corwin, Richard W., Michigan University, Ann Arbor, Mich. (24).
Coxe, Eckley B., Drifton, Jeddo P. O., Luzerne Co., Pa. (23).
Crawford, Dr. John S., Galena, Ill. (21).
Crehore, John D., 458 Prospect St., Cleveland, Ohio (24).
Crosby, M. S., Pres't Waterbury Scientific Soc., Waterbury, Conn. (23)
Cross, Japheth, Adrian, Mich. (24).
Cummings, John, Woburn, Mass. (18).
Curtis, Rev. W. S., D.D., Rockford, Ill. (21).
Cusick, Cornelius C., Lieut. 22nd Reg. Infantry, U.S.A., Washington, D. C. (24).
Cutter, John D., 1208 Pacific St., Brooklyn, L. I. (23).

Davis, George T., Portland, Me. (23).
DeCamp, Dr. William H., Grand Rapids, Mich. (21).
Delano, Joseph C., New Bedford, Mass. (5).
Derby, O. A. (23).
Devereux, J. H., President C.C.C. & I.R.R., Cleveland, Ohio (18).
Dickinson, Rev. John, 167 Tompkins Avenue, Brooklyn, L. I. (23).
Dixwell, Epes S., Cambridge, Mass. (1).
Dodd, C. M., Williamstown, Mass. (19).
Doggett, Mrs. Kate N., Chicago, Ill. (17).
Doggett, Wm. E., Chicago, Ill. (17).

Doolittle, Prof. C. L., South Bethlehem, Pa. (25).
Dorr, Capt. E. P., 280 Niagara St., Buffalo, N. Y. (25).
Doughty, John W., Newburgh, N. Y. (19).
Douglas, Prof. Samuel T., Ann Arbor, Mich. (24).
Douglas, Prof. Silas H., Ann Arbor, Mich. (24).
Downer, Henry E., 103 Woodward Ave., Detroit, Mich. (21).
Drowne, Charles, care William L. Drowne, Esq., Canaan Four Corners, New York (6).
Drumm, Rev. Thomas, St. Mark's School, Rochester, N. Y. (25).
Dudley, Charles B., Altoona, Pa. (23).
Duschak, Adolf, 13 Chippewa St., Buffalo, N. Y. (25).
Dyer, Clarence, Lawrence, Mass. (22).
Dyer, Elisha, 26 Westminster St., Providence, R. I. (9).

Eaton, Prof. James H., Beloit College, Beloit, Wis. (17).
Eaton, Hon. John, U. S. Commiss'r of Education, Washington, D. C. (25).
Edgar, Col. George M., Principal of Edgar Institute, Paris, Ky. (20).
Edwards, A. Mead, M.D., 9 Gouveneur St., Newark, N. J. (18).
Elliott, Arthur H., Highlands Chem. and Mining Co., Highlands Station, Putnam Co., N. Y. (23).
Ellis, Wm. Hodgson, Trinity College, Toronto, Canada (25).
Emerson, Prof. Benjamin K., Amherst, Mass. (19).
Engelmann, Dr. Geo. J., 3003 Locust St., St. Louis, Mo. (25).
Ennis, Jacob, Principal Scientific Inst., Philadelphia, Pa. (19).
Everts, Miss M. M., care Rev. W. W. Everts, D.D., Chicago, Ill. (22).

Faries, R. J., Wauwatosa, Wis. (21).
Farnam, Charles H., New Haven, Conn. (25).
Farquharson, Dr. Robert James, Davenport, Iowa (24).
Fernald, Prof. Charles H., State Agricultural College, Orono, Me. (22).
Fernald, Prof. M. C., State Agricultural College, Orono, Me. (22).
Ficklin, Prof. Joseph, University of Missouri, Columbia, Mo. (20).
Fisher, Prof. Davenport, 642 Marshall St., Milwaukee, Wis. (17).
Fletcher, Ingram, care Fletcher & Sharpe, Indianapolis, Ind. (20).
Flint, D. B., Watertown, Mass. (25).
Fontaine, Prof. Wm. M., Univ. of W. Va., Morgantown, W. Va. (25).
Foreman, Henry L., U. S. Signal Service, Washington, D. C. (23).
Fosgate, Dr. Blanchard, Auburn, N. Y. (7).
Foster, Henry, M.D., Clifton, N. Y. (17).
Franklin, Gen. W. B., Vice Pres't Colt's Armory, Hartford, Conn. (23).
Frazer, Persifor, jr., Ass't Geologist 2nd Geol. Survey of Pa., 1517 Walnut St., Philadelphia, Pa. (24).
Frazier, Prof. B. W., The Lehigh University, Bethlehem, Pa. (24).
French, Dr. Geo. F., 666 Congress St., Portland, Me. (22).

Frothingham, Rev. Frederick, Milton, Mass. (11).
Fuller, A. S., Ridgewood, Bergen Co., N. J. (24).
Fuller, Charles B., 39 Preble St., Portland, Me. (22).
Fulton, Prof. Robert B., University of Mississippi, Oxford, Miss. (21).

Gage, Rev. William Leonard, Hartford, Conn. (23).
Garbett, Wm. A., 8 and 10 Guild Building, Boston Highlands, Mass. (22).
Gardner, James T , Am. Geog. Soc., 11, W. 29th St., N. Y. (25).
Garland, Landon C., Chancellor Vanderbilt Univ., Nashville, Tenn. (25).
Garnett, Algernon S., M.D., Hot Springs, Arkansas (23).
Garrett, Ellwood, Wilmington, Newcastle County, Del. (22).
Gaskill, Joshua, Lockport, N. Y. (22).
Gatling, Dr. R. J., Hartford, Conn. (23).
Gennadius, Panajotes, care J. Gennadius, 32 Bury St., St. James's, London, Eng. (23).
Gibbs, R. M. W., Kalamazoo, Mich. (24).
Gilbert, Dexter W., Sup't of Schools, Keene, N. H. (23).
Glenny, William H., jr., Buffalo, N. Y. (25).
Gold, Theodore S., West Cornwall, Conn. (4).
Goldschmidt, S. A., 142 West 21st St., New York (24).
Gooch, Frank A., Cambridge, Mass. (25).
Goodfellow, Edward, U. S. Coast Survey Office, Washington, D. C. (24).
Goold, W. N., Sec'y Portland Society Natural History, Portland, Me. (22).
Gould, Prof. B. A., care H. McMurtrie, P. O. Box 2414, Boston, Mass. (2).
Gould, Sylvester C., Manchester, N. H. (22).
Green, J. C., M.D., Buffalo, N. Y. (25).
Green, Jacob L., Sec. Mut. Life Ins. Co., Hartford, Conn. (23).
Green, Dr. Samuel E., 25th St., Pittsburgh, Pa. (22).
Greene, David M., Dep't S. E. & S., Albany, N. Y. (19).
Gregory, Prof. J. J. H., Marblehead, Mass. (18).
Gridley, Albert L., Benzonia, Mich. (25).
Grinnell, George Bird, Yale College Museum, New Haven, Conn. (25).
Grossklaus, John F., Navarre, Ohio (24).
Gundlach, Ernest, Rochester, N. Y. (25).
Gunning, William D., Waltham, Mass. (22).

Hall, Prof. Asaph, U. S. Naval Observatory, Washington, D. C. (25).
Hall, George E., Cleveland, Ohio (19).
Hall, Israel, Ann Arbor, Mich. (24).
Hamel, Prof. Thomas E., Laval University, Quebec, Canada (18).
Hanaman, C. E., Troy, N. Y. (19).
Harger, Oscar, Yale College Museum, New Haven, Conn. (25).
Hart, Rev. Prof. Samuel, Trinity College, Hartford, Conn. (22).
Harvey, Charles W., Sup't Public Schools, Greensburg, Ind. (20).
Harvey, Dr. Leon F., Buffalo, N. Y. (22).

Hastings, Charles S., 44 McMechen St., Baltimore, Md. (25).
Hatfield, Marcus P., M.D., Chicago Medical College, Chicago, Ill. (23).
Hayes, George E., Buffalo, N. Y. (15).
Hazen, E. H., M.D., Davenport, Iowa (25).
Henderson, John G., Winchester, Ill. (25).
Henshaw, Henry W., Office U. S. Engineers, Lock Box 93, Washington, D. C. (24).
Herdman, W. J., Ann Arbor, Mich. (24).
Herrick, Mrs. Sophie B., Baltimore, Md. (23).
Hervey, Rev. A. B., 10 North 2d St., Troy, N. Y. (22).
Hicks, John D., Old Westbury, Queen's Co., L. I. (23).
Higgins, F. W., Detroit, Mich. (24).
Hills, S. W., Marshall, Mich. (6).
Hoadley, Frederick H., Ass't in Yale Coll. Mus., New Haven, Conn. (23).
Holley, George W., Niagara Falls, N. Y. (19).
Holmes, John C., Moffat Block, Detroit, Mich. (24).
Holmes, Rev. Thomas, D.D., Ann Arbor, Mich. (20).
Homes, Henry A., Librarian State Library, Albany, N. Y. (11).
Hopkirk, Prof. W. H., Burlington, Iowa (24).
Hornaday, William T., Rochester, N. Y. (25).
Horr, Asa, M.D., Dubuque, Iowa (21).
Horrobin, William T., Cohoes, N. Y. (19).
House, John C., Union Gas Works, Waterford, N. Y. (19).
Hovey, Prof. Edmund O , Wabash College, Crawfordsville, Ind. (20).
Hovey, Miss Mary F., Crawfordsville, Ind. (20).
Howard, Miss Catharine L., Springfield, Mass. (25).
Howe, Dr. Lucien, 9 Niagara St., Buffalo N. Y. (25).
Howell, Edwin E., Rochester, N. Y. (25).
Hubbard, H. G., Detroit, Mich. (24).
Hubbard, Prof. Oliver P., 65 W. 19th Street, New York (1).
Hubbard, Mrs. Sara A., 31 Thirty-third St., Chicago, Ill. (17).
Humphrey, D., M.D., Lawrence, Mass. (18).
Humphreys, A. W., Box 1384, New York (20).
Hunt, Miss Sarah E., Salem, Mass. (20).
Hyatt, Jonathan D., Morrisiana Station, New York (19).
Hyde, E. W., University of Cincinnati, Cincinnati, O. (25).

Irish, Thomas M., 58 Prairie St., Dubuque, Iowa (21).
Isom, John F., Cleveland, Ohio (24).

Jackson, Prof. C. L., care P. T. Jackson, Boston, Mass. (20).
James, Frederick H., M.D., Lancaster, N. Y. (25).
James, Thomas Potts, Cambridge, Mass. (22).
Jarvis, George C., M.D., Hartford, Conn. (23).
Jasper, Gustavus A., 12 Central St., Boston, Mass. (18).

Jewell, Pliny, Mineralogical Amateur, Hartford, Conn. (23).
Jewell, Lt. Comdr. Theo. F., U.S.N., Annapolis, Md. (25).
Jillson, Dr. B. C., Pittsburg, Pa. (14).
Johnson, Prof. Hosmer A., Academy of Sciences, Chicago, Ill. (22).
Johnson, Samuel M. (23).
Johnston, J. Frank, Bethlehem, Pa. (25).
Jones, Prof. Richard W., University of Mississippi, Oxford, Miss. (25).
Jones, Rev. Prof. William, Trinity College, Toronto, Canada (23).
Jordan, Prof. David S., Irvington, Ind. (23).
Joy, Prof. Charles A., Columbia College, New York (8).
Joyce, Rev. J. Jay, jr., 33 North 17th St., Philadelphia, Pa. (22).

Keely, Prof. G. W., Waterville, Me. (1).
Kellogg, John H., M.D., Battle Creek, Mich. (24).
Kinder, Miss Sarah, 27 Lockerbie St., Indianapolis, Ind. (20).
King, Ephraim L., M.D., Box 251, Ashtabula, Ohio (24).
King, Mrs. Mary B. A., 21 Madison St., Rochester, N. Y. (15).
Kinner, Dr. Hugo, 1517 South Seventh St., St. Louis, Mo. (21).
Kirkpatrick, James A., 19 South Fifth St., Philadelphia, Pa. (7).
Kite, Thomas, Gen'l Agent Washington Life Ins. Co. of N. Y., 122 Vine St., Cincinnati, O. (5).
Klippart, John H., Cor. Sec'y State Board of Agriculture, Box 1453, Columbus, Ohio (17).
Knapp, Frederick N., Plymouth, Mass. (19).
Knapp, Herman, M.D., 25 West Twenty-fourth St., New York (22).
Kneeland, Dr. Samuel, Mass. Institute Technology, Boston, Mass. (20).
Knight, J. B., care Franklin Institute, Philadelphia, Pa. (21).

Land, William J., P. O. Box 305, Atlanta, Ga. (25).
Lawrence, Hon. Edw., Pres't Bunker Hill National Bank, Charlestown, Mass. (18).
Lawrence, George N., 172 Pearl, cor. Pine St., New York (7).
Leakin, Rev. George A., Baltimore, Md. (17).
Leland, Eugene R., Eau Claire, Wis. (25).
Lesley, Joseph, jr., 233 South Fourth St., Philadelphia, Pa. (8).
Letchworth, Josiah, Buffalo, N. Y. (25).
Lewis, Elias, jr., 111 St. Mark's Ave., Brooklyn, N. Y. (23).
Lewis, George A., 10 Lewis Block, Buffalo, N. Y. (25).
Lewis, James, Mohawk, N. Y. (25).
Loudon, Prof. James, Toronto, Canada (25).
Love, Edward G., School of Mines, Columbia College, New York (24).
Lovewell, Prof. Joseph T., State College, Centre Co., Pa. (25).
Lunn, Miss Emma, Montreal, Canada (24).
Lyford, Prof. Moses, Waterville, Me. (22).

MacArthur, Charles L., Troy, N. Y. (19).
Mack, William, M.D., Salem, Mass. (21).
Macloskie, Rev. George, College of New Jersey, Princeton, N. J. (25).
Mansfield, J. M., Mt. Pleasant, Iowa (25).
Marden, George H., 7 Parker St., Charlestown, Mass. (18).
Mark, Edward L., Fredonia, N. Y. (21).
Martin, Prof. B. N., 236 W. 4th St., New York (23).
Martin, Prof. Daniel S., Rutger's Female College, New York (23).
Mason, Otis T., Columbian University, Washington, D. C. (25).
Mathews, Robert, Rochester, N. Y. (25).
Maxwell, J. M. (24).
Maycock, Prof. M. M., Buffalo, N. Y. (25).
McCollister, Rev. S. H., Pres't Buchtel College, Akron, Ohio (22).
McCutchen, Aug. R., Dalton, Ga. (25).
McMurtrie, Horace, P. O. Box 2414, Boston, Mass. (17).
McRae, John, Camden, S. C. (3).
McWhorter, Tyler, Aledo, Ill. (20).
Means, Rev. A., Oxford, Ga. (5).
Mees, Carl Leo, Columbus, Ohio (24).
Metcalf, Caleb B., Highland Military Academy, Worcester, Mass. (20).
Milburn, John G., Buffalo, N. Y. (25).
Miller, John A., Box 110, Paducah, Ky. (22).
Minifie, William, 114 Baltimore St., Baltimore, Md. (12).
Moffatt, Dr. George T., Boston, Mass. (25).
Moody, Mrs. Mary B., 187 N. Pearl St., Buffalo, N. Y. (25).
Moore, Edward, M., M.D., Rochester, N. Y. (25).
Moore, Joseph, Pres't Earlham College, Richmond, Ind. (20).
Morehouse, Geo. W., Wayland, Steuben Co., N. Y. (24).
Morison, Dr. N. H., Provost of Peabody Institute, Baltimore, Md. (17).
Moses, Prof. Thos. F., Urbana University, Urbana, Ohio (25).
Mudge, Benjamin F., Manhattan, Riley Co., Kansas (25).
Muir, John, 1419 Taylor St., San Francisco, Cal. (22).
Mulford, S. P., Marysville, Ohio (24).
Munroe, William, Concord, Mass. (18).
Murray, Charles H., Louisville, Clay Co., Ill. (24).

Nason, Almond F., 15 State St., Boston, Mass. (22).
Nelson, Prof. Edward T., Delaware, Delaware Co., Ohio (24).
Newcomb, Mrs. Lydia B. Black, Teacher, New Haven, Conn. (23).
Newman, John S., 48 East Washington St., Indianapolis, Ind. (20).
Newman, Mrs. John S., 48 East Washington St., Indianapolis, Ind. (21).
Newton, Rev. John, Mary Esther, West Fla. (7).
Nichols, Charles A., Providence, R. I. (17).
Nicholson, Thomas, M.D., 490 Magazine St., New Orleans, La. (21).

Northrop, Birdsey Grant, Secretary Connecticut State Board of Education, New Haven, Conn. (23).
Norton, Miss Mary E. B., Rockford Seminary, Rockford, Ill. (21).
Noyes, Henry D., M.D., 73 Madison Avenue, New York (23).
Noyes, Dr. James F., 101 Shelby St., Detroit, Mich. (24).

Ogden, Mahlon D., Chicago, Ill. (17).
Ogden, Robert W., 44 Carondelet St., New Orleans, La. (21).
Ogden, W. B., High Bridge, Westchester County, N. Y. (17).
Olmsted, Fred. Law, Commissioner of Public Parks, 209 W. 46th St., New York (22).
Ordway, Prof. John M., Mass. Inst. Technology, Boston, Mass. (9).
Osborne, Amos O., Waterville, Oneida Co., N. Y. (19).

Paine, Charles, 163 Prospect St., Cleveland, Ohio (22).
Palmer, Rev. Benj. M., Box 1762, New Orleans, La. (21).
Palmer, Dr. Edward, care Peabody Museum, Cambridge, Mass (22).
Palmer, Rev. James M. (22).
Parker, Wilbur F., Editor Rod and Gun, West Meriden, Conn. (23).
Parvin, Theodore S., Iowa City, Iowa (7).
Patterson, John P., Supt. Instruction, Washington C. H., Ohio (25).
Paul, Mrs. Caroline A., Vineland, Cumberland Co., N. J. (23).
Peck, W. A., care Peck and Hillman, Troy, N. Y. (19).
Peet, Rev. Stephen D., Ashtabula, Ohio (24).
Peirce, Prof. Benjamin, Cambridge, Mass. (1).
Percival, Rev. Chester S., Rockford, Ill. (21).
Perkins, Prof. George H., Burlington, Vt. (17).
Perkins, S. E., jr., Indianapolis, Ind. (20).
Perkins, T. Lyman, Salem, Mass. (22).
Phelps, Gen. Charles E., Baltimore, Md. (13).
Phelps, Mrs. Almira Lincoln, Baltimore, Md. (13).
Philbrick, Prof. Philetus H., Iowa City, Iowa (23).
Phillips, A. W., Cheshire, Conn. (24).
Phillips, Prof. Charles, Univ. of North Carolina, Chapel Hill, N. C. (25).
Phin, John, Ed. Am. Journ. Microscopy, 176 Broadway, N. Y. (25).
Pickett, Dr. Thos. E., Maysville, Mason Co., Ky. (25).
Pierce, Henry D., Indianapolis, Ind. (20).
Pillsbury, John H., Teacher of Natural Sciences, Wesleyan University, Middletown, Conn. (23).
Pitt, Prof. William H., Buffalo, N. Y. (25).
Porteous, John, Agent Grand Trunk Railway, Portland, Me. (22).
Potter, Prof. William B., Washington University, St. Louis, Mo. (25).
Pratt, William H., Davenport, Iowa (17).
Preston, Prof. W. C., Laboratory, Iowa State Univ., Iowa City, Iowa (21).

Prince, Gen. Henry, U.S.A., Headquarters of the Army, San Francisco, Cal. (22).
Pruyn, John V. L., Chancellor Univ. of N.Y., 13 Elk St., Albany, N.Y. (1).
Pulsifer, Sidney, Peoria, Ill. (21).
Putnam, Mrs. F. W., Cambridge, Mass. (19).
Putnam, George P., Buffalo, N. Y. (25).

Ramsdell, Hon. J. G., Traverse City, Mich. (24).
Rau, Dr. Charles, Smithsonian Institution, Washington, D. C. (24).
Read, Ezra, Terre Haute, Ind. (20).
Richards, Mrs. Robert H., Mass. Inst. of Technology, Boston, Mass. (23).
Ritchie, E. S., 150 Tremont St., Boston, Mass. (10).
Robertson, Col. Robert S., Fort Wayne, Ind. (20).
Robinson, Henry C., Lawyer, Hartford, Conn. (23).
Robinson, Prof. Otis Hall, Rochester, N. Y. (23).
Rockwell, Alfred P., Eastern R. R., Boston, Mass. (10).
Rogers, A. J., Milwaukee, Wis. (25).
Rogers, Hon. Sherman S., Buffalo, N. Y. (25).
Rominger, Carl, M.D., Ann Arbor, Mich. (21).
Root, Prof. Elihu, Amherst College, Amherst, Mass. (25).
Rose, Prof. P. B., Ann Arbor, Mich. (23).
Ross, Angus, Morris Street School, Dartmouth, Halifax Co., N. S. (22).
Rowland, Prof. Henry A. (23).
Rumsey, Bronson C., 58 and 60 Exchange St., Buffalo, N. Y. (15).
Russell, Gurdon W., M.D., Hartford, Conn. (23).
Russell, I. C., School of Mines, N. Y. (25).
Russell, L. W., Providence, R. I. (20).
Russell, W. H. H., 322 Chestnut St., cor. 4th, St. Louis, Mo. (24).

Sage, John H., Ornithologist, Portland, Conn. (23).
Sampson, Com'd W. T., U.S.N., Naval Academy, Annapolis, Md. (25).
Sanders, Benjamin D., Wellsburg, Brooke County, W. Va. (19).
Scammon, J. Young, Chicago, Ill. (17).
Schanck, Prof. J. Stillwell, Princeton, New Jersey (4).
Schmelter, Henry, P. O. Box 2135, N. Y. (25).
Schmid, Dr. H. Ernest, White Plains, N. Y. (25).
Schweitzer, Prof. Paul, State University of Mo., Columbia, Mo. (24).
Seaman, Ezra C., Ann Arbor, Mich. (20).
Seeley, Charles A., 26 Pine St., New York (18).
Seymour, Prof. William P., 105 Third St., Troy, N. Y. (19).
Sheafer, P. W., Pottsville, Pa. (4).
Sheafer, Walter S, Pottsville, Pa. (25).
Sheldon, Edwin H., Chicago, Ill. (17).
Sill, J. M. B., Zoölogist, Detroit, Mich. (23).

Simonds, Frederic W., Cornell University, Ithaca, N. Y. (25).
Skinner, Joseph J., New Haven, Conn. (23).
Sloan, Dr. John, New Albany, Ind. (20).
Smith, Prof. Eugene A., University of Alabama, Tuscaloosa, Ala. (20).
Smith, Henry T., Buffalo, N. Y. (25).
Smith, J. W., M.D., Charles City, Iowa (21).
Smith, Mrs. S. H., Jersey City, N. J. (25).
Smith, W. H., University of N. C., Chapel Hill, N. C. (24).
Smock, Prof. John Conover, Rutger's Coll., New Brunswick, N. J. (23).
Snyder, Prof. Monroe B. (24).
Southworth, Mase S., Ph.D., Williamstown, Mass. (25).
Sprague, E. C., Buffalo, N. Y. (24).
Springer, Alfred, Cincinnati, Ohio (24).
Stearns, Winfrid Alden, Amherst, Mass. (23).
Sternberg, George M., Surgeon U.S.A., Post Surgeon, Fort Walla Walla, W. T. (24).
Stevens, R. P., Jamaica, N. Y. (18).
Stevens, Thaddeus M., M.D., Indianapolis, Ind. (20).
Steward, A., 155 State St., Chicago, Ill. (21).
Stewart, Elliott W., Lake View, Erie Co., N. Y. (25).
Stimpson, Thomas M., Peabody, Mass. (18).
Stone, Miss Mary H., Salem, Mass. (25).
Stone, Ormond, Director Cincinnati Observatory, Univ. of Cincinnati, Mt. Lookout, Ohio (24).
Storer, Dr. D. H., Boston, Mass. (1).
Storer, Prof. Frank H., Jamaica Plain, Mass. (13).
Stowell, John, 48 Main Street, Charlestown, Mass. (21).
Straight, H. H., Oswego, N. Y. (25).
Strong, Prof. Edwin A., Grand Rapids, Mich. (24).
Stubbs, Prof. Wm. C., Alabama Agricultural and Mechanical College, Auburn, Lee Co., Ala. (25).
Sutton, Dr. George, Aurora, Dearborn Co., Ind. (20).
Swain, James, Fort Dodge, Iowa (21).
Swain, Mrs. James, Fort Dodge, Iowa (21).
Swasey, Oscar F., M.D., Beverly, Mass. (17).

Taylor, Wm. Curtis, 914 Chestnut St., Philadelphia, Pa. (24).
Tenney, Prof. Sanborn, Williamstown, Mass. (17).
Thompson, Harvey M., Brevoort House, Chicago, Ill. (17).
Thompson, Joseph P., Portland, Me. (22).
Thompson, Robert H., 24 First St., Troy, N. Y. (19).
Thomson, Prof. A., Ames, Iowa (21).
Thurber, Miss Elizabeth, Plymouth, Mass. (22).
Tice, John H., St. Louis, Mo. (24).
Tittmann, Otto H., U. S. Coast Survey, 214 Stockton St., San Francisco, Cal. (24).

Todd, Prof. James E., Tabor, Fremont Co., Iowa (22).
Tomlinson, Dr. J. M., 28 East Ohio St., Indianapolis, Ind. (20).
Townsend, Franklin, Adj. Gen. State of N. Y., Albany, N. Y. (4).
Townshend, Prof. N. S., Columbus, Ohio (17).
Trembley, J. B., M.D., Oakland, Alameda Co., Cal. (17).
Trowbridge, Mrs. L. H., 211 Jefferson Ave., Detroit, Mich. (21).
Turner, Dr. Robert S., Box 712½, Minneapolis, Minn. (18).
Twiss, George H., Columbus, Ohio (24).

Upham, Dr. J. Baxter, 31 Chestnut St., Boston, Mass. (14).
Upham, Warren, Nashua, N. H. (25).

Vail, Prof. Hugh D., 1927 Mt. Vernon St., Philadelphia, Pa. (18).
Vasey, George, Department of Agriculture, Washington, D. C. (20).

Wakeman, Thaddeus B., No. 93 Nassau St., N. Y. (25).
Walcott, Charles D., Trenton Falls, N. Y. (25).
Walker, George C., No. 13 Chamber of Commerce, Chicago, Ill. (17).
Walker, N. B., 19 and 21 Cliff St., New York (20).
Wanzer, Ira, Brookfield, Conn. (18).
Warder, Robert B., Cleves, Hamilton Co., Ohio (19).
Wardwell, George J., Rutland, Vt. (20).
Warner, Mrs. J. D., 199 Baltic St., Brooklyn, N. Y. (21).
Warren, G. Washington, 54 Devonshire St., Room 1, Boston, Mass. (18).
Warring, Charles B., Croton Landing, N. Y. (25).
Wead, Prof. Charles K., Malone, N. Y. (23).
Webb, Benjamin, jr., Salem, Mass. (18).
Webster, Prof. H. E., Union College, Schenectady, N. Y. (23).
Welch, Mrs. G. O., Lynn, Mass. (21).
Weld, W. G., Boston, Mass. (23).
Wells, George A., Troy, N. Y. (19).
Wells, Samuel, 31 Pemberton Square, Boston, Mass. (24).
Wendell, Emory, First National Bank, Detroit, Mich. (24).
West, Charles E., 138 Montague St., Brooklyn, N. Y. (1).
Wheeler, C. G., 240 Calumet St., Chicago, Ill. (18).
Wheeler, Lt. Geo. M., U.S.E., Lock Box 93, Washington, D. C. (25).
Wheeler, Orlando B., Ass't U. S. Lake Survey, Detroit, Mich. (24).
Wheeler, Dr. T. B., 714 Craig St., Montreal, Canada (11).
White, Lieut. Com'd H. C., U. S. Navy, care W. I. Harris, Continental National Bank, New York (23).
White, John, Buffalo, N. Y. (25).
White, LeRoy S., Waterbury, Conn. (23).
White, Samuel S., Publisher Dental Cosmos, Chestnut St., cor. Twelfth, Philadelphia, Pa. (23).

Whitney, Mary W., Waltham, Mass. (19).
Willet, Prof. Joseph E., Mercer University, Macon, Ga. (25).
Williams, Charles H., Buffalo, N. Y. (25).
Williams, E. B., Strawberry Point, Iowa (24).
Williams, Edward H., jr., Drifton, Jeddo P. O., Pa. (25).
Williams, Frank, Buffalo, N. Y. (25).
Williams, H. S., Williams Brothers, Phenix Iron Works, Ithaca, N. Y. (18).
Williams, J. G., 224 South 11th St., Philadelphia, Pa. (19).
Williams, John T., Rafferty and Williams, Foot 44th St., East R., N. Y. (25).
Williams, P. O., M.D., Watertown, Jefferson Co., N. Y. (24).
Williamson, Lieut. Col. R. S., U. S. Engineers, Light House Engineer, San Francisco, Cal. (12).
Wilson, Walter T., Buffalo, N. Y. (25).
Worster, Joseph, M.D., 115 East Thirtieth St., New York (22).
Wright, Prof. Albert A., Oberlin College, Oberlin, Ohio (24).
Wright, Rev. William J., Wilson College, Chambersburg, Pa. (25).
Wurtz, Henry, 12 Hudson Terrace, Hoboken, N. J. (10).

Young, William H., Bookseller, 8 and 9 First St., Troy, N. Y. (19).

[522 MEMBERS.]

NOTE.—The omission of an address in the foregoing list, indicates that letters directed to that last printed were returned as uncalled for. Information of the present address of the members so indicated is requested by the PERMANENT SECRETARY.

FELLOWS.

Abbe, Prof. Cleveland, Washington, D. C. (16).
Agassiz, Alexander, Curator Mus. Comp. Zoology, Cambridge, Mass. (18).
Alexander, John S., 1935 Arch St., Philadelphia, Pa. (20).
Allen, Joel A., Museum Comparative Zoology, Cambridge, Mass. (18).
Allen, Zachariah, Providence, R. I. (1).
Alvord, Benj., Paymaster General, U. S. Army, Washington, D. C. (17).
Andrews, Prof. E. B., Lancaster, Ohio (7).
Appleton, Prof. John H., Brown University, Providence, R. I. (22).
Atkinson, Prof. Wm. H., 41 East 9th St., New York (22).
Austin, E. P., Box 434, Cambridge, Mass. (18).

Bailey, Prof. Loring W., University of Fredericton, N. B. (18).
Bailey, W. W., 31 Market Square, Providence, R. I. (18).
Baird, Prof. S. F., Smithsonian Institution, Washington, D. C. (1).
Bardwell, Prof. F. W., University of Kansas, Lawrence, Kan. (13).
Barker, Prof. G. F., University of Pennsylvania, Philadelphia, Pa. (13).
Barnard, F. A. P., President Columbia College, New York (7).
Barnard, Gen. J. G., U. S. Army, Army Building, New York (14).
Bassett, Homer F., Waterbury, Conn. (23).
Batchelder, Dr. J. H., Salem, Mass. (18).
Batchelder, John M., No. 3 Divinity Avenue, Cambridge, Mass. (8).
Bell, Samuel N., Manchester, N. H. (7).
Bethune, Rev. Chas. J. S., Trinity Coll. School, Port Hope, Canada (18).
Blake, Eli W., New Haven, Conn. (1).
Blake, Prof. Eli W., jr., Providence, R. I. (15).
Blake, Francis, jr., Assistant U. S. Coast Survey, Newton Lower Falls, Mass. (23).
Bolton, Dr. H. C., School of Mines, Columbia College, New York (17).
Bouvé, Thomas T., Pres't Boston Soc. Nat. History, Boston, Mass. (1).
Bowditch, Henry I., M.D., 113 Boylston St., Boston, Mass. (2).
Brackett, Prof. C. F., College of New Jersey, Princeton, N. J. (19).
Brewer, Prof. Wm. H., Box 55, New Haven, Conn. (20).
Brocklesby, Prof. John, Trinity College, Hartford, Conn. (23).
Brooks, William K., Johns Hopkins University, Baltimore, Md. (23).
Bross, Hon. Wm., Chicago, Ill. (7).
Brown, Robert, jr., Office Cincinnati Gas Light Co., Cincinnati, Ohio (11).
Brown, Mrs. Robert, jr., Cincinnati, Ohio (17).
Brush, Prof. George J., Yale College, New Haven, Conn. (4).
Bushee, Prof. James, Worcester, Mass. (9).

Caldwell, Prof. George C., Cornell University, Ithaca, N. Y. (23).
Capen, Rev. Francis L., 5 Worcester Square, Boston, Mass. (23).
Carmichael, Prof. Henry, Bowdoin College, Brunswick, Me. (21).

Carrier, Prof. Joseph C., President St. Mary's College, Galveston, Tex. (20).
Chadbourne, Prof. P. A., Pres't Williams Coll., Williamstown, Mass. (10).
Chandler, Prof. C. F., School of Mines, Columbia Coll., New York (19).
Chandler, Prof. W. H., The Lehigh University, Bethlehem, Pa. (19).
Clark, Prof. John E., Mathematics, Yale College, New Haven, Conn. (17).
Clarke, Prof. F. W., Cincinnati University, Cincinnati, Ohio (18).
Coffin, Prof. John H. C., U. S. Navy, Washington, D. C. (1).
Coffin, Selden J., Ph.D. Lafayette College, Easton, Pa. (22).
Collett, John, Newport, Ind. (17).
Comstock, Prof. M. L., Galesburg, Knox Co., Ill. (21).
Cook, Prof. George H., Lock Box 5, New Brunswick, N. J. (18).
Cope, Prof. Edward D., Haddonfield, N. J. (17).
Cornwall, Prof. Henry B., College of New Jersey, Princeton, N. J. (22).
Coues, Elliott, Surgeon U. S. Army, Smithsonian Institution, Washington, D. C. (25).
Cox, Prof. Edward T., Indianapolis, Ind. (19).
Craik, Robert, M.D., McGill University, Montreal, Canada (11).
Cramp, Rev. J. M., D.D., Wolfville, N. S. (11).
Crocker, Charles F., Lawrence, Mass. (22).
Crocker, Susan E., M.D., Lawrence, Mass. (21).
Cummings, Rev. Joseph, D.D., President Wesleyan University, Middletown, Conn. (13).
Curtis, Dr. Josiah, Ebbitt House, Washington, D. C. (18).
Cutting, Dr. Hiram A., Lunenburgh, Vt. (17).

Dall, Mrs. Caroline H., 141 Warren Avenue, Boston, Mass. (18).
Dall, Wm. H., care Smithsonian Institution, Washington, D. C. (18).
Dalrymple, Rev. Dr. E. A., Baltimore, Md. (11).
Dana, Edward Salisbury, New Haven, Conn. (23).
Dana, Prof. James D., New Haven, Conn. (1).
Danforth, Edward, Dep't of Public Instruction, Elmira, N. Y. (11).
Dawson, Dr. J. W., Principal McGill College, Montreal, Canada (10).
Day, F. H., M.D., Wauwatosa, Wis. (20).
Dean, George W., U. S. Coast Survey, Fall River, Mass. (15).
Dimmock, George, Cambridge, Mass. (22).
Dinwiddie, Robert, 113 Water St., New York (1).
Dodge, Charles R., Washington, D. C. (22).
Dutton, Capt. Clarence E., U. S. Ordnance Dep't, Washington, D. C. (23).
Dwyer, Dr. John, Hartford, Conn. (23).

Eaton, Prof. D. G., Packer Institute, Brooklyn, N. Y. (19).
Eddy, Prof. H. T., University of Cincinnati, Cincinnati, Ohio (24).
Edwards, Thomas Carne, P. O. Box 417, Vineland, N. J. (21).
Eimbeck, Wm., U.S.C.S., P. O. Box 1588, San Francisco, Cal. (17).
Elliott, Ezekiel B., Statistical Bureau, Washington, D. C. (10).
Elmore, Samuel E., Pres't Conn. River Banking Co., Hartford, Conn. (23).
Elsberg, Louis, M.D., 614 Fifth Avenue, New York (23).

Emerson, Prof. C. F., Dartmouth College, Hanover, N. H. (22).
Emerson, George B., LL.D., 3 Pemberton Square, Boston, Mass. (1).
Emerton, James H., Salem, Mass. (18).
Engelmann, Dr. George, St. Louis, Mo. (1).
Ernst, Rev. Carl W., P. O. Box 671, Providence, R. I. (23).
Eustis, Prof. Henry L., Cambridge, Mass. (2).
Evans, Asher B., Principal Union School, Lockport, N. Y. (19).

Fairbanks, Rev. Henry, St. Johnsbury, Vt. (14).
Farlow, Dr. W. G., 6 Park Square, Boston, Mass. (20).
Farmer, Moses G., Torpedo Station, Newport, R. I. (9).
Ferrel, William, U. S. Coast Survey Office, Washington, D. C. (11).
Fitch, Edmund H., Secretary Toledo Soc. Nat. Sciences, Toledo, O. (23).
Fitch, Edward H., Ashtabula, Ohio (11).
Fitch, O. H., Ashtabula, Ohio (7).
Foote, Dr. A. E., 3725 Lancaster Avenue, Philadelphia, Pa. (21).
Freeman, H. C., Alto Pass, Union Co., Ill. (17).

Gardiner, Rev. Frederic, D.D., Middletown, Conn. (23).
Garman, S. W., Museum Comp. Zoology, Cambridge, Mass. (20).
Genth, Prof. F. A., University of Pennsylvania, Philadelphia, Pa. (24).
Gilbert, G. K., P. O. Box 806, Washington, D. C. (18).
Gill, Prof. Theodore, Smithsonian Institution, Washington, D. C. (17).
Gillman, Henry, Waldo, Alachua Co., Fla. (24).
Gilman, Daniel C., President of the Johns Hopkins University, Baltimore, Md. (10).
Gleason, F. L., Manufacturer Iron and Steel, Hartford, Conn. (23).
Goessman, Prof. C. A., State Agricultural College, Amherst, Mass. (18).
Goodale, Prof. G. L., Botanic Gardens, Cambridge, Mass. (18).
Goode, Prof. George Brown, Middletown, Conn. (22).
Grant, Mrs. Mary J., Brookfield, Conn. (23).
Gray, Prof. Asa, Botanic Gardens, Cambridge, Mass. (1).
Green, Dr. Traill, Easton, Pa. (1).
Grimes, J. Stanley, 165 Mason St., Milwaukee, Wis. (17).
Grinnan, A. G., Orange Court House, Va. (7).
Grote, Aug. R., Director of the Museum Buffalo Society Natural Sciences, Buffalo, N. Y. (22).
Guyot, Prof. Arnold, Princeton, N. J. (1).

Hadley, George, 1355 Main St., Buffalo, N. Y. (6).
Hagen, Dr. Hermann A., Museum Comp. Zoology, Cambridge, Mass. (17).
Haldeman, Prof. S. S., Chickies, Pa. (1).
Hale, Dr. William H., Albany, N. Y. (19).
Hall, Prof. James, Albany, N. Y. (1).
Hambly, John B., Portsmouth, R. I. (18).
Hamlin, Dr. A. C., Bangor, Me. (10).

Harrington, Prof. Mark W., Ann Arbor, Mich. (22).
Harrison, Dr. B. F. Wallingford, Conn. (11).
Hartshorne, Dr. Henry, Union Springs, Cayuga Co., N. Y. (12).
Hasbrouck, Prof. Isaac E., Rutger's College, New Brunswick, N. J. (23).
Haskins, Rev. David Greene, Cambridge, Mass. (25).
Hawes, George W., New Haven, Conn. (23).
Hawkins, Dr. B. Waterhouse, Geological Museum, New Jersey College, Princeton, N. J. (17).
Hawley, C. T., 2400 Spring Avenue, Milwaukee, Wis. (20).
Hedrick, B. S., Examiner U. S. Patent Office, Washington, D. C. (19).
Henry, Prof. Joseph, Secretary Smithsonian Institution, Washington, D. C. (1).
Hilgard, Prof. Eugene W., University of California, Berkeley, Cal. (11).
Hilgard, Prof. Julius E., U. S. Coast Survey, Washington, D. C. (4).
Hill, George W., Mathematician, Nyack Turnpike, N. Y. (23).
Hill, Rev. Dr. Thomas, 109 State St., Portland, Me. (3).
Hinrichs, Prof. Gustavus, State University, Iowa City, Iowa (17).
Hitchcock, Prof. Charles H., Hanover, N. H. (11).
Holden, Prof. Edward S., Naval Observatory, Washington, D. C. (23).
Horsford, Prof. E. N., Cambridge, Mass. (1).
Hosford, Charles E., Terre Haute, Ind. (20).
Hough, Franklin B., Lowville, N. Y. (4).
Hough, G. W., Albany, N. Y. (15).
Howe, Elliott C., M.D., Yonkers, N. Y. (19).
Hoy, Philo R., M.D., Racine, Wis. (17).
Hunt, George, Providence, R. I. (9).
Hunt, Dr. T. Sterry, St. James Hotel, Boston, Mass. (1).
Huntington, Prof. J. H., Hanover, N. H. (19).
Hyatt, Prof. Alpheus, Natural History Society, Boston, Mass. (18).
Hyatt, James, Stanfordville, Dutchess Co., N. Y. (10).

Jenks, Elisha T., Middleborough, Mass. (22).
Jenks, Prof. J. W. P., Middleborough, Mass. (2).
Johnson, Prof. S. W., Sheffield Scientific School, New Haven, Conn. (22).
Johnston, Prof. John, Middletown, Conn. (1).
Julien, Alexis A., School of Mines, Columbia College, New York (24).

Kellogg, Justin, 259 River St., Troy, N. Y. (19).
Kerr, Prof. W. C., Raleigh, N. C. (10).
King, Robert, M. D., Kalamazoo, Mich. (21).
Kirkwood, Prof. Daniel, Bloomington, Ind. (7).

Lambert, Thomas R., Charlestown, Mass. (18).
Lambert, Dr. T. S., 449 West 21st St., New York (21).
Langley, Prof. John W., University of Michigan, Ann Arbor, Mich. (23).
Langley, Prof. S. P., Director of Observatory, Allegheny, Pa. (18).

Lattimore, Prof. S. A., University of Rochester, Rochester, N. Y. (15).
Laws, Samuel S., Columbia, Mo. (23).
Lea, Dr. Isaac, 1622 Locust St., Philadelphia, Pa. (1).
LeConte, Dr. John L., 1625 Spruce St., Philadelphia, Pa. (1).
Leeds, Prof. Albert R., Stevens Institute, Hoboken, N. J. (23).
Leonard, N. R., State University, Iowa City, Iowa (21).
Lesley, Prof. J. Peter, State Geol. of Pa., 1008 Clinton St., Phil., Pa. (2).
Levette, Gilbert M., Geologist, Indianapolis, Ind. (23).
Lindsley, Dr. J. B., Nashville, Tenn. (1).
Lintner, J. A., N. Y. State Museum of Nat. Hist., Albany, N. Y. (22).
Little, Prof. George, State Geologist of Georgia, Atlanta, Ga. (15).
Little, W. C., Albany, N. Y. (22).
Lockwood, Rev. Samuel, Freehold, N. J. (18).
Loomis, Prof. Elias, New Haven, Conn. (1).
Loughridge, Prof. R. H., Ass't State Geologist, Atlanta, Ga. (21).
Lovering, Prof. Joseph, Cambridge, Mass. (2).
Lupton, Prof. N. T., Vanderbilt Univ., Nashville, Tenn. (17).
Lyman, Prof. Chester S., New Haven, Conn. (4).
Lyman, Col. Theodore, Brookline, Mass. (23).
Lyon, Dr. Henry, 34 Monument Sq., Charlestown, Mass. (18).

Mallet, Prof. J. W., University of Va., Charlottesville, Va. (25).
Malone, David R., M.D., Edinburg, Ind. (20).
Mann, B. Pickman, 19 Follen St., Cambridge, Mass. (22).
Marcy, Prof. Oliver, Evanston, Ill. (10).
Marsh, Prof. O. C., Yale College, New Haven, Conn. (15).
Mayer, Prof. Alfred M., Stevens Inst. Technology, Hoboken, N. J. (19).
McClintock, Frank, West Union, Iowa (22).
McMurtrie, William, Dep't Agriculture, Washington, D. C. (22).
McRae, Hamilton S., Sup't of Schools, Muncie, Ind. (20).
Meehan, Thomas, Germantown, Pa. (17).
Meigs, Dr. James Aitken, 1408 Spruce St., Philadelphia, Pa. (12).
Mendenhall, Prof. T. C., Agric. and Mechanical Coll., Columbus, Ohio (20).
Mitchell, Miss Maria, Vassar College, Poughkeepsie, N. Y. (4).
Moore, Prof. James W., M. D., Lafayette College, Easton, Pa. (22).
Morgan, Hon. L. H., Rochester, N. Y. (10).
Morley, Prof. Edward W., Hudson, Ohio (18).
Morris, Rev. John G., Baltimore, Md. (12).
Morse, Prof. Edward S., Salem, Mass. (18).
Morton, Henry, Stevens Institute Technology, Hoboken, N. J. (18).
Munroe, Prof. Chas. E., U. S. Naval Academy, Annapolis, Md. (22).

Nason, Prof. H. B., Rensselaer Polytechnic Inst., Troy, N. Y. (13).
Nelson, The Rev. Dr. Cleland K., Annapolis, Md. (12).
Newberry, Prof. J. S., Cleveland, Ohio, and Columbia Coll., New York (5).
Newcomb, Prof. Simon, U. S. Naval Observatory, Washington, D. C. (13).

Newton, Hubert A., New Haven, Conn. (6).
Nichols, Prof. W. R., Mass. Inst. Technology, Boston, Mass. (18).
Niles, Prof. W. H., Cambridge, Mass. (16).
Nipher, Prof. F. E., Washington University, St. Louis, Mo. (24).
Norton, Prof. W. A., New Haven, Conn. (6).

Oliver, Prof. James E., Cornell University, Ithaca, N. Y. (7).
Oliver, Miss Mary E., Cascadilla Hotel, Ithaca, N. Y. (20).
Orton, Prof. Edward, President Ohio Agricultural and Mechanical College, Columbus, Ohio (19).
Osborne, John W., 212 Delaware Ave., N. E., Washington, D. C. (22).
Osten Sackerr, Baron R., Newport, R. I. (10).
Owen, Dr. Richard, Indiana State University, Bloomington, Ind. (20).

Packard, Dr. A. S., jr., Director Peabody Acad. Science, Salem, Mass. (16).
Paine, Cyrus F., Rochester, N. Y. (12).
Paine, Nathaniel, Worcester, Mass. (18).
Palfray, Hon. Charles W., Salem, Mass. (21).
Parkhurst, Henry M., Law Stenographer, 33 Park Row, New York (23).
Parry, Dr. Charles C., Davenport, Iowa (6).
Peckham, S. F., University of Minnesota, Minneapolis, Minn. (18).
Pedrick, Wm. R., Lawrence, Mass. (22).
Peirce, B. O., 413 Broadway, Cambridge, Mass. (18).
Perkins, Maurice, Schenectady, N. Y. (15).
Pettee, Prof. William H., Ann Arbor, Mich. (24).
Phippen, Geo. D., Salem, Mass. (18).
Pickering, Prof. Edw. C., Director of Observatory, Cambridge, Mass. (18).
Pourtales, L. F., Keeper Museum Comp. Zoology, Cambridge, Mass. (1).
Powell, Major J. W., 910 M St., Washington, D. C. (23).
Prescott, Prof. Albert B., Ann Arbor, Mich. (23).
Prime, Prof. Frederick, jr., Lafayette College, Easton, Pa. (24).
Pumpelly, Prof. Raphael, Owego, Tioga County, N. Y. (17).
Putnam, F. W., Curator Peabody Museum Archæology and Ethnology, Cambridge, Mass. Address as Permanent Secretary A. A. A. S., Salem, Mass. (10).
Pynchon, Rev. Thomas R., Pres't of Trinity College, Hartford, Conn. (23).

Quimby, Prof. E. T., Hanover, N. H. (22).
Quincy, Edmund, 160 South Market St., Boston, Mass. (11).

Rauch, Dr. John H., Chicago, Ill. (11).
Raymond, Rossiter W., 17 Burling Slip, New York (15).
Redfield, John H., care A. Whitney & Sons, Philadelphia, Pa. (1).
Remsen, Prof. Ira, Johns Hopkins University, Baltimore, Md. (22).
Rice, Prof. Wm. North, Middletown, Conn. (18).
Richards, Prof. Robert H., Mass. Inst. Tech., Boston, Mass. (22).

Riley, Prof. C. V., St. Louis, Mo. (17).
Rockwell, Joseph P., Creston, Iowa (17).
Rockwood, Prof. Charles G., jr., Rutger's Coll., New Brunswick, N. J. (20).
Roepper, Charles W., Bethlehem, Pa. (23).
Rogers, Fairman, 202 West Rittenhouse Square, Philadelphia, Pa. (11).
Rogers, Prof. Robert E., University of Penn., Philadelphia, Pa. (18).
Rogers, W. A., Ass't Harvard Coll. Observatory, Cambridge, Mass. (15).
Rogers, Prof. William B., 117 Marlborough St., Boston, Mass. (1).
Rood, Prof. O. N., New York (14).
Roosevelt, Clinton, 11 Wall St., New York (11).
Ross, Dr. Alexander M., Toronto, Ontario (21).
Runkle, Prof. J. D., President Institute of Technology, Boston, Mass. (2).
Rutherford, Lewis M., 175 Second Ave., New York (13).

Sadtler, Prof. Samuel P., University of Penn., Philadelphia, Pa. (22).
Safford, Dr. James M., Nashville, Tenn. (6).
Saunders, William, London, Ontario, Canada (17).
Saville, Dr. John J., Sioux City, Iowa (22).
Schott, Charles A., Coast Survey Office, Washington, D. C. (8).
Scudder, Samuel H., Cambridge, Mass. (13).
Seaman, Wm. H., Microscopist, Agric. Dep't, Washington, D. C. (23).
Shaler, Prof. N. S., Newport, Ky., and Cambridge, Mass. (19).
Sias, Solomon, M.D., Schoharie, Schoharie Co., N. Y. (10).
Sill, Hon. Elisha N., Cuyahoga Falls, Ohio (6).
Silliman, Prof. Benjamin, Yale College, New Haven, Conn. (1).
Silliman, Prof. Justus M., Lafayette College, Easton, Pa. (19).
Smith, Dr. J. Lawrence, Louisville, Ky. (1).
Smith, Prof. S. I., Yale College, New Haven, Conn. (18).
Snell, Prof. Ebenezer S., Amherst, Mass. (2).
Spencer, John W., Teacher and Geologist, Paxton, Ind. (20).
Stanard, Benjamin A., Cleveland, Ohio (6).
Stearns, Henry P., M.D., Hartford, Conn. (23).
Stearns, R. E. C., P. O. Box 567, Oakland, Cal. (18).
Steiner, Dr. Lewis H., Frederick City, Md. (7).
Stephens, W. Hudson, Lowville, N. Y. (18). Life Member.
Stetson, Thomas D., Solicitor of Patents, 155 E. 71st St., New York (23).
Stockwell, John N., 579 Case Ave., Cleveland, Ohio (18).
Stone, Mrs. Leander, 1571 Indiana Ave., Chicago, Ill. (22).
Storrs, Henry E., Jacksonville, Ill. (20).
Stuart, Prof. A. P. S., Lincoln, Nebraska. (21).
Swallow, Prof. G. C., Columbia, Mo. (10).

Tappan, Prof. Eli T., Kenyon College, Gambier, Ohio (20).
Terry, Prof. Nathaniel M., U. S. Naval Academy, Annapolis, Md. (23).
Thompson, Aaron R., 50 West 51st Street, New York (1).
Thurston, Prof. Robert H., Stevens Institute, Hoboken, N. J. (23).

Toner, Joseph M., M.D., 615 Louisiana Ave., Washington, D. C. (23).
Trowbridge, Prof. John, Cambridge, Mass. (25).
Trowbridge, Prof. W. P., New Haven, Conn. (10).
Turnbull, Laurence, M.D., 1208 Spruce St., Philadelphia, Pa. (10).
Tuttle, Prof. Albert H., Columbus, Ohio (17).

Uhler, Philip R., Baltimore, Md. (19).

Van der Weyde, Dr. P. H., P. O. Box 4379, New York (17).
Van Vleck, Prof. John M., Middletown, Conn. (23).
Vaux, William S., 1702 Arch St., Philadelphia, Pa. (1). Life Member.
Verrill, Prof. A. E., Yale College, New Haven, Conn. (16).
Vose, Prof. George L., Bowdoin College, Brunswick, Me. (15).

Wadsworth, M. Edward, Instructor in Mathematics and Mineralogy, Harvard University, Cambridge, Mass. (23).
Walker, Prof. Joseph B., care Bank of Kentucky, Louisville, Ky. (20).
Walker, Prof. J. R., Napoleon Ave., cor. Coliseum St., New Orleans, La. (19).
Waller, Elwyn, School of Mines, Columbia College, New York (23).
Walling, H. F., 102 Chauncy St., Boston, Mass. (16).
Ward, Prof. Henry A., Rochester, N. Y. (13).
Ward, Henry D. A., Meteorologist, Middletown, Conn. (23).
Ward, Dr. R. H., 53 Fourth St., Troy, N. Y. (17).
Warner, H. C., West Union, Iowa. (21).
Warner, James D., 199 Baltic St., Brooklyn, N. Y. (18). Life Member.
Warren, Gen. G. K., U.S.A., Engineer's Office, Newport, R. I. (12).
Warren, Prof. S. Edward, Newton, Mass. (17).
Watson, Sereno, Botanic Gardens, Cambridge, Mass. (22).
Webster, Prof. Nathan B., Principal Webster Inst., Norfolk, Va. (7).
Wells, Daniel H., Rocky Hill, Conn. (18).
Westcott, O. S., High School, Chicago, Ill. (21).
Wheatland, Dr. Henry, President Essex Institute, Salem, Mass. (1).
Wheatley, Charles M., Phœnixville, Pa. (1).
Wheildon, W. W., Concord, Mass. (13).
White, Prof. C. A., P. O. Box 806, Washington, D. C. (17).
Whitfield, R. P., 173 Elm St., Albany, N. Y. (18).
Whitney, Solon F., Watertown, Mass. (20).
Whittlesey, Col. Charles, Cleveland, Ohio (1).
Wilber, G. M., Pine Plains, N. Y. (19).
Wilbur, A. B., Port Jervis, Orange Co., N. Y. (23).
Wilder, Prof. Burt G., Cornell University, Ithaca, N. Y. (22).
Wiley, Prof. Harvey W., Purdue University, La Fayette, Ind. (21).
Williams, Charles H., M.D., 15 Arlington St., Boston, Mass. (22).
Williams, Prof. Henry W., 15 Arlington St., Boston, Mass. (11).
Wilson, Prof. Daniel, 117 Bloor St., Toronto, Canada (25.)

Winchell, Prof. Alex., Nashville, Tenn. (3)
Winchell, Prof. N. H., University of Minnesota, Minneapolis, Minn. (19).
Witter, F. M., Muscatine, Iowa (21).
Woodworth, Dr. John M., U. S. Marine Hospital Service, Washington, D. C. (17).
Worthen, A. H., Warsaw, Ill. (5).
Wright, Prof. Arthur W., Yale College, New Haven, Conn. (14).
Würtele, Rev. Louis C., Acton Vale, Province of Quebec, Canada East (11).
Wyckoff, Wm. C., Tribune Office, New York (20).
Wylie, Prof. Theophilus A., Indiana University, Bloomington, Ind. (20).

Yarrow, Dr. H. C., care Army Medical Museum, Washington, D. C. (23).
Youmans, Prof. Edward L., New York (6).
Young, Prof. Charles A., Dartmouth College, Hanover, N. H. (18).

[315 FELLOWS.]

TOTAL NUMBER OF MEMBERS AND FELLOWS, 867.

SUBSCRIBERS TO THE BUFFALO VOLUME.

Rev. W. Alfred Gay, 92 North Washington St., Buffalo, N. Y.
Hampton Dodge, 48 Court St., Buffalo, N. Y.
Rudolph Koenig, Ph.D., 30 Rue Hautefeuille, Paris, France.
A. R. Wright, M.D., 162 Pearl St., Buffalo, N. Y.
Abby J. Seymour, M.D., 35 Niagara Square, Buffalo, N. Y.
Young Men's Association, Buffalo, N. Y.
H. T. Buttolph, Buffalo, N. Y.
C. C. Merriman, Rochester, N. Y.

MEMBERS ELECTED

AT

BUFFALO MEETING.

One hundred and forty-six members were elected at the Buffalo Meeting. Of these one hundred and seventeen have paid the admission fee and assessment for the meeting, and their names have been incorporated in the list of Members. The following have not yet replied to the notifications sent to them:

Brown, Prof. W. Le Roy, Vanderbilt University, Nashville, Tenn.
Bush, John J., Clifton, Canada.
Calthrop, Rev. S. R., Syracuse, N. Y.
Farr, A. G., Columbus, Ohio.
Fletcher, Rev. Luther I., D.D., 263 Pearl St., Buffalo, N. Y.
Foster, William Edwards, Buffalo, N. Y.
Green, Lt. Commander F. M., U.S.N., Washington, D. C.
Hensel, Martin, Columbus, Ohio.
Holmes, Frank, Kingston, Mass.
Hovey, W. A., Ed. "Boston Evening Transcript," Boston, Mass.
Kedzie, Prof. Wm. K., Kansas Agricultural College, Manhattan, Kansas.
Poor, I. V., Brookline, Mass.
Postell, James, St. Simon's Island, Brunswick, Ga.
Powers, Stephen, Waterford, Washington Co., Ohio.
Shumway, Dr. Thomas D., Plymouth, Mass.
Silliman, Wyllis A., 3 Divinity Hall, Cambridge, Mass.
Smith, Alfred W., Adrian, Mich.
Thompson, A. H., Washington, D. C.
Ufford, H. P., Chillicothe, Ohio.
Venable, Prof. Charles, University of Virginia, Charlottesville, Va.
White, Prof. Henry C., University of Georgia, Athens, Ga.
White, Prof. I. C., Morgantown, W. Va.
Wilkinson, Russell, Box 189 P. O., Toronto, Canada.
Zalinski, Lieut. E. L., U. S. Army, St. Augustine, Fla.

DECEASED MEMBERS.

[Information respecting omissions in this list, and the date of birth and of decease of any of the former members, is requested by the Permanent Secretary.]

Abert, J. J., Washington, D. C. (1).
Adams, C. B., Amherst, Mass. (1).
Adams, Edwin F., Charlestown, Mass. (18).
Agassiz, Louis, Cambridge, Mass. (1).
Ainsworth, J. G., Barry, Mass. (14).
Allston, R. F. W., Georgetown, S. C. (3).
Anthony, Charles H., Albany, N. Y. (6).
Ames, N. P., Springfield, Mass. (1).
Appleton, Nathan, Boston, Mass. (1).

Bache, Alexander D., Washington, D. C. (1).
Bache, Franklin, Philadelphia, Pa. (1).
Bailey, J. W., West Point, N. Y. (1).
Barrett, Moses, Milwaukee, Wis. (21).
Beck, C. F., Philadelphia, Pa. (1).
Beck, Lewis C., New Brunswick, N. J. (1).
Beck, T. Romeyn, Albany, N. Y. (1).
Benedict, G. W., Burlington, Vt. (16).
Bicknell, Edwin, Boston, Mass. (18).
Binney, Amos, Boston, Mass. (1).
Binney, John, Boston, Mass. (3).
Blanding, William, R. I. (1).
Blatchford, Thomas W., Troy, N. Y. (6).
Blatchley, Miss S. L., New Haven, Conn. (19).
Bomford, George, Washington, D. C. (1).
Bradley, Leverette, Jersey City, N. J. (15).
Braithwaite, Jos., Chambly, C. W. (11).
Brown, Andrew, Natchez, Miss. (1).
Burnap, G. W., Baltimore, Md. (12).
Burnett, Waldo I., Boston, Mass. (1).
Butler, Thomas B., Norwalk, Conn. (10).

Carpenter, Thornton, Camden, S. C. (7).
Carpenter, William M., New Orleans, La. (1).
Case, William, Cleveland, Ohio (6).
Caswell, Alexis, Providence, R. I. (2).
Chapman, N., Philadelphia, Pa. (1).
Chase, S. Dartmouth, N. H. (2).
Chauvenet, William, St. Louis, Mo. (1).
Clapp, Asahel, New Albany, Ind. (1).
Clark, H. J., Cambridge, Mass. (13).
Clark, Joseph, Cincinnati, Ohio (5).
Clarke, A. B., Holyoke, Mass. (13).
Cleaveland, A. B., Cambridge, Mass. (2).
Cleaveland, C. H., Cincinnati, Ohio (9).
Coffin, James H., Easton, Pa. 1.
Cole, Thomas, Salem, Mass. (1).
Coleman, Henry, Boston, Mass. (1).
Cooper, William, Hoboken, N. J. (9).
Corning, Erastus, Albany, N. Y. (6).
Couper, J. Hamilton, Darien, Ga. (1).
Crosby, Alpheus, Salem, Mass. (10).
Crosby, Thomas R., Hanover, N. H. (18).
Croswell, Edwin, Albany, N. Y. (6).
Curry, W. F., Geneva, N. Y. (11).

Dayton, Edwin A., Madrid, N. Y. (7).
Dean, Amos, Albany, N. Y. (6).
Dearborn, George H. A. S., Roxbury, Mass. (1).
Dekay, James E., New York (1).
DeLaski, John, Carver's Harbor, Me. (18).
Dewey, Chester, Rochester, N. Y. (1).
Dexter, G. M., Boston, Mass. (11).
Dillingham, W. A. P., Augusta, Me. (17).
Doggett, Wm. E., Chicago, Ill. (17).
Doolittle, L., Lenoxville, C. E. (11).
Ducatel, J. T., Baltimore, Md. (1).
Duffield, George, Detroit, Mich. (10).
Dumont, A. H., Newport R. I. (14).
Duncan, Lucius C., New Orleans, La. (10).
Dunn, R. P., Providence, R. I. (14).

Easton, Norman, Fall River, Mass. (14).
Ely, Charles Arthur, Elyria, Ohio (4).
Emmons, Ebenezer, Williamstown, Mass. (1).
Engstrom, A. B., Burlington, N. J. (1).
Everett, Edward, Boston, Mass. (2).
Ewing, Thomas, Lancaster, Ohio (5).

Ferris, Isaac, New York (6).
Feuchtwanger, Lewis, New York (11).
Fillmore, Millard, Buffalo, N. Y. (7).
Fisher, Mark, Trenton, N. J. (10).
Fitch, Alexander, Hartford, Conn. (1).
Forbush, E. B., Buffalo, N. Y. (15).
Force, Peter, Washington, D. C. (4.)
Foster, J. W., Hyde Park, Chicago, Ill. (1).
Foucon, Felix, Madison, Wis. (18).
Fowle, Wm. B., Boston, Mass. (1).
Fox, Charles, Grosse Ile, Mich. (7).
Frazer, John F., Philadelphia, Pa. (1).
French, J. W., West Point, N. Y. (11).

Gavit, John E., New York (1).
Gay, Martin, Boston, Mass. (1).
Gibbon, J. H., Charlotte, N. C. (3).
Gillespie, W. M., Schenectady, N. Y. (10.)
Gilmor, Robert, Baltimore, Md. (1).
Gould, Augustus A., Boston, Mass. (11).
Gould, B. A., Boston, Mass. (2).
Graham, James D., Washington, D. C. (1).
Gray, Alonzo, Brooklyn, N. Y. (13).
Gray, James H., Springfield, Mass. (6).
Greene, Benjamin D., Boston, Mass. (1).
Greene, Everett W., Madison, N. J. (10).
Greene, Samuel, Woonsocket, R. I. (9).
Greer, James, Dayton, Ohio (20).
Griffith, Robert E., Philadelphia, Pa. (1).
Griswold, John A., Troy, N. Y. (19).
Guest, William E., Ogdensburg, N. Y. (6).

Hackley, Charles W., New York (4).
Hale, Enoch, Boston, Mass. (1).
Hance, Ebenezer, Fallsington P. O. Pa., (7).
Hare, Robert, Philadelphia, Pa (11).
Harlan, Joseph G., Haverford, Pa. (8).
Harlan, Richard, Philadelphia, Pa. (1).
Harris, Thaddeus W., Cambridge, Mass. (1).
Harrison, Jos., jr., Philadelphia, Pa. (12).
Hart, Simeon, Farmington, Conn. (1).
Haven, Joseph, Chicago, Ill. (17).
Hayden, H. H., Baltimore, Md. (1).
Hayward, James, Boston, Mass. (1).
Hilgard, Theo. C., St. Louis, Mo. (17).
Hickox, S. V. R., Chicago, Ill. (17).

Hincks, William, Toronto, C. W. (11).
Hitchcock, Edward, Amherst, Mass. (1).
Hodgson, W. B., Savannah, Ga. (10).
Holbrook, J. E., Charleston, S. C. (1).
Hopkins, Albert, Williamstown, Mass. (19).
Hopkins, James G., Ogdensburg, N. Y. (10).
Hopkins, T. O., Williamsville, N. Y. (10).
Hosford, Benj. F., Haverhill, Mass. (13).
Horton, C. V. R., Chaumont, N. Y. (10).
Horton, William, Craigville, Orange Co., N. Y. (1).
Houghton, Douglas, Detroit, Mich. (1).
Howland, Theodore, Buffalo, N. Y. (15).
Hubbert, James, Richmond, Province of Quebec (16).
Hunt, E. B., Washington, D. C. (2).
Hunt, Freeman, New York (11).

Ives, Moses B., Providence, R. I. (9).
Ives, Thomas P., Providence, R. I. (10).

Johnson, W. R., Washington, D. C. (1).
Jones, Catesby A. R., Washington, D. C. (8).

Keep, N. C., Boston, Mass. (13).
Kennicott, Robert, West Northfield, Ill. (12).
King, Mitchell, Charleston, S. C. (3).
Knickerbocker, Charles, Chicago, Ill. (17).

Lacklan, R., Cincinnati, Ohio (11).
Lapham, Increase A., Milwaukee, Wis. (3).
LaRoche, R., Philadelphia, Pa. (12).
Lasel, Edward, Williamstown, Mass. (1).
Lawford, Frederick, Montreal, Canada (11).
Lederer, Baron von, Washington, D. C. (1).
Lieber, Oscar M., Columbia, S. C. (8).
Lincklaen, Ledyard, Cazenovia, N. Y. (1).
Linsley, James H., Stafford, Conn. (1).
Lockwood, Moses B., Providence, R. I. (9).
Logan, William E., Montreal, Canada (1).
Loosey, Charles F., New York (12).
Lothrop, Joshua R., Buffalo, N. Y. (15).
Lyon, Sidney S., Jeffersonville, Ind. (20).

Maack, G. A., Cambridge, Mass. (18).
Mahan, D. H., West Point, N. Y. (9).
Marsh, Dexter, Greenfield, Mass. (1).

Marsh, James E., Roxbury, Mass. (10).
Mather, William W., Columbus, Ohio (1).
Maupin, S., Charlottesville, Va. (10).
McMahon, Mathew, Albany, N. Y. (11).
M'Conihe, Isaac, Troy, N. Y. (4).
Meade, George G., Philadelphia, Pa. (15).
Meek, F. B., Washington, D. C. (6).
Mitchell, O. M., Cincinnati, Ohio (3).
Mitchell, William, Poughkeepsie, N. Y. (2).
Mitchell, Wm. H., Florence, Ala. (17).
Morton, S. G., Philadelphia, Pa. (1).
Munroe, Nathan, Bradford, Mass. (6).

Newton, E. H., Cambridge, N. Y. (1).
Nicollett, J. N., Washington, D. C. (1).
Norton, J. P., New Haven, Conn. (1).
Noyes, J. O., New Orleans, La. (21).
Nutt, Cyrus, Bloomington, Ind. (20).

Oakes, William, Ipswich, Mass. (1).
Olmsted, Alexander F., New Haven, Conn. (4).
Olmsted, Denison, New Haven, Conn. (1).
Olmsted, Denison, jr., New Haven, Conn. (1).

Painter, Jacob, Lima, Pa. (23).
Painter, Minshall, Lima, Pa. (7).
Parkman, Samuel, Boston, Mass. (1).
Perkins, George R., Utica, N. Y. (1).
Perkins, Henry C., Newburyport, Mass. (18).
Perry, John B., Cambridge, Mass. (16).
Perry, M. C., New York (10).
Piggot, A. Snowden, Baltimore, Md. (10).
Plumb, Ovid, Salisbury, Conn. (9).
Pope, Charles A., St. Louis, Mo. (12).
Porter, John A., New Haven, Conn. (14).
Pugh, Evan, Centre Co., Penn. (14).

Redfield, William C., New York (1).
Resor, Jacob, Cincinnati, Ohio (8).
Robb, James, Fredericton, N. B. (4).
Robinson, Coleman T., Buffalo, N. Y. (15).
Rockwell, John A., Norwich, Conn. (10).
Rogers, Henry D., Glasgow, Scotland (1).
Rogers, James B., Philadelphia, Pa. (1).

Schaeffer, Geo. C., Washington, D. C. (1).
Scott, Joseph, Dunham, C. E. (11).
Senter, Harvey S., Aledo, Ill. (20).
Seward, William H., Auburn, N. Y. (1).
Sherwin, Thomas, Dedham, Mass. (11).
Silliman, Benjamin, New Haven, Conn. (1).
Skinner, John B., Buffalo, N. Y. (15).
Slack, J. H., Philadelphia, Pa. (12).
Smith, J. V., Cincinnati, Ohio (5).
Smith, James Y., Providence, R. I. (9).
Smith, Lyndon A., Newark, N. J. (9).
Sparks, Jared, Cambridge, Mass. (2).
Stimpson, William, Chicago, Ill. (12).
Stone, Samuel, Chicago, Ill. (17).
Sullivant, W. S., Columbus, Ohio (7).

Tallmadge, James, New York (1).
Taylor, Richard C., Philadelphia, Pa. (1).
Teschemacher, J. E., Boston, Mass. (1).
Thompson, Alexander, Aurora, N. Y. (6).
Thompson, Z., Burlington, Vt. (1).
Thurber, Isaac, Providence, R. I. (9).
Tillman, Samuel D., Jersey City, N. J. (15).
Tolderoy, James B., Fredericton, N. B. (11).
Torrey, John, New York (1).
Torrey, Joseph, Burlington, Vt. (2).
Totten, J. G., Washington, D. C. (1).
Townsend, Howard, Albany, N. Y. (10).
Townsend, John K., Philadelphia, Pa. (1).
Townsend, Robert, Albany, N. Y. (9).
Troost, Gerard, Nashville, Tenn. (1).
Tuomey, M., Tuscaloosa, Ala. (1).
Tyler, Edward R., New Haven, Conn. (1).

Vancleve, John W., Dayton, Ohio (1).
Vanuxem, Lardner, Bristol, Pa. (1).

Wadsworth, James S., Genesee, N. Y. (2).
Wagner, Tobias, Philadelphia, Pa. (9).
Walker, Joseph, Oxford, N. Y. (10).
Walker, Sears C., Washington, D. C. (1).
Walker, Timothy, Cincinnati, Ohio (4).
Walsh, Benjamin D., Rock Island, Ill. (17).
Warren, John C., Boston, Mass. (1).
Webster, H. B., Albany, N. Y. (1).
Webster, J. W., Cambridge, Mass. (1).

Webster, M. H., Albany, N. Y. (1).
Weed, Monroe, Wyoming, N. Y. (6).
Weyman, G. W., Pittsburg, Pa. (6).
Wheatland, Richard H., Salem, Mass. (13).
Whitman, Wm. E., Philadelphia, Pa. (23).
Whitney, Asa, Philadelphia, Pa. (1).
Whittlesey, Charles C., St. Louis, Mo. (11).
Willard, Emma, Troy, N. Y. (15).
Wilson, W. C., Carlisle, Pa. (12).
Winlock, Joseph, Cambridge, Mass. (5).
Woodbury, L., Portsmouth, N. H. (1).
Woodman, John S., Hanover, N. H. (11).
Wright, John, Troy, N. Y. (1).
Wyman, Jeffries, Cambridge, Mass. (1).

Young, Ira, Hanover, N. H. (7).

[256 DECEASED MEMBERS.]

ADDRESS

OF

Dr. JULIUS E. HILGARD,

THE RETIRING PRESIDENT OF THE ASSOCIATION.

Gentlemen and Ladies of the American Association for the Advancement of Science: —

In availing myself of the privilege accorded by the custom of the Association to its President of the preceding year, of making a general address, I cannot but feel a strong temptation, under the influence of the Centennial year of the Republic, to attempt a general review of the progress of science during the past century. The genius of the year is that of retrospective contemplation, of comparison of the past with the present, not unaccompanied with a justifiable sense of satisfaction at the evidences of real advance.

Much has been said and written on the progress of the nation in polity, in wealth, in the arts, and in science. The Centennial reviews, published in several of the leading popular periodicals, have sketched with more or less detail the history of scientific development in this country, but there still seems to remain room for a philosophic appreciation of the change that has been wrought in the dominant modes of thought in the different branches of human knowledge. The occasion invites the attempt to trace critically the steps by which the science of pure mathematics, having perhaps exhausted the methods within the range of the material world, has sought a new field of activity in investigating the properties of magnitudes inconceivable to the human mind as at present constituted, such as space of five dimensions, while the application of the previously developed mathematical modes of thought to the problems of the forces of nature has immensely aided the appropriation of those forces to the practical uses of man: to follow the influence which the principle of the conservation of force, recognized ages ago in the philosophic axiom that cause and effect are equivalent, but a hundred years ago still standing applied only to the doctrine of mass-motion, has exercised upon modern modes of thought, as the basis of the correlation of forces, the fruitful principle by which all phenomena of molecular force,—light, heat, electricity, galvanism and magnetism,—are reduced to the common idea of some mode of motion, convertible one with another and with mass-motion:

In Chemistry, to show how the same fundamental idea has led us from the crude conceptions of phlogiston and anti-phlogiston, of free and latent Caloric, to the acceptance of the energy of molecular motion as the condition of chemical combinations, or, as we should now say, the association or dissociation of different molecules:

In Geology, to illustrate how vague notions of cataclysms, upheavals and submergences, imagined to explain observed phenomena, but without assignable cause, have, under the influence of the same potent demand for balancing the debit and credit account of the forces invoked, given way to the research and discrimination of effects of recognized and enduring causes, acting slowly but ceaselessly; so slowly that the astronomical evidence of cosmical changes has been called in to account for even that very recent and superficial change, the glacial period:

To contrast, further, in the study of organic nature, the crude categories formed an hundred years ago on external resemblances with the classifications of to-day, based upon the most careful observation of genetic differences: and, last in logical sequence, but of the first importance in our day, to appreciate philosophically the progress in the doctrine of evolution, as it is called by its votaries, or the natural history of creation — as may be better understood by many of my hearers — these subjects present themselves forcibly to my mind.

But I am warned by this very brief recital of the broadest division of the subjects that would necessarily be treated of in a general review of the progress of Science during the past century, that a satisfactory treatment of the questions put, even from an individual point of view, is hardly possible within the limits of an address suitable to this occasion, assuredly not within the power of him who addresses you. It will be far wiser to confine myself to a recital of the progress in that department of applied Science to which my attention has mainly been given for many years — that of *Geodesy*, the study of the form of our globe, and that of *Topography*, the mapping of the land.

In treating of this subject my aim will be to lay before you in a succinct manner what has been done, and what is now being done in various countries toward the accurate admeasurement of the earth's dimensions and the delineation of its surface.

Geodesy in any exact sense falls quite within the period of 100 years—although some of the most important geometric principles had been developed, as usual in the progress of science, a century in advance of their application to practice.

The first accurate measurement was made in France, in 1699, by Picard, who first applied telescopes to astronomical observations of position, in the measurement of an arc of the meridian between Malvoisin and Amiens, which operation furnished to Newton the data for the demonstration of the law of gravitation. In that era, under the intelligent auspices of Louis XIV, the French continued to lead in geodesic operations, and in connection with the trigonometrical survey of France, three arcs were measured by the two Cassini (1683–1700) between Bourges, Paris, Amiens, and Dunkirk, by trigonometrical operations, avowedly for the purpose of determining the figure and magnitude of the earth.

While such measurements, taken in middle latitudes, would well serve to determine the average diameter of the earth, they could throw but little light on the question of its ellipticity, which can be derived only from arcs measured in widely different latitudes. From theoretical considerations, supported by the pendulum observations of Huyghens and Richer, Newton had concluded the polar axis of the earth to be between 1 : 180 and 1 : 500 less than its equatorial diameter, while Cassini's measures indicated a slight excess of the polar axis. In order to settle this question, the

French determined to measure arcs of the meridian near the equator and near the pole, in addition to those situated within their own country. Accordingly, about 1740, three degrees were measured in Peru, near the equator, by Bouguer and LaCondamine, and a corresponding arc in Lapland, near Tornea, by Maupertuis and Clairaut. The results of these operations showed a flattening at the pole of between 1 : 200 and 1 : 300, and first established the fact of the spheroidicity of the earth.

Since that time all civilized nations have been engaged in making geodesic surveys of their territories, either with the direct object of measuring arcs of the meridian, or of a parallel of latitude, for the determination of the elements of the earth's figure, or else in connection with a complete trigonometrical survey of their respective countries.

In every instance, as might be supposed, the utilitarian part of the work took precedence in time. The land was surveyed in order that the boundaries of property might be accurately defined, and that taxes might be levied upon it. For these purposes very simple geometry, and plane surveying, was quite sufficient, but when the attempt was made to fit such individual surveys together so as to form the map of a county or of a province, it was found that they could not be fitted accurately, because a plane cannot be truly applied to the surface of a sphere. A trigonometrical framework had therefore to be constructed, which could be adapted to the earth's figure by frequent reference to the true north and by observations of latitude, and into which the detailed surveys could be fitted without sensible error.

France was the first country that covered its entire territory with a trigonometrical network as the basis of general map, and the great map of France, published on scale of 1 : 80,000, or about three-quarters of an inch to the mile, is a monument of which that nation may well be proud. It is true that more perfect methods of representation have since been developed, especially in the matter of orography, or the representation of elevations and depressions, which in the original surveys of France were simply sketched upon the plan by shade-lines or hachures, while at present the system of horizontal contours, or lines of equal elevation, at regular differences of height is the only accepted method of representing the configuration of the surface; but France has not failed to supplement her ancient work in all particulars in which a greater precision seems now to be desirable than was aimed at more than a century ago, and the recent additions to the map of France are more and more brought up to our own day, both in the representation of orography and in the addition of modern detail, such as railroads and their appurtenances.

More familiar to the American, than that of France, is the great trigonometrical survey of Great Britain, commonly called the "Ordnance Survey," because it was first conducted by the Ordnance Department of the War Office, although it has long been under the direction of the Royal Engineers. This survey has covered the whole of the British Isles with a frame-work of triangulation, which serves as the basis of a map surveyed in far greater detail than that of France. The triangulation itself is composed of several successive schemes of different magnitude; first that of the longest sides that the surface of the country would afford, so as to permit a framework of the least possible number of links, and therefore of the greatest accuracy, upon which are based the positions of successively derived points, until there is as nearly as practicable some point trigonometrically determined in every square mile. Much of this work, however, is, as in France, superimposed upon the earlier and less elaborate scheme. The work was begun by making a map of England on a scale of one inch to the mile, but before this was completed the advantage of a detailed map and the insufficiency of its scale had been so fully recognized, that a re-survey on a scale of six inches to the mile, or about 1 : 10,000 of the natural size, was ordered, and the survey of Ireland was actually commenced, and has since been completed on that scale. It so happened that some years since Ireland was of the three realms the best represented on the maps, whatever it might be in Parliament. Since that time, however, the survey of the whole of Great Britain has been nearly completed, and the survey office at Southampton is busily engaged in publishing the results. In the publication on the scale of one inch to the mile England and Wales are represented on 110 sheets, Scotland on 120 and Ireland on 205 sheets. The maps of England and Ireland are completed. Scotland is about half done. The larger towns are separately published on scales varying from 100 to 200 feet to the inch.

We may look upon these maps of Great Britain as the exponent of what is achieved by a complete topographical survey of a country, and for this purpose we will review them somewhat in detail. If I had them here it would, of course, be impossible for you to see them, and I must still address myself to your mental conception as best I may; but to those who visit the International Exhibition at Philadelphia, I would say, do not fail to go and see the ordnance maps of Great Britain, as well as those of Switzerland, Sweden, Norway, and others. I am sure you will be convinced that we, as a nation, do also want to construct a complete map of our own country.

The map of Great Britain, then, consists in the first instance of the original sheets, about two by three feet in size, giving the plan of a corresponding area on a scale of six inches to the mile. Each chart shows the roads, houses, fences, brooks, woods, meadows, fields, copses and every natural and artificial surface detail that has any permanence or is sufficiently large to be seen on the scale of representation. In addition to this, there are numerous elevations marked along the roads and in the fields, all of them determined by actual spirit-levelling, and several points are marked that have been determined trigonometrically and in reference to which all the superficial detail has been located. Such a map contains all the geometric information that the owner of the property can want for his own uses, or that the Commonwealth can want in regard to the same in distances, areas, slopes, points of reference by means of which any new sub-divisions of the land, roads or buildings can be accurately laid down, and the data requisite for planning improvements and estimating their cost. One feature is wanting, which the Swedish maps have, namely, a representation of the quality of the soil.

These maps are reproduced on the original scale, for the use of the inhabitants, in limited number to suit each case, by a process of photo-zincography, and the copies are sold at a shilling apiece, which covers the cost of reproduction, on the average. Moreover, a reduced map is engraved on copper in the very highest style of art, on a scale of one inch to the mile, somewhat larger than the scale of the map of France before spoken of. These maps, each sheet comprising about sixteen sheets of the former, still contain the same information, but presented to the eye in a more comprehensive form, although not with the precision of measurement that the larger scale affords. They serve for all purposes of administration, for engineering plans, for the mapping of geological features, or any statistical facts.

Is a new road in question? It is at once planned on the map and can be laid out by reference to the station points. Does a town want a water supply? Without spending a penny for a survey, an expert can at once determine the possibility from the Ordnance map, and make an approximate estimate of the cost.

But I need not multiply illustrations to show the advantage of a perfect map. That a nation needs to know all the particulars of its domain, as a good housewife knows her chambers and closets, is too obvious to require demonstration.

Continuing our review of the surveys of other European countries, we will next turn our attention to Switzerland, the conformation of which taxes most severely the methods of topographical representation, while its triangulation is comparatively easy. It has been completely surveyed under the auspices of the Federal government, and is represented in an atlas of twenty-five sheets, on a scale of 1 : 100,000, published between 1842 and 1860. There is also a general map in four sheets, and since 1871 the original sheets on a scale of 1 : 25,000 are commenced to be published with the introduction of colors, brown for the contour lines and blue for water, while the general plan is black. These maps are a treasure of excellent topography, in which scientific knowledge, eminent taste and artistic skill are blended in a work which gives equal satisfaction to the geographer and to the artist.

In connection with the triangulation, numerous determinations of azimuth, latitude and longitude have been made, and an extended network of exact leveling has been executed, affording precise points of reference for the altitudes obtained by trigonometrical leveling. These levels of precision, in a country of so excessively broken surface as Switzerland, have given rise to an interesting discussion as to the effect of local attraction on the surface of equilibrium. In connection with the same, observations on the force of gravity, by means of the pendulum, have also been made at numerous points, and the comparative observations at Geneva and on Mt. Righi are a model of experimental work.

In the various states of Germany, detailed topographical surveys, based upon trigonometrical frameworks, and in general executed in commendable style, were accomplished during the first half of the century, but owing to the division of the country into small independencies, they show a great variety as to method and scale. Among the best in style are the maps of the former Electorate of Hessia, surveyed on 1 : 25,000, published on 1 : 50,000, engraved on stone, forty sheets, and those of Bavaria, on like scales, engraved on copper, in 112 sheets.

In Prussia the surveys are made on 1 : 25,000 and the maps published on 1 : 80,000 for Prussia proper and the Eastern provinces; the Rhenish provinces are published on 1 : 100,000 to match the map of France.

Since the reconstitution of the German Empire a scheme for a general map on a uniform scale has been determined on, and the publication of the original maps has been commenced. But several years previous to the political unification a scientific union had been effected, which, under the name of the Central European Geodesic Association, undertook to supply the trigonometrical links requisite for uniting the geodesic surveys of all countries adjoining Germany—the heart of Europe—to remeasure base-lines by a common standard, to make the requisite astronomical determinations of azimuth, latitude and longitude,—in brief, to complete the geodesic network of Central Europe with a degree of precision commensurate with the present state of science. Besides the immediately contiguous nations, Scandinavia, Italy and Spain have fallen into line. France last of all; only Great Britain, with characteristic conservatism, still holds back from a scheme which she did not originate. The annual reports of that Association are among the most important contributions to science, of our day. National and personal emulation constitute a stimulus which is wanting when in any one country some interest has passed into the hands of the select. We all need a thorn in our flesh, to prevent us from taking life too easy.

But to proceed with our review:

The survey of Belgium, like its National autonomy, is of recent origin, but is progressing in a way fully up to the advanced requirements of the day. The Netherlands have a good map of their territory, published (1850–64) in sixty-two sheets; scale 1 : 50,000. The Dutch maps of Java, printed in colors, are admirable specimens of cartography.

Austria is well up to the times in her work on the German provinces, while in Hungary and the Slavonic dependencies the work is far behind in date, and in its quality is to be classed rather as a reconnaisance for military purposes, than as an exact topographical survey — always based, however, on a good trigonometrical framework; and doubtless all is done that the present occasion calls for.

In Italy the want of political unity has until very lately prevented any concerted action, and while science has not been asleep, its practical applications to the needs of the common interest have been awaiting the regeneration of the nation.

In the former Austrian provinces good surveys have been made and published. The power that has reunited Italy has shown its intellectual title to the crown by maintaining territorial surveys of Piedmont and Sardinia, while all the rest slept — the *pontifex maximus* himself forgot his original function.

Spain shows its revival not more brilliantly in any other branch of public life than in its great trigonometrical and topographical survey. Taking advantage of all preceding experience, the work but recently commenced stands in the very front rank of similar enterprises; both as to rapidity of execution by judicious organization and quality of results. Some of the methods devised and practised for the prosecution of the trigonometrical survey are recognized in Europe as establishing the highest grade of accuracy yet attained and the published maps — but few in number as yet — will be readily admitted as having dealt more successfully with the difficulty of representation by successive printings in color than any heretofore attempted.

Of Portugal we have a very creditable map for military purposes, but insufficient for industrial or scientific demands.

Denmark, with a flat and limited territory, has developed the representation of surfaces of small inequality of height with great success. For minute detail and expression of surface-quality the Danish maps excel all others, not being hampered by shading of slopes.

Sweden and Norway are covered by a good framework of triangulation, the topographical surveys are well advanced and the publication of maps is keeping progress. These maps are printed in colors and are notable for expressing general distinctions of soil for economic purposes.

Russia (in Europe) is being surveyed on a general plan of 1½ inches to the mile for average country, half that scale for level country such as the prairies of the Don and twice as large for densely settled townships. Much energy and skill is devoted to the publication of the maps, which are not far behind the best examples of other nations, notwithstanding the vast extent of territory and sparseness of population. The trigonometric work, while it does not quite meet the latest demands of precision, is fully equal to the average standard, and its meridional arc of more than 25° is a most important contribution to pure geodesy.

Such is a brief recital of the work of geodesy and topography done in Europe by the civilized nations of the earth. But before I can pass to the review of what we are doing ourselves, I must mention the stupendous work accomplished in India by our elder cousins, those British — who have covered the country by a triangulation extending from Cape Comorin to Kashmir, from the Indus to the Irawaddy, have added 21° to the meridional measurements, and have made such progress with the topography as the needs of the country warrant.

They have, moreover, measured an arc of 4° northwardly from the Cape of Good Hope, part of which had been previously measured by the French with less adequate means.

Turning now to our own country, we find in comparison with the older countries of Europe, a great want of accurate topographical and even of geographical information. It may be said that for most of the states the information requisite for constructing a true plan, on even so small a scale as ten miles to the inch does not exist. In the attempt to put together the local plans of townships and counties, irreconcilable differences, often amounting to several miles, are encountered. The accepted geographical positions of capitals are found greatly in error when determined by accurate means. A river boundary such as the Ohio when drawn from the data available for one state will differ widely from that constructed for an adjoining one. The commonwealth of Massachusetts forms the only exception to this state of things, having forty years since provided a trigonometrical framework for its territory. The topography, however, was compiled from the town plots, and the State map is, therefore, very deficient in its surface representation. A movement is now making to supply an adequate topographical representation.

The so-called survey of our public lands is a mere parcelling out of properties for sale, and from the nature of the methods employed, cannot form the basis of a correct map, but it must be admitted, answers well and cheaply the primary purpose of dividing up the land for occupancy.

It is to the extension of our Coast Survey into a trigonometrical survey of the whole country that we must look for the basis of a correct and complete map of the United States. Whatever measure of detail may be left to the choice of the several states, uniformity of plan, unity of precision, equal participation in advantages obtained at the common charge, — in short, national unity, demands that the general framework of our country's map should be made by a national organization. We have seen that all civilized nations have found it to their advantage to possess an accurate map of their domain; we certainly cannot long remain behind. Our territory is so extensive, so unequally settled, so varied in economic value, as to require the most careful adaptation of method in different localities. This can only be done by entrusting the whole matter to an organization especially provided for the purpose, and composed of persons trained to that particular duty.

I may freely assume that the Association is well acquainted with the geodesic and geographical work in progress in this country — not only that of the Coast Survey, with its extension across the continent, to connect the two oceans and the authorized spreading over those states that provide for their topographical or geological surveys, and its younger sister, the survey of the Great Lakes, which is conducted on the same scientific principles, although under a different administration,— but also with other works of a similar character, but in which exactness is a less dominant feature, such as the geological survey of California, with its admirable general topography, with the explorations of the fortieth parallel, the geological examinations of the territories and the military explorations of the same.

I need not detain you by endeavoring to assign to each its specific value; suffice it to say that they are all timely and well done according to their requirements, and give the information immediately required for the development of the country.

You will, however, be pleased to have some account of the effect of a recommendation made to Congress by this Association, many years ago, in regard to the extension of the triangulation made for the survey of the coast into the interior of the country, as a basis of more accurate maps than we now possess. Twenty years ago it was not unusual for the members of the Association to ask at the hands of their elder brethren reports on special subjects, setting forth the actual condition of our knowledge and defining the questions next before us — a custom which it would be well to revive.

A committee of twenty members, appointed in that way eighteen years ago to report on the progress of the Coast Survey, recommended the continuation of the primary triangulation along the Appalachian mountain chain, from Maryland to Alabama, and thence to the Gulf coast, not only as a proper part of the survey of the coast, necessary to rectify the innumerable small steps of the circuitous survey along the low southern coasts by a direct chord of great strength, but also as affording to the states traversed by that main chain of triangles the basis for exact local surveys.

The importance of this recommendation being recognized by the Government, the work was undertaken soon after the war, and is now well advanced. Moreover, the eminent geometer to whose lot it fell to carry those views into effect, also obtained the assent of Congress to a system of trans-continental triangulation, connecting the surveys of the Atlantic and Pacific coasts, and furnishing to the intermediate states the advantages of a trigonometrical back-bone for their state surveys.

Let us all use our influence to have this plan steadily pursued, and we may feel assured that in due time the great American nation will have a map of its domain not inferior to any in the world.

Science demands more especially the contribution of the large arcs, both in latitude and longitude, which it is our province to measure. The Coast Survey has incidentally added its share, and is even now nearly ready to furnish much more, but we must not anticipate.

The latest general combination of the principal meridional arcs heretofore measured, with a view to the determination of the figure of the earth, has been made by Clarke, of the British Ordnance Survey. His computation, published in 1866, does not comprise the results of American measures, which were only published in 1868; but these so closely agree with the general result that their introduction would not materially modify the elements. It is based, like all previous discussions, upon the supposition that the uncertainty of the measured lengths of the arcs is extremely small, compared with that of their amplitudes or differences of latitude; since the latitudes are affected by the irregularities of local attraction to an amount generally between one and two seconds of an arc, attaining in mountainous regions even ten seconds. Stations exhibiting extraordinary discrepancies in latitude are, of course, excluded from the discussion, which was preceded by a minute comparison of all the standards of length that had served in the several operations. The following arcs, entitled to equal consideration by their superior precision, have entered into the comparison: 1. The French arc, from Formentera (lat. 38° 40′) to Dunkirk (lat. 51° 02′), having an amplitude of 12° 22′, and comprising six latitude-stations. 2. The British arc, from Greenwich (lat. 51° 28′) to Saxavord (lat. 60° 49′), amplitude 9° 21′, with six latitude-stations. 3. The Indian arc, between Punnae (lat. 8° 10′) and Koliana (lat. 29° 31′), amplitude 21° 21′, with eight latitude-stations. 4. The Russian arc, from Staro Nekrassowka (lat. 45° 20′) to Fuglenoess (lat. 70° 40′), amplitude 25° 20′; thirteen latitude-stations. 5. The Cape of Good Hope arc, from North End (lat. 29° 44′) to Cape Point (lat. 34° 21′), amplitude 4° 37′; five latitude-stations. 6. The Peruvian arc, from N. lat. 0° 02′ to S. lat. 3° 4′, amplitude 3° 06′; two latitude-stations. These six groups, aggregating an arc of over 78°, and comprising forty latitude-stations, when treated with reference to a spheroid of revolution, yield the following results:

Equatorial semi-axis	= 20,926,060 feet	= 6,378,206 metres.	
Polar semi-axis	= 20,855,120 "	= 6,356,584 "	
Ellipticity	= 1 : 295.		

When the latitudes of the several stations are computed from the mean of each arc upon these elements, the difference between the computed and observed latitudes is on the average 1″·8, a degree of discordance fairly ascribable to local deviations of the plumb-line. A quadrant of the meridian of the above spheroid is equal to 10,001,887 metres, showing that the metre falls short of its presumed value by its 1–5300th part, equivalent to about one foot in a mile.

The same data, treated with reference to an ellipsoid of three axes, indicate an ellipticity of the equator of 1 : 3270, while the average error of the latitudes is reduced to $1''\cdot4$. This better representation of the observations by the assumed figure corresponds, however, only to the introduction of three, instead of two, unknown quantities into the problem, and furnishes no evidence that there is really a general ellipticity of the equator. Such a conclusion could only be reached by the evidence of many more arcs measured near the equator, in different longitudes, than we now possess.

Since the U. S. Coast Survey arcs, from Mt. Blue in Maine to Nantucket (1868), and from the head of Chesapeake Bay to Ocracoke, N. C. (1875), comprising 8° and swelling the aggregate to 84° of latitude, accord closely with the elements of Clarke's spheroid (1866), it appears that this figure may be taken as the most probable that can be deduced from geodesic measurements published at the present time; nor is it likely that they will be materially changed by the operations now in progress in Central Europe. It is only when larger arcs shall have been measured in North and South America, in Siberia, Africa and Australia, that we may look for a more accurate knowledge of the figure of our globe.

Such exact determination of the earth's form and magnitude has an interest quite apart from the problem of perfectly mapping its surface. Its equatorial diameter is the unit of measure for all distances in our planetary system, by means of which we are enabled to compare them with distances appreciable to our perception. By ascertaining the fact that its actual figure is that of equilibrium of a fluid body rotating at the same rate, we establish a degree of probability, almost amounting to demonstration, that it once was in a fluid state, and thus obtain a sound link in the chain of evidence for our received conception of the probable history of our solar system. But it is mainly in looking forward to the probable continued existence of mankind through time comparable in duration with what is called geological time, that we find a just incentive to extreme accuracy in our geodesic work. If in that remote future it should be possible to determine, by a repetition of our operations, what changes the dimensions of the earth have undergone during the intervening time, how appreciative will be the man of that day of the endeavors of our age, which he will look upon as lying at the very beginning of exact knowledge.

The permanence of the earth's magnitude involves not only our measure of space, but also that of time, since the revolution of the earth about its axis is our only available unit, and a contraction of its dimensions would inevitably cause an increased rate of rotation.

We cannot presume that our several individual achievements, however marked in our day, will remain distinguishable after a million of years in the report of the aggregate advance of the chiliad in which we have lived. Eminence in political history and in literature is surely doomed to oblivion, because the interests involved in the one, and the modes of thought and habits of life portrayed in the other are transient. If any names are destined to go down to such remote posterity it must be those of the fortunate ones who have been enabled to announce to mankind the discovery of great laws of nature, that endure forever. Among the most permanent monuments of science is the work of Geodesy. The interests of an intelligent race of inhabitants of our globe in its cosmic history can never fail. Well ascertained facts relative to the size and form of the earth, from the remotest epochs, will ever retain their importance. I desire to proclaim as loudly as I can, that there is no monument so enduring that could be erected for himself by one of our great money-princes, as the establishment of a Geodesic fund for the measurement of extensive arcs of meridians and parallels. Deservedly called by the name of the Founder, his fame would go down to the remotest generations of man.

Now, before taking leave of you, I will permit myself to say a few words, to enforce what I have sought to imply in my introductory remarks, on the part which abstract thought and pure mathematics have had, and necessarily must have, in the development of human knowledge. In doing this I desire particularly to recall Prof. Newton's plea for a more extended study of "the science that draws necessary conclusions," as Peirce has defined it. It is the science that develops the absolute modes of thought, and all other sciences must be subject to it, whenever they become sufficiently advanced to undertake to account for cause and effect in measure as well as in mode. In physical science, in chemistry, the work that now remains to be done in order that we may understand the constitution of the material elements, and predict the result of experiments, is almost purely mathematical. Here lies the golden key that will unlock the mystery of the microcosm. It is not given to every one to pursue with success the study of abstract science—*non omnibus licet adire Corinthum:*—but let every student who loves mathematics and to whom the simpler forms of mathematical thought have come as by intuition, without effort and with a sense of great delight—this is the test of the gift—let him believe that there is a great prize within his reach and devote himself to pure science.

How much remains to be done! If we are to interpret the qualities of molecules, as arising from the combined effect of numberless atoms moving in orbits constrained by mutual influences—how must we despair of any exact interpretation of the microcosm when we reflect that we have not yet sufficiently developed our modes of thought to enable us to take into account in the macrocosm of the heavens the mutual interaction of more than two bodies, except by processes of approximation unsatisfactory to the mind and impossible when extended to many bodies. But shall we allow ourselves to be discouraged and say: nay, this is too troublesome, let it go; matter is very well as it is and it is useless to inquire how it came to be so; we will eat and drink and be merry, for to-morrow we die? Never! We want to know!! The Promethean fire is deathless; to seek to know the causes of things—*rerum cognoscere causas*—is a Divine quality of our nature that we cannot divest ourselves of, fraught though it be with unrequited labor and pains. WE WANT TO KNOW! and as our revered President told us at the opening of the session, with the retrospect upon a long life, in which wisdom has been the fruit of much seeking to know, *there is no satiety of knowledge.* Friends, to my mind this insatiate desire to know the causes of things, and to understand the ways of creation, is the strongest evidence that there is a Divine mind from which the human mind is derived, and that man *has* been made in the image of God.

REPORT OF THE COMMITTEE ON WEIGHTS, MEASURES AND COINAGE.

In conformity with the design of its institution, which was to bring to the notice of the Association, from time to time, such subjects as seem to call for their action or the expression of their opinions, the Committee on Weights, Measures and Coinage beg leave first, to submit a statement of the progress made in the effort to secure the participation of the government of the United States in the establishment of an International Bureau of Weights and Measures; and secondly, to invite attention to certain questions of grave importance now agitating the public mind, concerning the principles which should govern legislation in regard to standards of value, in the decision of which, in the view of the Committee, are largely and directly involved the material interests and welfare of the people of the United States, and indirectly those of the whole civilized world.

First, as to the International Bureau of Weights and Measures. In the report of this Committee presented to the Association at the annual meeting of 1875, held at Detroit, the fact of the then recent negotiation of an international convention, to which the Government of the United States had been an assenting party, for the establishment and permanent maintenance of an institution for the preparation and verification of standards of measurement, and the preservation unaltered forever of the prototypes from which such standards are derived, was stated, accompanied by a resolution that a memorial to Congress be prepared for signature by members of the Association and others, praying the Senate to ratify the action of the Executive Department in entering into the Convention, and praying Congress to make provision to discharge the obligations resting upon us in consequence of our adhesion to such convention, which resolution was unanimously adopted.

In compliance with the requisitions of this resolution, the proposed memorial to Congress was prepared without delay, and was circulated as widely as circumstances and the limitation of time would allow. The professional engagements and absorbing occupations of most of the members of the Committee prevented as extensive a personal canvass as could be desired; but it is highly gratifying to the Committee to be able to state that, in nearly every quarter in which the memorial was presented for signatures, it met with cordial approval and was signed without hesitation.

As it seemed to be desirable that the presentation of the memorial to Congress should take place as early as possible after the opening of the session in December, the effort to obtain signatures was not persisted in later than that date. Up to that time, however, there had been obtained more than eleven hundred names, a number already large, in which nevertheless the individual weight is more strikingly significant than the aggregate total. Copies of the memorial with these signatures attached have been printed for distribution. It will be seen that among the signers twenty states are represented, and a large proportion of the names are easily recognizable as those of the most prominent and influential citizens of the communities to which they respectively belong. Especially noticeable is the fact that the educators of the country, whether engaged in literary, scientific or professional institutions of learning, have largely contributed to swell this list. Among this class, the sentiment has been found to be almost universally one not merely of passive approval, but of lively interest in the object of the memorial. Next to this may be mentioned the engineers, architects, and leading manufacturers of our principal towns, men whose daily occupations impress them continually with the importance of exactness in the measurement of quantity, and with the necessity of accurate standards of weights and measures, in order that such exactness may be secured. By examining the list of these names it will be perceived that this intelligent class of our fellow citizens distinctly recognize the fact that the object of the International Bureau is one of far higher importance and dignity than that of promoting the advancement of any particular system of metrology, since there will be found among them the names of well-known men who have publicly expressed their unwillingness to see the metric system of weights and measures made compulsory in the United States.

Another class of great weight in the body politic, represented in this list, is made up of the bankers, merchants and capitalists of our great commercial centres. These, in every instance in which the expression of their sympathy has been solicited, have, without hesitation, added their names to the memorial. The signatures of a single city belonging to this class, represent an aggregate of more than a hundred millions of dollars.

It will thus be seen that among the signers of the memorial are represented largely both the intelligence and the wealth of the country. And the readiness with which these names have been given affords a convincing evidence that the sentiment of the classes, to which they severally belong is everywhere, with a near approach to absolute unanimity, favorable to the objects of the memorial.

The ratification of the convention establishing the bureau, should it be ratified on the part of our government, will entail upon the United States a certain but quite insignificant expense, first as a contribution toward the creation and equipment of suitable buildings, and secondly as an annual subsidy for the permanent maintenance of the institution. The amount is too trivial to be for a moment considered, in view of the inappreciable importance of the objects to be secured. But were it much greater, it is quite evident that the people, by whom this expense is to be ultimately borne, are entirely content, not to say desirous, to assume this inconsiderable burthen.

On its presentation to Congress, the memorial of the Association was referred in the House of Representatives to the Committee on Appropriations, and in the Senate to that on Foreign Relations. In compliance with the invitation of this latter Committee, certain members of the Committee of the Association appeared before that body, and made statements more fully explanatory of the nature and design of the proposed Bureau, the impression left being apparently favorable. The Senate Committee nevertheless delayed its report on this subject, presumedly waiting for the action of the Committee of the House on the question of appropriation, while the House seemed similarly to await the decision of the Senate on the question of ratification; and thus the session of Congress became at length so far advanced that it seemed inadvisable to urge a report liable to be exposed to the hazards of hasty and inconsiderate action in the closing hours of a protracted session. By the advice of the friends of the measure in the Committee of the Senate, therefore, the subject was permitted to lie over until after the assembling of Congress in December next, when it will come up among the unfinished business.

In the mean time, however, it is greatly to be desired that the members of the Association in all parts of the country should bring to bear all the means of influence which they possess to impress the popular mind with the importance of the proposed object, and to secure the expression of popular opinion in regard to it, in modes which may tend to promote its success. Much may be done in this respect, after the excitement of the approaching or actual political struggle shall have passed away, and men's minds, ceasing to be occupied with matters of temporary concern, shall be capable of contemplating calmly those of more permanent interest, by a judicious use of the press. The conductors of our public journals may be counted on, almost to a man, as favorable to the objects of the memorial, and many of them, if the subject is properly brought to their attention, will undoubtedly lend to the measure the support of their editorial columns. All will, without question, give publication to properly written communications regarding it; and thus the friends of the measure may, by means of a little effort, diffuse valuable information in quarters where it is needed, and awaken an active interest in favor of the movement, where, for no reason but the lack of information, such interest has not hitherto been felt.

The assent of the government of the United States to the convention establishing the International Bureau of Weights and Measures is not necessary to the existence of that institution. The International Bureau already exists. By the adhesion to the convention of the other signatory powers, it has been established on a permanent footing; and all the benefits to the world, to our own people among the rest, which it was designed and which it is certain to secure, have been provided for beyond the possibility of failure. The only question for us now to settle is whether, in a measure which will be regarded by all future generations as one of the wisest ever adopted by the common consent of peoples, a measure which marks perhaps more signally than any other the advanced civilization of the nineteenth century, the people of the United States shall be participators or not. It is no longer a question of public economy or of public policy merely, which the fate of this measure before our Congress is to decide; it is rather a question of national character and of the repute we are to leave behind us for national enlightenment.

Since the presentation of the report of this Committee made to the Association at the Detroit meeting, facts have been ascertained at the Bureau of Weights and Measures of the United States in Washington, which add materially to the weight of the considerations to which the project of the International Bureau was originally owing. It has become manifest that solid measures of length, heretofore prepared with the utmost care, and regarded as perfect and invariable, have undergone, after not a very extended period of years, changes sufficiently great to destroy their character as standards, and to render it necessary to resort to new and laborious comparisons to determine their errors. Concerning this matter, and others connected with this subject, the actual head of this Bureau, Mr. Hilgard, a member of this Committee, will make a statement supplementary to this report, which will be found to possess a novel and very important interest.

The subject to which, in the second place, the Committee feel it their duty to call the attention of the Association, is the confusion at present prevalent in the public mind in regard to the principles which should govern the creation by law of standards of value. In many instances the opinions professed on this subject appear to have been formed on the basis of a belief that, being popular, they may be made instrumental in securing some partisan political advantage; and not from any profound study of the principles which must control the results of the policy adopted, and which, being inexorably fixed in the nature of things, no legislation can successfully set at defiance. With the partisan aspects of the subject this Committee have nothing to do. They can only deplore that, in a matter so vitally important to the prosperity of communities or individuals, any consideration of momentary interest should be powerful enough to obscure truth, or to predispose men to receive the truth unwillingly.

Money was originally introduced to facilitate the transactions of commerce. Commerce may be comprehensively defined as the exchange of equivalent values. In its earliest stages, commerce took the form of barter. In this form it was embarrassed by the infrequency with which individuals, desirous of effecting exchanges, found themselves in possession of commodities reciprocally acceptable. In order to remove this embarrassment, the expedient was devised of employing a material of well-known and universally recognized value to serve as a medium of exchanges, and as a measure of the value of all exchangeable commodities. Such a material was found in the precious metals, and hence, from a very early antiquity, gold and silver have constituted the medium of commercial exchanges among all nations. From the nature of the exigency which called it into existence, and from that of the function which it continues to fulfil, it follows that money, in order to be capable of serving as a means of exchanging values, must possess a positive value itself. On no other supposition can it properly be called a standard of value. The term standard implies comparison, and there can be no comparison between objects which do not possess a common quality. It is usual to apply the term money to the promissory notes of banking institutions, or to those of the government; but this is a perversion of language, since such notes are not money but only the representatives of money, and they are accepted as the equivalents of money only on the faith that the money they represent shall itself be forthcoming whenever demanded. When such conversion is not promptly possible, or when the period at which it may be effected is remote or doubtful, these bills of credit will only be receivable in commercial transactions at a discount which is understood to measure the disadvantage of the delay or the hazard implied in the doubt. There is no power in legislation to make the promise of a dollar the equivalent of a dollar, so long as such promise is not invariably fulfilled on demand, or so long as the time or the fact of the fulfilment remains in the slightest degree uncertain. The law which makes the greenback notes of the government a legal tender in all payments except those of the customs-duties, fails signally to accomplish its object; and it has done so in degrees varying between very wide limits, for the past twelve or fourteen years. This law enacts that the government promise to pay a dollar shall be a legal tender for the value of a dollar. But as the value of a dollar is only ascertainable by reference to the law creating the coin of that name, and as such a coin will purchase in any market a larger amount of any commodity than its legal tender representative, it is obvious that, while the letter of the law is observed, its intent is effectually evaded. The seller who has commodities to dispose of, indemnifies himself for the depreciation of the currency by marking up his prices. A standard of value must therefore be intrinsically valuable. A perfect standard, if such a one could be found, should not only be intrinsically valuable, but invariable in value, through successive years or centuries. And the usefulness of any medium of exchanges will be greater, in proportion as it is capable of representing a larger value under a given bulk or weight. No substance has been found possessing the desirable property of a value absolutely invaluable. This property however, belongs to the precious metals in so high a degree as to have secured for them the preference over all other substances for the purposes of money. Their variations of value have hitherto been so inconsiderable and so slow, as to have produced no perceptible effect upon the transactions of business. But the fact that these slight changes have not usually been in the same direction for both metals simultaneously, and that consequently the relative as well as the absolute value of the two have been subject to gradual fluctuations, has introduced frequent and sometimes serious disturbances into the monetary systems of those nations in which both have been made by law equally standards of value and the material of coinage legal tender for all amounts.

A coinage founded on what is called a double standard, must necessarily assume a fixed relation of value between the two standard metals, weight for weight, or a definite relation of weight, value for value. This relation is naturally determined by the ruling ratio at which the metals in the form of bullion may be purchased in the market at the time of institution of the system. So long as these rates remain unaltered, the coins struck from both metals circulate with equal freedom, and no pecuniary advantage can be gained by the payment of a debt in one description of these coins rather than in the other. But this condition of things is never lasting. Every variation of the market rates introduces a new relation of actual value between the metals, and generally a relation differing from that established by law; so that it will be found almost invariably that, at any given time, a given gold coin will purchase more or less silver than the amount of that metal contained in the silver coins which are at the same time its legal equivalents. If it will purchase more, then the coin-silver is overvalued, and it will be a paying transaction to exchange silver coins for gold, and to export the latter or melt them up and sell them as bullion. If it will purchase less, the coin-silver is undervalued, and it will pay to reverse the operation just described and buy up, for the purpose of melting, or export, silver coins with gold.

As in this state of things the debtor will always elect to satisfy his obligations in the coinage of the relatively cheaper metal, it follows that wherever a double standard exists in law, a single standard inevitably rules in point of fact; and this ruling standard is that of the metal in which the coins of a given name represent the smallest actual value. The consequence is, that the coins of the undervalued metal disappear; nor can any activity of the mints maintain them in circulation; since being at a premium with the practical standard, they are to all intents and purposes demonetized and converted into bullion. Thus the effect of a double standard is, singularly enough, to limit the coinage of the nation adopting it to a single metal. This may not, it is true, be always the same metal; for in the gradual fluctuations which are continually going on in the relations of value between gold and silver, it may happen that either of these metals may be alternately at a premium when compared with the other as a standard. In the experience of the world, hitherto, such alternations have not often occurred except after considerable intervals of time. In case of such an occurrence, it of course happens, that during the period of transition there is a moment in which the double standard is a reality; but this condition has the character of an unstable equilibrium which cannot endure. Ordinarily, governments have not waited for the turning of the scale through the operation of natural causes. In innumerable instances the attempt has been made to restore the lost equilibrium by altering the relations of weight between the coins of the different metals; and in making these alterations it has been unfortunately true that the more valuable coin has invariably been reduced in weight, and not that the less valuable has been increased. An illustration of this tendency in the history of our own coinage may not be beyond the recollection of some of the members of this Association. Early in this century, and down to the year 1834, a gold coin of the United States was an object rarely seen; so rarely, indeed, that collectors sometimes treasured up those coins in their cabinets as curiosities. This was in consequence of the coinage act of 1792, which fixed the relative values of the two metals in the ratio of one to fifteen. These numbers express a relation which may have been approximately true at the date of the act, but very shortly ceased to be so, the market ratio between the metals becoming soon after more nearly one to fifteen and a half. In this state of things, gold coins legally representing the nominal value of fifteen dollars melted down into bullion, would sell in the market for fifteen and a half legal tender silver dollars, and gold totally vanished from our circulation. At length, in the year above referred to, 1834, Congress undertook to restore the disturbed equilibrium, and this object it accomplished, not by raising the value of the silver coins, but by depressing that of the gold. The weight of these was reduced so as to make it almost exactly one-sixteenth that of the legally equivalent coins of the less valuable metal. The object, as just observed, was accomplished. In point of fact it was somewhat more than accomplished, for the effect of the new statute which brought gold into circulation, was at the same time to some extent to undervalue silver. Silver coin therefore ceased in turn to circulate, except in the form of fractional currency.

In France the legal relation of value between gold and silver has been, ever since the establishment of the metric coinage, as one to fifteen and a half. The market ratio has fluctuated in the mean time (down to 1876), between the limits, one to fifteen and one to sixteen, without ever quite reaching either. The consequence has been that both metals have been alternately at a premium when either is compared with the other. From about 1852 to 1870, the market ratio was about one to fifteen and three-eighths. Silver accordingly almost ceased to circulate. More recently the ratio has passed the point of equilibrium, and the balance has turned the other way; there is a tendency of silver to circulate to the exclusion of gold. But this tendency has been wisely counteracted by the government of that country partly by retiring the silver, and partly by putting a limitation on the amount of the future coinage.

Examples of this description might be multiplied almost indefinitely. They serve as practical illustrations of the truth, already sufficiently evident from abstract considerations, that the notion of a double standard in coinage is a fallacy which experience most inevitably contradicts; that practically at any given time, a single standard of value is only possible, and that, wherever, in the contemplation of the law, there are two such standards in existence, the actual standard will always be that which is furnished by the metal most cheaply purchasable in the market. It is further apparent that wherever, as in France from 1852 to 1870, and in the United States during the forty years from 1834 to 1874, silver is the metal which is undervalued, it will be found practically impossible on the double standard system, to maintain in circulation a sufficient amount of coin of small denominations to subserve the every-day wants of society in the petty transactions of trade. This difficulty can only be effectually overcome by creating a silver coinage legal tender only for limited amounts, in which the weight of metal shall be so reduced as to make its market value less than its legal. Such a coinage may ideally coexist with another of the same metal having the full proportion of weight required by law to make it legal tender for all amounts; although this latter, in consequence of undervaluation, is practically excluded from circulation. Such a condition of things was created in France, Belgium, Switzerland and Italy by the quadripartite treaty entered into between those powers in December, 1864, by which provision was made for the issue of silver coins below the denomination of five francs, of debased standard, and legal tender only to the amount of fifty francs, while the outstanding silver coins of full standard continued to be legal tender for all amounts. Such also was the case in the United States from 1853 to 1874, during which period the silver dollar continued to be legal tender for all amounts, while the smaller coins were degraded in weight, and were, as they still continue to be, legal tender only to the value of five dollars. From 1834 to 1873 the silver dollar was coined in our mints only in small quantities, and was rarely seen in circulation. In the year last named its further coinage was prohibited, and in the revision of the statutes, completed during the following year, the dollars still outstanding were deprived of their legal tender character, except to the same extent as the minor coins of the same metal.

This plan of providing a subsidiary silver coinage to subserve the minor transactions of daily commerce was first introduced in the year 1816, in Great Britain, and since that time has been extensively adopted on the continent of Europe, as well as in our own country.

From what has been said, it may fairly be inferred that the principle of a double standard of value in monetary systems is objectionable, both because it is practically fallacious, and because its inevitable tendency is to degrade the value of the coinage in the countries in which it prevails. These truths have in recent years been so generally recognized by the ablest financiers of all countries, that in every one of the now somewhat numerous international conferences which have been called to consider questions relating to coinage within the last quarter of a century, there has been a decided expression of opinion in favor of a single standard, and of adopting gold to serve as that standard universally. Already in many countries large steps have been taken toward the realization of this result. In some, in which the silver standard has not been absolutely abolished, it has been practically so by the introduction of a subsidiary coinage, and by stringent limitations upon the further coinage of legal tender silver. This is true of Germany, Holland and the Scandinavian countries, and also of the parties to the quadripartite treaty of 1864. France, indeed, though naturally clinging to the silver standard, in consequence of having originally connected her monetary system with her system of weights and measures by making the legal tender silver franc her money unit, and giving to it a metric weight, went so far, through her commissioners to the monetary conference held in 1869 at the Hague, as to signify her willingness to adhere to the gold standard exclusively, provided the other nations represented would adopt as their unit the weight of the five franc piece of gold.

Within the past year or two there has been a very large and hitherto wholly unprecedented fluctuation in the market price of silver. This price has varied, within a period of a few months, through a range of not less than twenty per cent. of its original value. From its lowest depression it has partially recovered, and it now stands about midway between the extreme limits of its variation. The causes of this singular phenomenon have been variously assigned. It is not necessary to speculate on them here. But the fact of its occurrence, and the uncertainty how long this instability is to continue, are considerations which must be duly weighed, when the proposition is presented to us to restore this metal to the place in our monetary system from which it has been deliberately and for good reason by law excluded. No quality is more absolutely indispensable in a standard than stability of value. It is to the possession of this quality in an eminent degree during the past centuries, that silver has owed the place it has so long occupied in the monetary systems of the world. With the loss of this quality, it has lost its claim any longer to hold that place;

and to make it once more, until at least the lapse of a long period of years shall have shown (what is very improbable) that the present disturbance is but a momentary accident not likely to be repeated, the material of coinage legal tender for all amounts, would be to disconcert all the calculations of commerce, derange all the operations of business, and entail upon the country wide-spread disaster reaching every class of society.

The committee have no design to exhaust the argument upon the subject under consideration. That would require an examination of the causes which have operated to affect the price of silver, the probability of their permanence, and the degree to which their action in the future is likely to be regular or irregular. To pursue these inquiries would be to distract attention from the single point to which it is the desire of the committee to draw the attention of the Association at the present time. They reserve to themselves the privilege of going more fully into the subject at a future meeting, should not the questions which it involves, so deeply concerning the credit of the government and the general welfare of the people, be in the meantime satisfactorily disposed of.

One point, however, connected with this last aspect of the subject, should not be overlooked even in this cursory view. It has been said above that the discount on bills of credit, the liquidation of which is distant or doubtful, measures the inconvenience of the delay, or the hazard of the ultimate loss. If such a bill were immediately convertible into the coin it promises, there would be no discount. It would be accepted in business transactions for precisely that amount of coin; and, because of its greater portability, would in general be preferred to coin. The greenback bills of credit of the Government, if now redeemable on demand, would be worth one hundred cents to the dollar. The uncertainty in the centres of commerce as to the time when they shall be redeemed, or as to the question whether they shall ever be redeemed at all, is represented at present by a fluctuating discount of from ten to twelve per cent.; so that the greenback dollar is only accepted in the affairs of commerce for an amount varying from eighty-eight to ninety cents. Should the silver dollar of 412½ grains of standard metal be made legal tender for all amounts, its value at present would be about the same as that of the greenback dollar at present — say ninety cents — also. Then, if the greenback should be made immediately redeemable, its purchasing power would remain unaltered, or it would continue to be worth in the market ninety cents. But if it should not be immediately redeemable—if it should continue to be subject to the same uncertainty as at present in regard to the time of its redemption or the question of its ultimate redemption at all, its purchasing power would fall below that of the silver dollar, by as large a percentage as it is now below that of the gold dollar; and it would actually be worth as money only from about seventy-eight to eighty cents. Moreover, should silver in the continual oscillations of its value, touch once more the point it reached no longer ago than July last, the silver dollar would itself be worth but eighty cents, and the greenback dollar would fall to seventy.

The effect of this sudden degradation in the value of the only currency available to our people at the present time, upon the immediate means of subsistence of all who labor for wages, or are dependent on fixed salaries for support, need hardly be pointed out. The income of every such person would be reduced by an amount which, should silver again descend to and remain at the minimum level of the past summer, may be as great as twenty per cent., and which certainly, should it continue at its present price, must be not less than ten. On the other hand the holders of merchandize of every description, including the necessary supplies of life, in order to protect themselves against loss, must raise their prices; and this, in view of the instability of the standard, and the hazard attending possible change, they would be likely to do in a greater proportion than that in which the currency is degraded; so that every man in the country who depends upon a fixed income, would find himself doubly straitened—by the diminution of his resources on the one hand, and by the disproportionate increase in the cost of all the necessaries of life on the other.

In conclusion, the Committee would only add, that the existence of a double standard in coinage, at any time and anywhere, has been a consequence of a provision of nature quite accidental, according to which two metals, and only two, possess the properties which fit them, or have heretofore fitted them, both to be standards of value. Had there been three or four or five metals possessing equally the same properties, there would, beyond question, have been a triple, a quadruple or a quintuple standard. But, in the state of things which would have existed on such a supposition, the evils consequent on employing more standards than one would, unquestionably, have been recognized at a much earlier period in the history of the human race than they have been in fact, and the nineteenth century would not have been vexed with the questions on this subject which keep the thinking men of our country in a state of so constant anxiety to-day.

With these remarks the undersigned respectfully submit the preamble and resolution appended to this report, and ask for them the favorable consideration of the Association.

F. A. P. BARNARD, Chairman,
JOSEPH HENRY,
J. E. HILGARD,
WILLIAM B. ROGERS,
E. B. ELLIOTT,
H. A. NEWTON,
J. LAWRENCE SMITH.

} *Committee.*

PREAMBLE AND RESOLUTION.

Whereas, gold and silver, like other gifts of nature or products of human industry, are subject to gradual changes of value; and no relation of values which may be by law established between them can permanently conform to the actual relation existing in the markets of the world; and

Whereas, coins of these two metals cannot for any length of time be maintained in circulation side by side, when the coinage of both is equally free from limitation, and both are made legal tender in payment for all amounts; and

Whereas, it is historically true that legislation, in the endeavor to restore the disturbed equilibrium between the standard metals, has tended invariably to depress the grade of the coinage in one or the other and ultimately in both; and

Whereas, it is the aim of enlightened governments everywhere at the present day, whether by the absolute abolition of the double standard or not, to make gold for all practical purposes the single standard, and to employ silver chiefly or wholly as the material of a subsidiary coinage, legal tender only to limited amounts; and

Whereas, the United States after an experience of nearly a century in the effort to maintain a double standard, during the earlier portion of which, under the operation of the inexorable laws governing exchanges, gold was completely banished from circulation, while during the later, silver of legal tender grade has shared the same fate; and after the complete disappearance of the silver dollar from business transactions for more than forty years, have at length, by an act of deliberate and wise legislation, extricated the country from this long standing evil and adopted gold as the sole standard of value in their monetary system;

Therefore

Resolved, as the sense of this Association, that the single standard of value in our system of coinage, now fully established by law, ought to be maintained, and that every proposition to restore to silver the legal tender character for payments to all amounts ought to be discouraged.

On motion the report was received and referred to the Standing Committee.

Prof. Barnard moved that the title of his committee be changed to read "Permanent Committee on Weights, Measures and Coinage." Carried.

SECTION A,

MATHEMATICS, PHYSICS AND CHEMISTRY.

ADDRESS

OF

PROFESSOR CHARLES A. YOUNG,

VICE PRESIDENT, SECTION A.

FELLOW MEMBERS OF THE ASSOCIATION—

LADIES AND GENTLEMEN :—

I propose at this time, in fulfilling a duty which the honor you have conferred upon me imposes, to choose my subject in accordance with the suggestions of the season, and to speak briefly of the History of Astronomy in the United States during the past Century, its present condition, needs and prospects. I regret exceedingly that ill health, and the pressure of other imperative duties have prevented me from giving the time and research really necessary for the preparation of anything worthy of the subject, the occasion, and the audience, but such as I have I offer, with the hope that you will extend to me your kind consideration and indulgence.

Astronomy is the oldest, the most mature and beautiful of the whole family of Sciences—that immortal Sisterhood whose loveliness and vigor only increase with age and lapsing centuries, whose eternal youth is beyond the reach of all decay.

A century ago she was perhaps even more preëminent than now, for at that time geology and chemistry were mere infants, and the science of dynamical electricity had no existence. During the past hundred years she may, therefore, have lost something relatively by the rise and development of other branches, whose progress has outstripped her own.

Yet her own progress has been wonderful. A hundred years ago no planet had ever been discovered: the heavenly bodies known to science were only the same (excepting the satellites of Jupiter and part of those of Saturn) which had been observed in Egypt and Chaldea in prehistoric antiquity.

Observatories and observers were few and far between compared with their present numbers, and the instruments employed were far below those now used, in power and accuracy. Great things had indeed been accomplished by Galileo, Tycho and Kepler, by Newton and Halley and Bradley, Euler, Cassini and Clairaut, in the preceding two hundred years; but a new era of more intense activity was just opening. La Place and La Grange and Herschel had begun their work, and were to be followed by Gauss and Encke; Fraunhofer, Struve, Airy and the younger Herschel, by Adams and Leverrier and Pierce, Hansen and Delaunay and Newcomb and an uncounted multitude, who each have contributed something of importance to the fund of knowledge.

Since 1776 two great worlds, eight satellites, one hundred and sixty-six minor planets, some forty periodic comets, and the hosts of meteors have been added to the rolls of the Solar System. A century ago about one hundred double stars were all that were known. Since then some ten thousand have been discovered, their physical connection has been demonstrated and investigated, and the periods and orbits of many have been worked out. The number of nebulæ and clusters has been increased from less than a hundred to more than seven thousand, and their nature and physical constitution ascertained. The parallax of fixed stars has been measured, their distance determined, and their motions, and that of the Solar System, through space have been brought to light and to some extent investigated. The sun and stars have been analyzed in their chemical constitution, and a host of new and most interesting results have been reached by instruments and methods undreamed of a hundred years ago.

New and more precise methods of computation have supplanted the old. The accuracy of angular measurements, and the delicacy of graduation and reading, have been greatly increased. The electro-magnetic methods of recording time-observations have superseded the older fashions. Telescopes have been enormously improved — refractors almost beyond belief; for a hundred years ago not a single one had ever been made with an aperture exceeding four inches; and even fifty years ago the Dorpat refractor of nine inches aperture, was regarded as hardly less than a miracle of art. Reflectors of considerable power had indeed been built, but the great instruments of Herschel and Ramage were not constructed until later.

The polariscope and spectroscope, to which we owe so large a part of our limited knowledge in regard to the constitution of the heavenly bodies, have both been invented since 1800.

Altogether we think it may safely be asserted that in no equal period of human history, has astronomy progressed so rapidly, or so widely extended her domain.

To this great advance our own country has made her contributions, small perhaps, but real and creditable — not by any means so great as those of England, France, and Germany, nor so great perhaps as might fairly have been expected of a nation where education has been so widely diffused, and thought and action so free; but still enough to obtain for her a place and honorable mention in the history of science.

The only astronomical work of any importance done in this country before the revolution, was the measurement of the so-called Pennsylvania arc of the meridian, by Mason and Dixon in 1764; and the observations of the Transit of Venus in 1769, by a committee of the American Philosophical Society. Rittenhouse was the chairman of the committee, and the observations seem to have been among the best made anywhere, as the weather was perfect, and the skill and care of the astronomers concerned fully competent to the work.

During the first half century of our national existence, science of every kind was utterly neglected; astronomy especially seems to have been regarded almost with aversion by the popular mind, as something aristocratic and unrepublican. When Hassler in 1807 submitted to the Government the project for the Survey of the Coast, it contained a provision for the establishment of an observatory to supply the needed astronomical data; but the proposition received no favor. The original law authorizing the Survey omitted all mention of the subject, and the law of 1832 expressly prohibits any such establishment. In 1825 President J. Q. Adams in his first message to Congress suggested and recommended the founding of a National Observatory, and in very eloquent language pointed out its importance; but the recommendation was received with scorn and ridicule. In referring to the observatories of Europe, he had spoken of them as "light-houses of the skies" and the expression was seized upon and made for years a byword of reproach. The prohibition of an observatory in connection with the Coast Survey, undoubtedly arose from the partisan feeling which had been excited by President Adams' suggestion, and it was not until 1842 that the National Observatory could be established, in a semi-surreptitious manner, as a Depot for Charts and Instruments. The same narrow Philistinism is still rampant, though fortunately no longer regnant, in the halls of legislation, and on every opportunity manifests itself in opposing all measures for the encouragement of Art or Science. The debates on the appropriations for the Transit of Venus will be recollected by many as a case in point.

The only name of eminence before 1836 is that of Bowditch, who was born in 1773 and died in 1838. He was undoubtedly a mathematician of great ability. Professor Newcomb has said of him that "there was hardly a man living who was a more complete master of the celestial mechanics of his time," and again that considering his circumstances "we can hardly refuse him in genius a place alongside of Laplace and Hansen." At the same time Professor Newcomb also points out that his original investigations in no way correspond to what might reasonably have been expected from a man of such ability in Europe, where he would have been stimulated by intercourse with men of science. His reputation will probably always rest mainly upon his translation of Laplace's "Mecanique Celeste," a work beautifully done, but of course not calling for much originality—nor really of any great importance. His "American Navigator," which after running through almost innumerable editions, still remains a standard among all English speaking peoples, and even some others, has been of great value to the commercial marine of all nations, and deserves especial mention.

While, during the period of sixty years our country possessed none who could properly be called astronomers, the love of the science, and some knowledge of its state and progress was kept alive by the professors in our colleges and other institutions of learning. The great eclipse of 1806, the comet of 1811, and some minor eclipses served also as reminders, and prevented a complete dying out of all interest among the people.

About 1836 affairs began to mend. In that year the first observatory in the country was built by Professor Hopkins in connection with Williams College, and in the next year the second, by Professor Loomis at Western Reserve College in Ohio. Within the next ten years, *i.e.* between 1836 and 1846, the observatories of the Philadelphia High School, the West Point Military Academy, the Washington Naval Observatory, the observatories of Georgetown, Cincinnati, Cambridge, and Tuscaloosa, and the private observatories of Mr. Jackson near Philadelphia, and of Mr. Rutherfurd in New York, were all established. In 1836 the only telescope in the country of any power, was the so-called Clark telescope of Yale College, with an aperture of 5 inches and a focal length of 10 feet, mounted on a wooden frame, at the top of a steeple; in 1846 we had eight or ten instruments, of diameters varying from 15 inches to 6. During the next ten years the observatories of Amherst, Dartmouth, and Hamilton Colleges, of Michigan University, the Dudley Observatory at Albany, and a number of private establishments were founded, and the number and power of astronomical instruments in the country was largely increased. Comparatively few of these observatories it is true have ever borne any fruit, at least directly in the form of astronomical results: but indirectly, by exciting in the minds of students an interest in the science, they have all had an important influence, and some — I think I shall be guilty of no impropriety in naming especially those of Cambridge, Washington, Ann Arbor and Hamilton College — have given to the world results and discoveries of the greatest value. To the Coast Survey however, more than to any other single organization, American astronomy owes her development and triumphs; not so much indeed by the direct contributions of this organization, although these have been by no means insignificant, as by its encouragement of the highest forms of astronomical investigation and research. Under the direction of Prof. Bache and his successors the Coast Survey has always kept in alliance and coöperation, when not directly in its service, the best mathematical and astronomical talent of the country, and by its judicious subsidies, paid to individuals and observatories for astronomical work of various kinds, has, at a small expense, maintained in their service of the science many who otherwise would have been obliged to abandon it for more remunerative work.

The survey of the N. and N. W. Lakes, under the War Department and the various boundary commissions, have exerted a similar, though less extensive and powerful influence; and the "Nautical Almanac," established by law in 1849, but first published in 1852, has also been of great benefit in a similar manner.

The catalogue of American discoveries and contributions to astronomical science, although not very extensive still contains some items of considerable importance.

In 1848 Hyperion, the 7th satellite of Saturn (the 8th in order of discovery) was detected at Cambridge by Professor Bond. Curiously enough it was also independently discovered by Lassell in England just two days later.

In 1850 the same observer discovered the so-called dusky ring of Saturn, again anticipating an English astronomer (Mr. Dawes) by about a fortnight. It is only fair to mention, however, that the same appearances which led to the discovery, had been seen and described by Galle at Berlin in 1838, although he did not perceive their meaning. It is perhaps also proper to notice in this connection, that the observations of Bond upon the non-permanent divisions of the rings, with the mathematical investigations of Peirce upon the mechanical conditions involved, demonstrated the fact that they were not continuous sheets of any kind, either solid or liquid, but composed of discrete particles, some years before the classical researches of Maxwell on the same subject.

Ten comets have been first seen on this side of the water (six by Mr. H. P. Tuttle), and several hundreds of double stars have been added to our lists by the sharp eyes and indefatigable industry of Burnham, the Clarks and others. The detection of the companion of Sirius, by Alvan G. Clark with the 18-inch telescope, constructed by their firm for the Chicago Observatory, is perhaps the most celebrated of these discoveries, and was crowned with the Lalande Medal of the French Academy.

Our American observers have, however, been most preëminently successful in the discovery of asteroids; of 166 now known, 49 were discovered in this country. France follows us hard with 48, while Germany claims 40, England 19, and Italy and other countries 10. The first of the American asteroids was found by

Ferguson at Washington in 1854 (Eurynome, the 31st of the group), and in 1857 and 1860 he added two others. Pandora was found by Searle at Cambridge in 1858, and in 1861 and 1862 Tuttle, also at Cambridge, discovered his two. In 1861, Dr. Peters, at Hamilton College, discovered the first of twenty-five, for which he now stands sponsor, and in 1863, Professor Watson, at Ann Arbor, detected the first of his eighteen. The venerable Luther, of Düsseldorf, alone comes into competition with the two last named gentlemen, standing credited with twenty-one.

Two of the most important modern improvements in Practical Astronomy are of American origin, the Chronograph and the Zenith Telescope. The electro-magnetic method of recording time has worked something like a revolution, and diminished errors of observation at least fifty per cent. in all cases where it is applicable: it is now used at Greenwich and Berlin, indeed at all but a very few of the leading observatories of the world.

It seems to have been invented nearly simultaneously by several persons about 1848, the names of Walker, Locke, Saxton, Mitchell and Bond, all being prominent in its early history; but its complete development, and its application to the ascertainment of longitudes with a precision before undreamed of, is due mainly to the Coast Survey, whose officers plume themselves not a little, and very justly too, on their recent determination of the difference of longitude between Greenwich and Paris, and the rectification of an error amounting to nearly a whole second of time. It was found that the difference of longitude between Greenwich and Cambridge, as determined by the cable operations of 1866, and that between Cambridge and Paris resulting from the operations over the French cable in 1870, could not be harmonized with the received difference between Greenwich and Paris; so that it became necessary, in order to reconcile things, to reinvestigate and correct this latter quantity, which had already been determined by European astronomers of the highest reputation. The result makes it evident that the longitude work of our Coast Survey is unsurpassed by that of any other observers, and only equalled by those who have adopted their methods.

Hardly less important is the American Method (as it is called by many European astronomers) of determining latitude by means of the zenith telescope, or the ordinary transit instrument, fitted up and used in the manner first proposed and practiced by Professor Lyman of New Haven. The method seems to have been first suggested more than a century ago by Horrebow, an English astronomer. At that time however it was useless, being impracticable for want of suitable instruments, and star catalogues of the necessary accuracy, so that it was completely lost sight of until its introduction by Capt. Talcott in 1834, and its adoption and development by the Coast Survey. At present the only method which, for field work, can compete with it at all, is that of Prime Vertical Transits, so much used by the Germans and Russians. Probably in precision and the time required for observation, there is little to choose; but as regards the ease of observation and computation, the advantage is decidedly with the American method. As our star catalogues increase in comprehensiveness and accuracy the only objections to it are rapidly diminishing, and it grows more and more in favor. The parties of all nations depended upon it mainly for determining the latitudes of their Transit of Venus stations.

Nautical Astronomy also is indebted to America for its most important recent improvement. The "new navigation," as Villarceau calls it, which is being rapidly developed at present, and already, in the French Navy at least, has largely superseded the older fashions, is a mere expansion, as Villarceau himself distinctly recognizes, of the method of "Circles of Position" first published by Capt. T. H. Sumner of Boston, in 1843. It enables the navigator to utilize in determining his place, observations made at any time, as well as those taken when the sun is on the meridian, a matter of great importance in cloudy weather, and sometimes under other circumstances.

Our American contributions to astronomical literature have not been very extensive, but some of them are of value.

We have already mentioned Bowditch's translation of the "Mecanique Céleste," and his "American Navigator." We have, later, the work of Walker upon the orbit and theory of Neptune; of Pierce upon Neptune, upon the moon, the theory of Saturn's rings, and various other subjects; of Ferrel upon the tides; of Gould, Hall, Winlock, Safford, Stockwell and others upon various topics relating to the lunar, planetary and stellar theories; and above all the medal-crowned works of Newcomb upon the theories of Uranus and Neptune, which have won high honors for American astronomy.

We have Professor Loomis's "Practical Astronomy" (which is largely used in England as a text book), and the more elaborate and thoroughly admirable work of Chauvenet, undoubtedly the best book upon the subject of which it treats to be found in any language. Probably also the same may be truthfully said of Professor Watson's treatise on Theoretical Astronomy, which gathers into a systematic and accessible form what otherwise could be found only by long and wearisome searching through foreign journals of science.

We have the volumes issued annually by the National Observatory, containing valuable star catalogues and other papers of importance; among the most important of the catalogues, if one may speak proleptically, will undoubtedly be Burnham's Catalogue of double stars, now in press as a supplement to the volume for 1874. We have the Annals of the Observatory of Harvard College, including the magnificent monographs of Bond upon Donati's Comet, and the nebulæ of Andromeda and Orion; and in the Coast Survey reports we find numerous papers upon various topics of field astronomy. Our "Nautical Almanac," issued annually since 1851, compares favorably in every respect with the similar publications of other nations. With the exception of Gould's short-lived "Astronomical Journal," the "Astronomical Notices" of Brunnow, and the still more evanescent "Sidereal Messenger" of Mitchell, we have been without any astronomical periodicals. A considerable number of astronomical papers of more or less importance have however appeared in the publications of our scientific organizations, in the "American Journal of Science," and in foreign astronomical and scientific periodicals. The popular lectures and volumes of Professor Mitchell deserve recognition in this connection, for although of little strictly scientific value, they undoubtedly have had a powerful influence upon popular sentiment, in bringing astronomy into notice and general favor.

In the organization of expeditions for astronomical work the United States has of late years perhaps done as much as could reasonably be expected. The first of these expeditions was the Chilian, sent out in 1849, under the charge of Lieut. Gilliss, for the purpose of observing the opposition of Mars, and the inferior conjunction of Venus, in coöperation with our own observatories, thus furnishing data for a purely American determination of the Solar parallax. The expedition was successful, though it must be admitted that its results are of less weight in settling the final value of the parallax than was hoped. Perhaps its most important outcome was the arousing of an interest in astronomy among the Chilians, which led to the purchase of the instruments by their Government, and the founding of a permanent observatory, whose work, from the peculiar geographical position of the institution, is of especial value. In passing too we may note that the Argentine Observatory at Cordova, though not originating in precisely the same manner, is yet a daughter of ours, and while it remains under the direction of Dr. Gould, and his American assistants, we may fairly claim for American astronomy an interest and share in the laurels she is winning.

The next astronomical expedition of any importance was, I think, that sent by the Coast Survey to Labrador in 1854, to observe the eclipse of that year. In 1869 a large number of parties, some organized by the Coast Survey, some by the Nautical Almanac office and the National Observatory, and some by private enterprise, observed the eclipse of that year, and obtained results of importance. In 1870 the Coast Survey and National Observatory again sent eclipse parties to Spain and the Mediterranean. In 1872 two parties were sent, by special appropriations, expended under the direction of the Coast Survey, to elevated stations on the line of the Pacific Railroad to investigate the astronomical advantages of a high altitude. In 1874 the Transit of Venus occurred, and the United States took the field with eight parties, which in the completeness and effectiveness of their astronomical equipment certainly compared favorably with those of any other nation. It is too early as yet to pronounce upon the accuracy and value of their observations, but it is believed that they will prove satisfactory. The Coast Survey of course has astronomical parties continually engaged in the determination of latitudes and longitudes; and among these the chronometric expeditions of 1849 and 1855, with the telegraphic parties of 1866, 1870 and 1875 for determining the difference of longitude between Europe and this country, deserve special mention. At present also a naval party in the Caribbean sea is engaged in similar work of great importance to navigation.

In the department of Astronomical Physics we have also accomplished something. The investigations of Olmsted, Twining, Kirkwood and Newton hold an important place in the history of Meteoric Astronomy. Celestial photography originated in this country with the experiments of Bond in 1849, and no one has yet gone beyond Draper and Rutherfurd, or even equalled the latter. Something has been done with the spectroscope by the lamented Winlock, Harkness and others. Some very beautiful and delicate work upon the constitution of the solar surface, and the distribution of its radiation has been recently published by Professor Langley; and the work of Mason upon certain nebulæ as long ago as 1839, and the more recent work of the Bonds, Trouvelot, and Holden on the same subject must not be passed without notice.

In the construction of astronomical "apparatus of precision," comparatively little as yet has been accomplished in the country: though some small field instruments of great excellence have been constructed in the shops of the Coast Survey at Washington, and by one or two makers of surveying and nautical instruments in New York and Philadelphia. But in the construction of telescopes our American makers are, to say the least, unsurpassed. The number, magnitude and excellence of the great refractors, made by Clark and Sons of Cambridge, have secured them the first rank among opticians, and their instruments are now found in all parts of the world. The great telescope at Washington, of 26 inches aperture and 36 feet focal length, with its sister, the McCormick telescope not yet mounted (though optically completed), are the two largest and finest refractors in existence. The same firm also made the Chicago telescope of 18 inches aperture, and have constructed six others with apertures of between 11 and 13 inches; besides a host of smaller ones from 10 inches down. Some of the best work of Dawes and Huggins in England was done with their object glasses. Some other large instruments with apertures of about 12 inches have been built by Fitz and Spencer; among them the telescopes which in the hands of Peters, Watson and Langley are doing such good work. Indeed, the only foreign telescopes in the country of much value are the great 15 inch refractor of the Harvard College Observatory, the 12 inch instrument at Cincinnati, and the 9½ inch at Washington, all from the establishment of Merz at Munich. These, with about half a dozen of smaller size from the same makers, were imported before 1850.

In the line of reflectors we have done very little comparatively, although the first American essays at telescope-making were in this direction, by Mason, who in his observations on the nebulæ, to which allusion has been made, used an instrument of his own construction. Holcomb afterwards built several instruments of some power. The only one of much value in the country at present, however, is the magnificent silvered glass Cassegrainian of 28 inches diameter, constructed by Dr. Henry Draper of New York, and mounted in his private observatory at Hastings.

From this hasty review of our past astronomical history and achievement, which I hope has been written in an impartial and sober spirit, avoiding on the one hand all overstatement or claim of what is not our due, and on the other a disposition to depreciate and undervalue what has really been accomplished, I think we may justly entertain a certain amount of honest pride in our record; there is room, however, for a still more abundant humility, if we enter into detailed comparisons between our own scientific achievements and those of other nations. American Astronomy has not yet passed its infancy.

I must limit myself to brief remarks in speaking of its present condition, needs and prospects.

We have in the different states something over thirty establishments which bear the name of observatories, and the majority of them are fairly equipped with instruments—they need principally men and funds. Most of them are connected with our colleges or other institutions of learning, and are under the charge of some overworked Professor whose time is completely filled, and his strength burdened to the limit of endurance, by the routine of daily class instruction. He has no assistant, and there is no fund at his disposal to enable him to procure new instruments, to pay computers, or to publish any result he may have reached. In most cases he has come to his position by a promotion from some other chair, and without the thorough mathematical training which he ought to have had to make him really efficient as a man of science. Under these circumstances, all the use of the observatory and its instruments is merely to exhibit to the students of the institution a few of the more striking telescopic objects, and to keep the chapel clock to time. Original work is out of the question. To make a college observatory of any benefit to science it must be sufficiently endowed to enable its director to be free from the drudgery of teaching (it will not hurt him at all to be obliged to give a certain limited amount of instruction), and to furnish all needed improvements, assistance, and the means of publication. It is a fair question, however, whether such an observatory, equipped and manned for scientific work, would be any more effective as part of the teaching apparatus of the college than the present arrangement. The observatory of instruction needs no great telescope or meridian circle, but it does need and ought to have, the transit, fitted for use as a zenith telescope, the sextant, clock, chronometer and chronograph.

A few of our observatories however are effective, and stand fairly on a level with the other great observatories of the world. The establishments at Cambridge and Washington may justly be classed with Greenwich and Poulkowa. Some half a dozen other observatories also, though not so continuously active, send out good work from time to time, especially those at Clinton, Ann Arbor and Allegheny City. The Dudley observatory at Albany and that at Chicago, have been cramped and hampered by want of funds, but it is hoped that the difficulties in their way will be removed, and that they will soon again become active. In the near future it is expected that splendidly endowed and equipped observatories will be established by Yale College and the University of Pennsylvania, and it is to be presumed that they will be so organized and manned as greatly to reinforce our scientific strength. Then there is the magnificent donation of Mr. Lick, which, properly managed, will give to California an observatory whose income and equipment is rivalled only by the great Government institutions of Europe. It is to be hoped that this observatory will take advantage of the pure atmosphere of its mountain site, and provide especially for the cultivation of astronomical physics, and solar physics in particular, which at present is insufficiently represented in our country. One of the most encouraging signs of the times, is this, that private individuals of more or less wealth and leisure, are beginning to give themselves to astronomy. To Mr. Rutherfurd and Dr. Draper of New York, and Mr Burnham of Chicago, American science is already greatly indebted: may others follow their example, so that we may have what England has possessed in her amateurs, who have won for themselves and their country undying fame.

At present, as Professor Newcomb has pointed out, perhaps the immediate want of our American Astronomy is that of an Astronomical and Mathematical Journal of high order. Such a journal would have an influence upon our students and teachers like that of the "Astronomische Nachrichten" in Germany, and though probably its circle of contributors and readers would be but small, it would immediately and rapidly raise the tone and standard of astronomical work. A chief difficulty in the way of its establishment is that it would be pecuniarily unprofitable: another is the difficulty of finding a suitable editor. If, however, some wealthy patron of science should sufficiently endow such a publication the editorial difficulty could of course be overcome (if necessary by importation), and in my judgment, he would by such an endowment give a greater impulse to science among us than would be possible by the same expenditure in any other way.

As to the future of American science perhaps I am sanguine: but I fully believe that our free institutions are favorable to the highest scientific development, and that during the coming century our nation will take her place among the leaders of scientific progress. All signs seem to me to point that way. To be sure there must first be a great change in the spirit and temper of our students, and in the estimate of purely scientific work by the community at large. But the change appears to have already begun. An increasing number of our young men are willing to take the time needed for thorough training, and prefer the pursuit of truth to the mere getting of money; so that we have growing up among us a crop of young mathematicians who, in ability, will undoubtedly equal their predecessors, while in numbers, culture, incentive and opportunity, they have greatly the advantage. Our Universities are broadening and deepening their scientific and mathematical courses, and providing for post-graduate studies. Some of our men of wealth are finding in science a career and opportunity of honorable distinction more satisfactory than any other. Our public men are more ready to aid us in obtaining reasonable assistance from Government when needed. In short, everything seems to me to indicate progress, which since its beginning in 1836 has been uninterrupted, and is likely to continue. Therefore, I augur well for the future, and am confident that if the record of the century past can be called honorable, that of the century to come is to be glorious.

PAPERS READ.

SOME REMARKS ON THE USE AND INTERPRETATION OF PARTICULAR INTEGRALS WHICH "SATISFY" GENERAL DIFFERENTIAL EQUATIONS EXPRESSIVE OF DYNAMIC PROBLEMS, IN CASES WHERE GENERAL INTEGRATION IS IMPOSSIBLE: — SUGGESTED BY LAPLACE'S DYNAMIC THEORY OF THE TIDES. By J. G. BARNARD, U. S. Army.

LAPLACE commences the application of his general conditional equations to the actual tidal developments with the remark:

"In the general case the integration of eq. [2380]" (Bowditch) "surpasses the power of analysis; but to determine the oscillations of the ocean it is not necessary to integrate it generally—it is sufficient merely to satisfy it. For it is evident that the part of the oscillations which depends on the primitive state of the sea, would have quickly disappeared by resistances of different kinds; so that were it not for the action of the sun and moon, the sea would long since have assumed a permanent state of equilibrium. The action of these bodies continually disturbs it, and it is sufficient to ascertain the oscillations which depend on this action."

The above may, I think, be paraphrased and amplified thus: *Given* any initial condition (or "primitive state") of form and motion of the waters of the sea, the "general integral" should inform us what, at any future time, that form and motion will be, as resulting from this primitive state and ("resistances of different kinds" abstracted) the attractive forces of the sun and moon; but since, through these resistances, all initial disturbances would speedily resolve themselves into the condition of equilibrium, we need seek only to find that *particular disturbance* which, once imparted, would, by the action of the foreign attractions (abstracting resistances) be *permanently maintained.*

Such is the problem of Laplace; but the *real* problem, is this: Since all initial disturbances speedily vanish, therefore a state of equilibrium may be assumed as the "primitive state of the sea;"

— and the problem becomes, "from a state of equilibrium to find what at any future time would be, subjected to the action of the foreign attraction, *and* of resistances of different kinds, the form and motions of the sea.

The "oscillations which depend on the action" of the attracting bodies (their *initial imparting* being really implied, and resistances neglected), are not really the tidal oscillations, and can only be accepted as the nearest approximations attainable to the solution of an insoluble problem.

The expressions for the rise, angular *displacements*, *u* and *v*, in latitude and in longitude due to the diurnal tide given by Laplace (vol. 2, Bowditch), are

$$(a) \quad \frac{6\,L}{r^3} \; \frac{lq \sin \theta \cos \theta \sin \text{v} \cos \text{v} \cos (nt + \omega - \psi)}{2\,lgq\left(1 - \frac{3}{5\rho}\right) - n^2}$$

$$(b) \quad u = -\frac{\frac{3L}{r^3} \sin \text{v} \cos \text{v} \cos (nt + \omega - \psi)}{2\,lgq\left(1 - \frac{3}{5\rho}\right) - n^2}$$

$$(c) \quad v = \frac{\frac{3L}{r^3}\,\frac{\cos \theta}{\sin \theta} \sin \text{v} \cos \text{v} \sin (nt + \omega - \psi)}{2\,lgq\left(1 - \frac{3}{5\rho}\right) - n^2}$$

in which L is the mass of the attracting body (sun or moon); r its distance from the earth's centre, v its declination, ψ its right ascension, θ the polar distance of the particle of water, considered as part of an ocean covering the whole globe the depth of which is $l(1 - q\cos^2\theta)$. It must be remarked that r. v. and ψ are regarded as constants; that is, the formulæ express the tide as it would be had the attracting bodies no proper motion.

ω, the longitude of the particle counted from a fixed meridian on the earth's surface

n, angular velocity of earth's rotation; the time, t, being reckoned from the passage of the celestial meridian from which the right ascension Ψ is counted.

g, the force of gravity

ρ, ratio of mean density of the earth to that of water.

Earth's radius taken as unity; and its oblateness disregarded as it affects the distribution of the attraction of L, or the amount of the displacements; though, of course, to the undisturbed fluid surface must be attributed the form of equilibrium which is required by the forces g and (centrifugal) n^2.

The depth of the ocean (at the equator) is invariable with longitude, but decreases (q being positive) with increase of $\cos^2 \theta$ from equator to pole — increases if q be negative. The *difference* between greatest and least depth (equatorial and polar) is $= lq$.

An attentive examination of the expressions (b) and (c) will reveal the fact that the displacements in latitude and longitude of the particles of this fluid envelope are exactly as if it were a *solid* envelope possessing that extremely slight degree of flexibility required (in case the surface of the nucleus be spheroidal and not truly spherical) to adapt itself to change of position.

This may also be shown by reference to the expression (a) for rise and fall. Putting the variable depth $l\,(1 - q \cos^2\theta) = \gamma$; and supposing θ to receive a small increment $\Delta\theta$. The increment $\Delta\gamma$, of depth thereto corresponding is

$$(d) \quad \Delta\gamma = \frac{d\gamma}{d\theta}\Delta\theta = 2\, lq \sin\theta \cos\theta \Delta\theta$$

Now a particle of which the polar distance is θ when undisturbed, undergoes a tidal displacement in latitude, u, expressed by (b). Take this value of $\Delta\theta$, substitute it in the third member of (d), and the result is identical with (a). In other words the rise of the diurnal tide (a) as it is determined by Laplace, is nothing more than what is due to the slipping, *as a solid*, of the fluid shell over the surface of the nucleus.

Hence, if the fluid shell be of uniform thickness (depth) there will be *no* rise or fall of tide, accompanying the diurnal tidal disturbance of the ocean.

But according to the *dynamic* theory the vertical disturbance is due to two causes (of which the *equilibrium* theory takes into account but one), viz.:

1st. The foreign attraction.

2d. The pressure arising from the thereby induced *motion* (currents) of the fluid.

It is obvious, therefore, that since, with uniform depth, there is *no* diurnal vertical disturbance, these two causes must be *opposed to*, and exactly *neutralize* each other.

Moreover, when the depth is *not* uniform, the diurnal tidal rise (or fall), as expressed by (a), is solely owing to shifting *of place*, and the 2d of above category of pressures must not only neutralize the foreign attraction, but also sustain the abnormal height. These two propositions we shall find likewise to be deducible from Laplace's expressions (a), (b), (c).

Referring to Airy "Tides and Waves" § 87; or to the writer's more detailed *rationale* (Amer. Jn'l Science Vol. 27, 1859, p. 353) we find (p''' being the pressure generated by the tidal *motion*)

$$\frac{dp'''}{d\theta} = -\frac{d^2u}{dt^2} + 2n \sin\theta \cos\theta \frac{dv}{dt}$$

Deducing $\frac{d^2u}{dt^2}$ and $\frac{dv}{dt}$ from (b) and (c) and substituting in above and integrating:

$$(e) \quad p''' = \frac{n^2 \frac{3L}{r^3} \sin v \cos v \sin\theta \cos\theta \cos(nt + \omega - \psi)}{2\,lgq\left(1 - \frac{3}{5\rho}\right) - n^2}$$

Now the pressure due to the abnormal elevation will be expressed by (d) or its equivalent (a) multiplied by g. Subtract from this product the expression for the diurnal component of solar attraction (as given by Laplace)

$$(f) \quad \frac{3L}{r^3} \sin v \cos v \sin\theta \cos\theta \cos(nt + \omega - \psi)$$

and we get a result identical with the above value (e) of p'''.

If, in (e), q be zero; in other words if the depth be uniform, we have

$$p''' = -\frac{3L}{r^3} \sin v \cos v \sin\theta \cos\theta \cos(nt + \omega - \psi)$$

which, with the reverse algebraical sign is equal to that (f) the disturbing force generates; and neutralizes it.

The expressions (a), (b), (c), of Laplace are thus shown to *satisfy* (as they should) the conditions; but I have taken the pains to demonstrate this fulfilment, because I have shown that, regarding the water *by itself*, there are really no currents in it. The actual tidal currents are the motions of the parts of the *quasi* solid watery shell *with reference* to the underlying parts of the really solid nucleus. It remains to be said that this peculiar relative motion implies simply *a displacement of the axis of diurnal rotation of the quasi solid shell—i. e.*—an angular *separation* of its diurnal rotation-axis from that of the nucleus, which, at least for the entire range of values of expressions (a) (b) (c) retaining their approximate truth, is very slight. The angle is analytically expressed by putting, in (b), $\cos(nt + \omega - \psi) =$ unity. Indeed, for all *uniform* depths whatever (within the narrow limits in which the formulæ hold good), the angle of separation would be expressed by the ratio of the diurnal function of the disturbing force $\left(\frac{L}{r^3}\right)$ to the centrifugal force n^2, *i. e.* by $\frac{3L \sin v \cos v}{r^3 n^2}$[1] which, for the sun alone, in its maximum declination, is about $1\frac{1}{2}$ seconds of arc: for the moon (alone) twice that arc.

What precedes refers only to the diurnal tide. We know that the disturbing or tide-producing influence of the sun or moon is expressed by three harmonic terms, two of which, *tend* to produce in the fluid surface of the earth, the two forms of spherical harmonics indicated by Figs. 1 and 2; and these are, identically, the

Fig. 1.

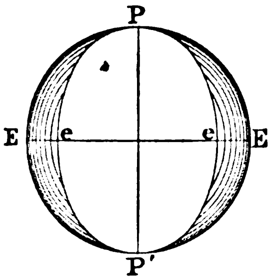
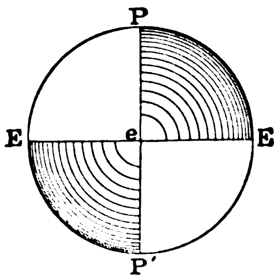


Fig. 2.



forms of the corresponding tidal developments in the "equilibrium" theory. $P\,P'$ (in both figures) is the axis of the earth; $E\,E$ the equator; the attracting body (on the right and in the plane of the paper) being supposed to have a northern declination, v. The shaded portions are bulged, the light portions depressed; the two meridional great circles $P\,e\,P'$ being neutral lines in Fig. 1, while in Fig. 2, the one meridian $P\,e\,P'$ and the equator itself are the neutral lines. Fig. 1 is the *semi-diurnal* disturbance; Fig. 2, the *diurnal.* As the earth revolves on its axis these *forms*,

[1] It is interesting to remark that, for a homogeneous fluid globe regarded as an ellipsoid of equatorial oblateness due to equilibrium with the centrifugal force, n^2, the diurnal distortion (shown by Fig. 2) indicates a slight rotation *of figure* about *e* (of that diagram) measured by identically the same expression. But in the latter case the relative *motion* (range of motion of particles from normal position) is very much less, the distortion being effected by a much smaller tilting (the above angle multiplied by *twice the ellipticity*) of the parallels of latitude (planes of diurnal rotation). Such a tilting will produce the rotation *of figure* required and it is shown by me (Addendum to Problems of Rotation, Vol. xix, Smiths. Contribution) as resulting from Mr. Hopkins' investigation that this *is* the real character of the very slight internal motion of the fluid to accommodate itself to the required *external form.*

to maintain their relation to the motionless attracting body, should move (relatively) around that axis. In Fig. 1 the axis of symmetry of figure coincides with the axis of rotation — whereas in Fig. 2 the axis of figure (an equatorial one perpendicular to the plane of the paper) is in the most incompatible position possible for such a relative motion. To adapt itself to the earth's rotation about an axis co-incident with that of its own figure, the motion required of Fig. 1 is a true "forced wave" motion, running from east to west, for which the foreign attraction supplies the needed force; whereas the motion demanded for Fig. 2, is in direct conflict with that, which, to maintain *this figure* as a wave, should be given. It may, however, be regarded as what is called a "stationary wave," the development of which is *meridional*, *i.e.* one which, on the same meridian, alternatively rises and falls in the N. and S. hemispheres, the equatorial elevation remaining *constant:* the *phases* progressively varying from one meridian to another.

But the "particular integrals" for both cases by which to express the tidal *rise* and *displacements* (in latitude and longitude) are all *wave forms*, such as, $K \frac{\sin}{\cos} (i\,t + A)$ which (disregarding the proper motion of the attracting body), become, for the semi-diurnal tide

$$(g) \quad K \begin{matrix}\sin\\ \text{or}\\ \cos\end{matrix} \left\{ 2\,nt + 2\,\omega - 2\,\psi \right)$$

and for the diurnal tide

$$(f) \quad K \begin{matrix}\sin\\ \text{or}\\ \cos\end{matrix} \left\{ nt + \omega - \psi \right)$$

in which last (f), the values of K for the *diurnal* tide are made manifest in (a), (b), (c), for each particular expression.

The resulting *wave* for the *semi*-diurnal tide, as it results from the dynamic theory, if the depth be sufficiently great, has the same general configuration as in Fig. 1, and all the characteristics of the disturbing influence itself. It is a *forced* wave which *travels with* the disturbing influence and is *maintained* by it — but mathematically speaking it implies an *initial* condition exactly conforming.

Nevertheless, since the disturbing force would actually *maintain* (resistances abstracted) this wave, we may suppose the *actual* wave approximates very nearly to this theoretical determination: since when generated, the resistances (friction, etc.) are regarded as small compared to the generating forces. But the case is quite different for the diurnal tide. Could the motions leading to the tidal developments (*a*) (*b*) and (*c*) be *generated* by the disturbing force? The following remark aids in answering this question. When a fluid moves *as if it were a solid, the laws of motion of solids to a certain extent apply.*

Regarding the fluid shell as a *solid* of revolution enveloping the nucleus, there is no tendency to disturb the direction of its rotation-axis — for a study of the expression for the diurnal term of the attractive force (and of the graphic delineation of its distribution in Fig. 2) shows that it can produce no *couple.* (In reality, when the *oblateness* of the earth is considered this diurnal term is the only one which *does* produce a couple, *i. e.*, that which causes the precession of the equinoxes: but in tidal theories the oblateness is disregarded.) All that can be said of it is that if the change of direction were *initially* established, the relative motion would *maintain itself*, the foreign attraction maintaining the *quasi* solidity, by everywhere neutralizing the pressures the *motion* of the *fluid* would generate and thus preventing internal strains.

An *initial* establishment of this motion is therefore implied. It may be argued however — it may even be admitted — that, since such motion, probably a minimum, fulfils the *conditions*, that which would *at first* be generated (considering the phenomenon as having a *beginning*) would, through the resistances of friction, viscosity, etc., resolve itself into *this.* But there is no such thing in the nature of *this* case as the prolonged *uniform* action needed. The required amount of angular separation of the solid nucleus and *quasi*-solid shell *varies* incessantly with, and nearly in proportion to, the declinations,[2] positive or negative, of the two attracting bodies (sun and moon) and is sometimes a *sum*, sometimes a *difference* according as their distinct requirements are in unison or in conflict. The axis of rotation of a solid cannot be shifted without producing internal strains; whereas it is *the* essential condition that a fluid may move *as a solid*, that there be *no* internal strains. What in a solid would be strains (in the case in hand) would, in the fluid, as pressures, appear as *tidal rise and fall.*

When $lq = \dfrac{n^2}{2g\left(1 - \frac{3}{5\rho}\right)}$; or when the difference between equa-

[2] It varies as *sin* v *cos* v, or as the *sin* 2v:

torial and polar depth is about seven miles (*half* the oblateness, nearly) *all* the *dynamic* force of the fluid motion is consumed in maintaining the corresponding abnormal heights due to the slipping of the fluid shell — leaving none available to counteract the foreign attraction;[3] no amount of velocities (and displacements) suffices and the expressions (*a*) (*b*) (*c*) become infinite. As their truthfulness is subject to the condition of their being *small*, this result shows that there is a considerable range of values for lq not very much greater than the depths of our actual oceans through which this particular solution, if it were otherwise unexceptionable, has no applicability and for which we have no clue as to what the tidal development would be.

For the *semi-diurnal* tide the values of the coefficient K are determinable for a continuous ocean of uniform depth, if the changes in right ascension, declination and distance, of the attracting bodies be disregarded, as also the density of the water in comparison with that of the earth. In that case the coefficient K of (*g*) for tidal elevations, has been developed by Laplace into a series

$$A^{(1)} \sin^2 \theta + A^{(2)} \sin^4 \theta + A^{(3)} \sin^6 \theta \; . \; . \; . \; . \; \&c.,$$

in which $A^{(1)}$ is the ratio of the semi-diurnal function of the disturbing force to gravity *i.e.*

$$A^{(1)} = \frac{3L}{4\, r^3 g} \cos^2 v$$

while $A^{(2)}$ is *in*determinate; and the remaining coefficients $A^{(3)}$ $A^{(4)}$ etc., are determinate linear functions of these two, expressed by the equation

$$(h) \quad 0 = A^{(f+2)}(2f^2 + 6f) - A^{(f+1)}(2f^2 + 3f) + \frac{2n^2}{lg} A^{(f)}$$

Taking the above value for $A^{(1)}$; retaining the symbol $A^{(2)}$ to represent *any* arbitrary value; determining by (*h*) the others in terms of these two, we should have an expression, in series, for the tidal rise, made up of *two* sets of terms one of which has

[3] There is a corresponding case for the semi-diurnal tide (when ($q = 1$) the depth is represented by $l \sin^2 \theta$, or is l at the equator and zero at the poles. When $l = 7$ miles (nearly) the expressions for the tidal elevations become infinite. The inclination of the ocean bottom has the same effect as in the corresponding case of the diurnal tide. With l greater than 7 miles the crests of high and low semi-diurnal tide are in their normal relation to the attracting body. When less they are *reversed*.

for coefficient the disturbing (or tide-generating) force $A^{(1)}$ (which disappears if that force has no existence); the other has for coefficient the indeterminate or *arbitrary* quantity $A^{(2)}$—which terms having no dependence upon a disturbing force evidently express a peculiar *free wave* semi-diurnal in its phases, which, once put in motion (*i. e.*, *initially* established) may exist, *resistances of friction*, *viscosity*, *etc.*, *abstracted*, not only independently of the disturbing force but simultaneously and *cumulatively* with the "forced wave" expressed by the terms in $A^{(1)}$. But this free wave "is one of the oscillations which depend on the primitive state of the sea," and which "must quickly have disappeared by resistances of various kinds." I therefore *omit* entirely all terms having $A^{(2)}$ as a coefficient (which is equivalent to putting $A^{(2)} = O$); and I find for expression of tidal rise [4]

[4] Mr. Airy has somewhat sharply criticised Laplace for the unwarranted inference that $A^{(2)}$ is subject to the law governing the succeeding coeffi·ients, expressed by the equation *(h)*. I think the above remarks justify my partial concurrence. But Mr Airy retains Laplace's developments (with (A^2) thus determined); which only amounts to so much increase to the coefficients of the arbitrary series which he adds, and says Laplace "ought to have added to this series" the very terms which I (as stated in the text) *have omitted*, as expressing what Laplace, at the outset, in language I have twice quoted, *ruled out*. These remarks seem necessary to explain why in formula *(i)* I use an expression not only different from Laplace's, but also from Mr. Airy's correction. Mr. Airy remarks that, through the indeterminateness of $A^{(2)}$, the condition may be imposed that the meridional component of tidal motion be nothing for a prescribed latitude—*i. e.*, that "an east and west barrier following a parallel of latitude be erected in the sea;" or in other words that the ocean be bounded north and south of the equator by shores following certain parallels. This must be admitted; and *likewise* that *to accomplish it*, one of those "quickly disappearing" *initial* oscillations (*i. e.*, *free waves* having the $A^{(2)}$ coefficients) is involved. It may, however, be alleged that the "barriers" introduced supply the *forces* by which a new and corresponding form of "forced waves" would be *maintained*. Compared with ocean dimensions the effect or *force* exerted by the solid barriers would be felt to but a trifling distance; and I am of opinion that elsewhere the enduring tidal development would be the same as before.

One further remark may be made. The $A^{(2)}$ terms represent an absolutely "free" wave, inasmuch as it can exist even though there be no semi-diurnal distorting force. They also represent an oscillation which when superimposed upon the "forced" wave, the *two together* "satisfy" the conditional equation (as the latter does singly). As in the case of the diurnal motion on which I have commented, this superimposed oscillation (absolutely arbitrary in the *magnitude* of its development) is neither generated nor *maintained* by the disturbing force. To the former, however, the external force is indispensable in order to suppress the strains which would mar the quasi-solidity of the water (and the *self*-maintenance of the motion); but the latter is absolutely independent of such force. I cannot doubt that I am right in my position that this latter would (through resistances) speedily disappear and settle down to the dynamic "equilibrium" expressed by the terms in $A^{(1)}$: in other words the "forced" wave alone: nor that, though it "satisfies" the conditional equation, it should form no part of the solution, either in the disguised form in which Laplace has introduced it; or in the explicit form given it by Mr. Airy.

$$(i) \quad \frac{3L}{4r^3g} \cos^2 v \Big\{ \sin^2 \theta - \frac{1}{4}\frac{n^2}{lg}\sin^6\theta - \frac{7}{40}\frac{n^2}{lg}\sin^8\theta$$

$$-\left(\frac{21}{160}-\frac{1}{72}\frac{n^2}{lg}\right)\frac{n^2}{lg}\sin^{10}\theta - \left(\frac{231}{2240}-\frac{1}{11520}\frac{n^2}{lg}\right)\frac{n^2}{lg}\sin^{12}\theta$$

$$-\left(\frac{1}{12}-\frac{1}{300}\frac{n^2}{lg}+\frac{1}{2280}\frac{n^4}{l^2g^2}\right)\frac{n^2}{lg}\sin^{14}\theta \ldots$$

$$\text{etc.} \ldots \Big\} \cos(2nt + 2\omega - 2\psi)$$

The expression above is not easy of interpretation.

In (i), $\frac{n^2}{lg} = \frac{1}{289}$ (ratio of centrifugal force at the equator to gravity). Hence $\frac{n^2}{lg} = 1$ *when* the ocean depth l is fourteen miles (nearly); $>$ 1 when l is less than that depth; and *vice versa.* Even when the depth is small the series is rapidly converging *in terms of sin θ;* hence for all latitudes somewhat removed from the equator the semi-diurnal tidal rise is essentially the same as that expressed by the *equilibrium theory* be the depths great or small —*i.e.*

$$\frac{3L}{4r^3g} \cos^2 v \sin^2 \theta \cos(2nt + 2\omega + \psi).$$

And the same dictum is probably applicable to *all* latitudes, even equatorial, when l is sufficiently large.

The changes of declination of the attracting bodies alter nothing, in this case, but the *intensity* of the semi-diurnal component of the foreign attraction and indeed this intensity, undergoes but slight changes.[5] The precise amount of rise and fall and the velocity and range of tidal motion are thus varied—but (for any given depth of ocean) their *distribution* is not altered. There are no such conflicting effects as are found in the demand for a shifting axis of rotation. Hence we may admit that by continued subjection to the foreign attraction the ocean would conform itself, in the main, to this configuration implied by the particular integral, especially if it can be proved that this configuration involves the *least possible* motion. But it must be borne in mind that, even with this concession, the tides thus expressed are *not* really the tides of nature; they are theoretical oscillations, abstracting resisting forces, and *implying initial conditions.* The latter are *generated* disturbances; (generated from *any* arbitrary initial displacement

[5] It varies as $\cos^2 v$, and never falling short for sun or moon separately of 85 per cent. of its max. value; the variation is very much greater when the *joint* action is considered.

we may, within limits, imagine) and limited in some degree in their development by the resistances.

In conclusion I would remark that the resort (compulsory) to particular integrals which "satisfy" the general conditions, does not (in a theoretical point of view) dispense with initial conditions, as Laplace's remarks would seem to imply; but they are the expressions for a form of disturbance which, *if once applied*, would be permanently maintained in their original form by the foreign forces. What relation these theoretical forms have to the case of nature I have tried (imperfectly) to indicate. Their *particularity* consists in defining a *particular* initial disturbance and also the character of *sea outline* and *sea bottom*, on which it could be maintained were the maintaining forces unvarying. The ocean to which they apply must, for the diurnal tide, cover the whole globe; or for the semi-diurnal, must be *bounded by parallels of latitude* equidistant from the equator, and its depth, in either case, must be uniform along such parallels.

The disregarding of the chronic variations of the foreign attractions (or proper motions of the attracting bodies) does not prevent acceptance of the solutions as approximate expressions for the semi-diurnal tides of an ocean fulfilling the above *non*-natural conditions: but it is in serious discordance with their applicability to the case of the diurnal tide.

Mr. Airy, in commenting on Laplace's dynamic theory, alludes to the evanescence of the diurnal tide when the ocean depth is uniform, as one of "the most remarkable results" of that theory, and one, he says, which Laplace himself has frequently alluded to in a way that shows that he considered it one of the happiest of his discoveries."

If my deductions are not erroneous that discovery is not of anything which could have place in nature, even if we concede to "nature" such an ocean as the theory prescribes; but of a purely mechanical theorem alien to the conditions under which "tides" are generated.

EXPERIMENTS ON THE GYRATION OF LIQUID MASSES IN ROTATION.

By J. W. OSBORNE, of Washington, D.C.

THE object of the present communication is to bring before the members of the Association certain experiments which have a general interest for the physicist, and a direct bearing upon the vexed question of the condition of the earth's interior. It is well known that Mr. Hopkins, in 1839, and subsequent years, brought before the Royal Society a number of papers, in which he endeavored to show, by very profound analysis, that to account for the phenomena of precession and nutation, the earth must necessarily have a rigid crust of about 1,000 miles in thickness. This was virtually disposing of the liquid nucleus as far as geological upheavals, and other movements of the crust were concerned. Sir William Thomson in 1863, examined Mr. Hopkins' investigations of this subject, and approving his conclusions generally, went much further, asserting the necessity for a much thicker crust—practically a solid sphere—possessed of a rigidity greatly superior to that of steel. M. Delaunay discussed this matter a few years later, combating Sir Wm. Thomson's conclusions. He expressed the opinion that the particles of a liquid may behave as those of a solid, and be practically without free movement upon each other, when the mass-motion demanded of them is exceedingly slow. General J. G. Barnard, in a number of papers published by the Smithsonian Institution, as late as 1872, criticised M. Delaunay's views, and went very fully into the whole subject. He arrived generally at the conclusion that, were the earth a liquid mass, it could not under the perturbing influence of the sun and moon, manifest the phenomena of gyration. Interpreting the drift of Sir Wm. Thomson's argument, and endorsing it at the same time, he says, that "a revolving spheroid destitute of rigidity, a homogeneous fluid one for instance, would have no precession."

From this very superficial sketch of the mathematical side of the discussion it will be seen, that while it is universally admitted that the earth obeys the gyroscopic law, making its gyration once in about 25,000 years, that very fact has been regarded, by some of the highest authorities, as furnishing proof that it cannot consist of a fluid mass covered with a thin shell.

Discussing this subject in the early part of the present year with two scientific friends in Washington, the fact was commented on, that one phase of the discussion at least depended for its solution on the question, whether or not masses of fluid in rotation would follow the gyroscopic law in a manner similar to solids. It was urged that *a priori* reasoning could not settle this fundamental problem, and that nothing short of experimental demonstration would be convincing and satisfactory. After some consideration I undertook to devise means for putting this doubtful question to a practical test; to determine whether or not a mass of fluid revolving about an axis would perform a gyration, under the joint influences of a perturbing force, and its own rotating momentum; notwithstanding the mobility of its molecules,—in other words the absence of rigidity.

The first endeavor was to construct a top consisting essentially of a mass of water enclosed in a thin pliant envelope; to get the whole into rapid rotation; and then argue, if it spun at all, that the water played an essential part in supporting the top in its condition of unstable equilibrium. The difficulty in the construction of this instrument lay in the centering of a pliant sack or envelope of symmetrical shape on a suitable spindle.

This was accomplished as follows: A tube of very thin brass about ⅕ inch outside diameter, and 5 inches long, is provided with a point on which it is finally to spin, and which screws into one end. Above the point, and fast to the tube is a small thin flange. On the tube above slides a very little stuffing-box carrying a similar flange at its lower edge; this can be clamped at any place by simply screwing the ring down upon the stuffing. The outside of the two flanges is turned so as to form part of a sphere three inches in diameter, and for each a thin washer is provided of the same curvature and size, which can be screwed down upon them by two nuts, one on the outside of the point, and one on the outside of the stuffing-box. The lower part of the tube is pierced with a number of very small holes running spirally round it. The whole of this arrangement runs perfectly true in the lathe.

A mass of sugar which has been melted and cast into a cylindrical mould, a little more than three inches in diameter, and the same in height, is next prepared. This has a hole bored through its centre into which the spindle is thrust; the latter being heated so as to attach itself to the sugar, which is also firmly held between the two flanges brought to within three inches of each other. When cold the spindle is again caught in the lathe, and the sugar turned off, so as to form a sphere three inches in diameter. The surface of this ball, including the outside of the flanges, is now to be repeatedly coated with thick collodion, made with a minimum of alcohol, and containing an exceedingly small percentage of castor oil, which gives a tough pliant character to it when dry. How thick this coating should be, must be left to the judgment of the operator. About the thickness of thin card-board will be sufficient, if the materials are good and carefully applied; but much depends upon the quality of the gun-cotton, and the quantity of castor oil employed.

The several coats having thoroughly dried, the point is to be screwed out, and the ball suspended from a rubber tube attached to the upper extremity of the tubular spindle. A stream of water is now to be sent down the axis; this finds its way through the lateral holes and gradually attacks the sugar. In about three hours the whole of the inner mass can be dissolved out, an operation which can be hastened after a while, by passing a plug of cotton to about the centre of the sphere; thereby obliging the water to enter at the upper, and pass out at the lower holes. This being completed, the point and concave washers are replaced, and the latter screwed tight down upon the film. A minute air-hole is finally bored through the upper washer and flange, which may be closed by a little wooden plug if desired.

The total weight of this top when full of water, is 4,082 grains, empty, it is 470 grains, while the spindle and brass-work weigh 394 grains.

The spindle and brass-work cannot be spun alone, except at a very high speed, — more than 100 revolutions per second; the empty top can, but with some difficulty, and for a short time only, requiring also a very high speed. When full of water the spinning is easily accomplished with apparatus suitable for the purpose, which is necessary, because the maximum speed at which the shell is driven must be steadily maintained for a considerable time, so as to get the water into rotation as well. When released the top will then spin steadily for four or five minutes; gyrate as if solid; and rise to a "sleeping" position like any solid top; the only observable difference being, that when it begins to fall, it does so very quickly. The speed should be considerable, but not so high as to burst the collodion envelope, on which the water exerts a very great pressure.

The legitimate deductions from this experiment would seem to be that the water plays an important part in preventing the fall of the top, and that its gyrations are caused by the perturbing force of gravity. It may be urged that the shell, being in rapid motion, supports itself and the water too; and that its rapid motion is continued by reason of the friction and momentum of the water within. But the empty shell is only capable of sustaining its own weight for any time, when driven at a very high speed; whereas, when it is full, the weight is increased more than eight times, and does not yield to gravity when the speed is comparatively slow.

It must be admitted, however, that the result in this experiment is not wholly due to the water, and I therefore contrived another in which any action ascribable to the case, or rigid parts of the apparatus, is totally eliminated. This I will now describe and exhibit to the section.

A flat cylinder closed at both ends, and of exceedingly thin metal, is constructed with much care, so as to secure symmetry and strength. This is eight inches in diameter, and two and one-half inches high. Through its axis a small tubular spindle is passed and firmly attached to the upper and lower disks, springing them apart in so doing, to give strength and rigidity to the whole. Into this spindle a long steel point is firmly inserted, on which it is intended this large top should spin; the extremity of the point being three and one-half inches below the centre of gravity.

When properly made, this instrument should not weigh more than eight ounces, but as it is very liable to burst when in use, its strength must not be sacrificed for lightness. I will not go into further details, remarking only, that the lathe should be used from first to last in its construction, so as to secure perfect concentricity.

This vessel is now to be filled with water through the tubular spindle (which has lateral holes for this purpose inside the cylinder), whereby its total weight is increased to nearly six pounds. The point is then placed on a small porcelain saucer (such as those used for grinding Indian ink), and held there by a sort of curved wooden nippers, which can be removed at any moment. Rotary motion is now applied to the spindle, which is gradually increased till the top makes about eighteen or twenty revolutions per second. By means of the nippers the spindle can be inclined, and held so till the motion has been sustained long enough to insure the equally rapid rotation of the mass of water within, when it is to be suddenly released. The top will now spin, making large, slow gyrations, rising gradually to the upright position, and so continue for fifteen minutes or more. So far there is nothing new; it is only the previous experiment in a modified form; but if, while it is still gyrating, we suddenly grasp and stop the shell with the fingers, and as suddenly let it go again, what will take place? Will the top stand or fall? If this operation is dexterously performed it will not only stand but will continue the gyration which has been interrupted, showing no tendency to fall whatever, and the experiment can be repeated several times, before the power that sustained it is exhausted. Of course the water continues its rotation after the shell is stopped, and when let go, the latter takes up that rotation rapidly from the water; but long before the speed becomes sufficient to support even the shell alone, time enough will have elapsed for the whole to come to the ground.

When we consider that at the moment of release, not only has the shell weighing half a pound to be supported, but also five and one-half pounds of water, with the centre of gravity hanging over the point of suspension, it must be admitted that we have here a very distinct manifestation of force.

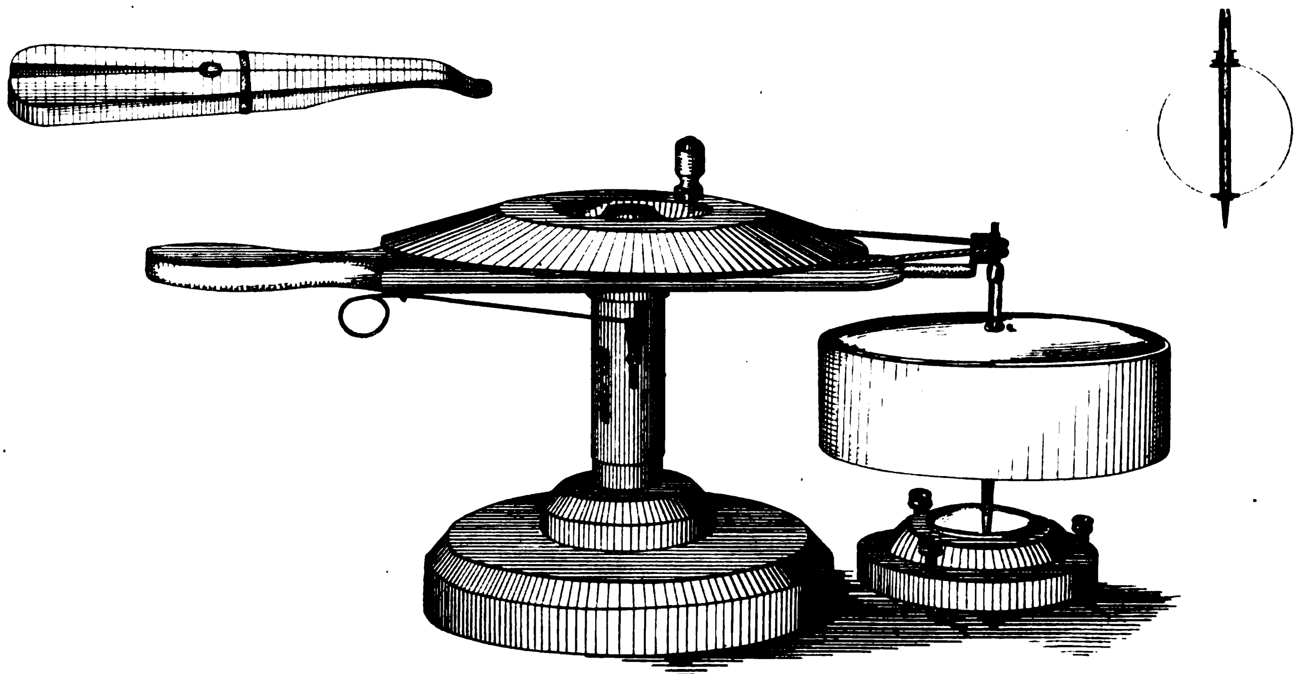
This I submit is a crucial experiment, establishing beyond question, that rotating masses of liquid behave *similarly* to solids when acted on by a perturbing force, which tends to modify their plane of rotation, giving a gyration as the resultant in both cases.

As some persons may wish to repeat these experiments, and as a method of imparting rapid, long continued motion, in a definite and convenient manner, is a very indispensable condition of success, I will conclude this paper by giving a general description of the instrument I have constructed for this purpose, and which works very satisfactorily.

A flat wooden base about nine inches in diameter is heavily loaded with lead. From the centre of this rises a short upright pillar one and one-half inches in diameter, the top of which is about six and one-half inches above the table on which the instrument rests.

Upon this slides up and down, but hindered from turning, a sleeve of brass, to the upper end of which a long cross-piece of wood is firmly screwed. A horizontal, multiplying wheel, eleven inches in diameter, runs in a suitable bearing in the cross-piece, its centre being exactly over the pillar. The ends of the wooden cross-piece extend beyond the multiplying wheel. One of these is shaped to form a suitable handle by which the spinner is grasped

THE GYRATION OF LIQUID MASSES IN ROTATION.



when in use; and the other contains a horizontal socket which receives a short brass bar, terminating at its outer end in open bearings for a little pulley about one-half an inch in diameter driven by the multiplying wheel, the cord from the latter being kept stretched by a spiral spring inside the socket. This pulley consists of a short perpendicular axis, upon which are two little flanges, one fixed, and the other capable of being screwed up like a nut. Between these flanges two disks of rubber one-eighth of an inch thick are pinched, the adjacent sharp edges having been rounded off, thereby forming a groove for the cord. As these rubber disks can easily be replaced by others of larger or smaller diameter, any desired relation can be had between the pulley so formed, and the driving wheel. The advantage secured by the rubber is the total absence of slip. The lower extremity of the axis of this little pulley is expanded and hollowed out to a cup or bell shape; this is let down on the end of the top spindle, the extremity of which is also made slightly conical in form. The friction between the two drives the water-top.

We must now return to the pillar and sleeve. In the axis of the former a very strong spiral spring is inserted, which tends at all times to lift the sleeve and all that is above it with considerable force. To lay hold of the top-spindle by the hollow cone on the end of the axis of the little pulley, this spring must be compressed, in which position it is held by a catch falling into a small steel rack on the outside of the sleeve, which is controlled by a trigger conveniently placed under the handle of the instrument.

When the speed of the top is sufficient it is instantaneously released without ceasing to turn the multiplying wheel, by touching the trigger with the finger of the left hand, the movement being a steady upward one which gives no impulse to the top in any direction. Finally, the porcelain saucer on which the top spins, is capable of adjustment up and down by three levelling screws, so that the height of the upper extremity of the spindle can be made to suit the spinner exactly.

The experiments described in the foregoing, must be regarded as tolerably conclusive, especially that exhibited before you; but they are by no means exhaustive, and I hope to extend and generalize the experimental treatment of this subject, eliminating in each successive modification some one of the restrictions which interfere with a just appreciation of the phenomena.

Determinations of Subjective Temperature. By J. W. Osborne, of Washington, D. C.

At the meeting of the Association last year in Detroit, I described and exhibited to the Section, an instrument designed to furnish means for ascertaining the fluctuations in sensible climatic temperature. It is not my intention to refer on this occasion to the observations and experiments that have since been made with that instrument, any further than to state generally, that the results, though interesting and valuable, are too negative in their character to make their presentation desirable at present. This is especially true of the long series of efforts to determine the equation of the curve of cooling, upon a knowledge of which all systematic work must depend. Only step by step have the difficulties been discovered and appreciated which interfere with the establishment of constant external conditions, on the one hand; and the accurate measurement of the intervals of time required for the loss of precisely equal increments of heat, upon the other. Many of these difficulties have been overcome, but some still remain to be dealt with and conquered. If progress in this direction is slow, the complex influences which affect living beings, producing in them the sensations of heat and cold, are still active; and the problem above stated is as important, and its solution as urgently needed as ever.

I wish now to direct attention to another method quite distinct in its nature from that already referred to, but calculated to give very similar results. Before passing to details however, it will be well to make a concise statement of the conditions and influences which affect the body thermically, and which in that particular respect constitute climate in relation to animal life.

The body of a warm-blooded animal, of man for instance, is constantly generating heat; no doubt the amount differs in the same person under different circumstances; but it is always sufficient while he remains in health, to maintain a constant temperature of the blood, and is therefore exactly as much as he loses.

When the climatic conditions tend to rob the body rapidly of its heat, the sensation of cold is produced, because a demand is made upon it to furnish the supply more rapidly. The body must respond, or the cooling influences avoided by flying to shelter, clothes, or artificial warmth. If, on the oth r hand, the external conditions are such that the body cannot get rid of its physiologically generated warmth with sufficient rapidity, we begin to suffer inconvenience; the organism itself takes steps to lower its temperature by throwing out moisture on the surface, and if this is not enough we have to find some means by which to keep the temperature down, or sickness and death will ensue.

It will be seen, therefore, that physiologically speaking, it is the *rate at which the body cools* which interests us, and the physio-thermic influence which a climate exerts should be estimated in reference to this fact.

The meteorological elements which chiefly determine the rate of cooling, and which, therefore, are of especial importance in this consideration, are the actual temperature; the relative humidity; and the motion of the air. The first affects radiation from the warm human body, and the cooling due to contact; the second, evaporation from the skin and lungs; the third, the convection of heat from the surface, and also the rapidity of evaporation. It is the aggregate influence which these three factors exert, with some other minor ones, for which a numerical value is required.

It will be seen from the foregoing—

1. That the actual temperature as determined by the thermometer does not furnish the information sought, but must, on the contrary, often lead astray.

2. That meteorological observations as at present conducted and published, do not give data from which the physiologist or biologist can form an estimate of the thermic influence.

3. That any means by which the intensity and fluctuations of subjective temperature could be relatively expressed, would have a positive practical value.

Having thus stated the nature and salient aspects of the problem, which is obviously a difficult one, I proceed to describe the method recently adopted in Washington for its approximate solution. This consists in obtaining from a sufficiently large number of intelligent persons, their individual estimate of the sensible temperature for certain fixed hours in each day, and then deducing means from the whole record, which, it was assumed, would be a fair statement of the average opinion for that locality.

This was accomplished by conceiving the total range of sensible climatic temperature to be divided into twenty equal parts, counting upwards from the extreme of cold. To each of these divisions a descriptive expression was affixed conveying in as unambiguous a way as possible, the idea of a progressive elevation of temperature at every step. The observer, in the open air, and sheltered only from the direct rays of the sun, selects the expression which most nearly describes his appreciation of the sensible temperature at the time, and records, not the expression, but the number attached to it. From such records, means and generalizations can be obtained which increase in value as the observers increase in numbers, and gain experience.

To give this method a practical character it is necessary to reduce the trouble and inconvenience to a minimum. This is done by furnishing each observer with a little printed card for each week, which folds into so small a space that it can be constantly kept about the person. In this the entries are made. The plan will be best understood by examining the specimens lying on the table.

(INSIDE OF CARD).

SUBJECTIVE OBSERVATIONS OF TEMPERATURE.

Week ending Saturday,................................, 1876.

DATE.	7:35, A. M.		NOON.		4.35, P. M.		11, P. M.	
	Obs.	Rem'ks.	Obs.	Rem'ks.	Obs.	Rem'ks.	Obs.	Rem'ks.
Sunday........	......	....	......	....	......	....	......	....
Monday.......	......	....	......	....	......	....	......	....
Tuesday......	......	....	......	....	......	....	......	....
Wednesday...	......	....	......	....	......	....	......	....
Thursday.....	......	....	......	....	......	....	......	....
Friday........	......	....	......	....	......	....	......	....
Saturday......	......	....	......	....	......	....	......	....

....*Observer.*

(OUTSIDE OF CARD).

REMARKS.

X. Quite still.
W. Still.
V. Breezy.
U. Windy.
T. Gusty.
S. Blowing hard.
R. Gale.

Q. Very dry.
P. Dry.
O. Moist.
N. Very moist.
M. Rain.
L. Fog.
K. Frost.
I. Snow.

H. Quite clear.
G. Bright.
F. Fine.
E. Dull.
D. Hazy.
C. Lurid.
B. Overcast.
A. Dark and gloomy.

SCALE OF OBSERVATIONS.

20. Intolerably hot.
19. Excessively hot.
18. Very hot.
17. Tolerably hot.
16. Very warm.
15. Decidedly warm.
14. Agreeably warm.
13. Mild and soft.

12. Mild and fresh.
11. Quite fresh.
10. Very fresh.
9. Decidedly cool.
8. Very cool.
7. Moderately cold.
6. Cold and fine.
5. Cold and sharp.
4. Very cold.
3. Bitterly cold.
2. Painfully cold.
1. Unbearably cold.

N. B. — However qualified, these expressions must be understood to refer to equal gradations of sensible temperature only.

The inside of the card is arranged for seven days ending with a Saturday, and the hours fixed for observing are 7.35 A.M.; noon; 4.35 P.M.; and 11 P.M. These are synchronous with those employed by the signal service in Washington, for simultaneous observations throughout the country, not those best calculated for giving daily means which, as established by that office, are 7 A. M., 2 P. M., and 9 P. M. But the former are very convenient for persons employed in Government Departments, and in other ways, which the latter are not, and the daily means obtained from these will not, it is thought, differ greatly from the truth. Besides we do not really know what the best hours are, influenced as the sensible temperature so decidedly is, by variations in the force of wind, percentage of moisture, etc. The inside of the card also provides for a registration of "Remarks." These are expressed by letters which give a general characterization of the weather, quite subordinate of course to the other observations, but useful in many ways, especially in giving the observer an opportunity to express qualifying circumstances which might otherwise modify his estimate of the sensible temperature.

The outside of the card has printed upon it the "Scale of observations" and "Remarks" with their corresponding figures and letters for constant reference.

With a view to secure uniformity of endeavor amongst the observers, they have been furnished with printed instructions, which with a few recent additions are as follows: —

GENERAL INSTRUCTIONS TO ACCOMPANY OBSERVATION CARD.

Washington, D. C., June 4, 1876.

In making SUBJECTIVE OBSERVATIONS OF TEMPERATURE, it is above all things desirable to divest the mind of preconceived ideas of what that temperature ought to be. While trying to form an unbiassed estimate, the person doing so should regard himself simply as an animal with a single sense; that of heat and cold. One should also make the observation when in a passive condition, and should then select the proper expression, not doubtingly, but with careful deliberation.

Each observation should be made in as open a place as possible, changing the locality at different times of the day, so as to get out of the sun while exposed to the wind; but one must not stand in a hall-way or window, so as to be affected by a local draught; or move rapidly through the air while observing. It will often be well to go to both sides of the house before deciding, beginning and ending with the better locality. With the direct radiant heat of the sun, these observations have nothing to do, although it is often necessary to subject one's self to it for a short time, so as not to escape the cooling effects of the wind. This is especially the case in winter.

Avoid all consultation with friends relative to the observation until it is made and recorded, and never on any account alter what is written in consequence of misgivings due to the opinion of others. Never read a thermometer, or report of weather-probabilities previous to the time of observing.

Always remain sufficiently long in the open air, before selecting an expression, to feel that the warm or cool influence of the house has been thoroughly replaced by that of the outside air. One should therefore endeavor to begin the observation five minutes before the times specified, which are those adopted by the Signal Service Bureau for meteorological registrations.

As a rule it is better to be a little too early than too late. Ten minutes either way is all that can be allowed; if this limit is overstepped, make no entry.

Never under any circumstances record an observation from recollection of what the weather *was*, or from the statement of another observer, or when, from indisposition, one is conscious that his feelings are abnormal, or when absent from the District of Columbia; and relinquish the work altogether as soon as it is decidedly disagreeable and irksome.

Dress about as one would wish to be dressed (leaving style and appearance out of consideration) in the weather that prevails at the time of the observation.

The temperature of the room one leaves before experiencing the thermic influence without, is calculated to prepossess the mind of the observer in a greater or less degree, as to what he has to expect. He must try to resist such bias, and judge at all times independently. It is a good rule to fix on no number for the first two or three minutes, holding the mind without a decision, so as not to add to the difficulty by having to undo a hasty one formed on the first impulse.

When the wind is intermittent an effort should be made to express the average sensation of cold or warmth. This is sometimes difficult; it is best done by fixing on two numbers and recording one that is intermediate.

Fractional registrations must not be indulged in for the present; they indicate doubt and indecision only, not accuracy. In time observers may be able to divide the total range of temperature into thirty-nine, but not now.

After a continuance of hot weather, when a slight but sudden depression takes place, one is apt to say that it has become pleasantly cool; whereas the fall may have left the sensible temperature much higher than such a phrase expresses. The observer in such, and converse cases, should endeavor to maintain an unbiassed judgment, notwithstanding the tendency of the strong contrast to mislead him, careful at the same time to express exactly what he feels. When the change is considerable, and one is conscious of the danger of going too low, or too high, as the case may be, the best way is to *conceive* one's self coming to the existing state of things from the opposite extreme, and under that influence to make his decision.

In using the "Scale of Observations," after a selection has been made, one should test its correctness by inquiring with himself whether the next higher or lower expression could not be used with propriety; in this way a considerable degree of certainty will be acquired. Be careful when approaching the extremes of the scale to realize the number of possible gradations one is capable of distinguishing before reaching the end. This effort of the mind will prevent premature exhaustion of the expressions, and encourage moderation.

The "Scale of Observations" — which is intended for the whole country and not for any particular locality—is divided into an upper and lower group, the sensible temperature rising and falling gradually from between Nos. 12 and 13. By fixing this neutral place in the mind, and recurring to it frequently as a line of demarcation, the accurate selection of expressions will be greatly facilitated. The expressions used in this series are intended to refer to *degrees or grades of sensible temperature only*, however they may be qualified by descriptive words such as, "fresh," "fine," etc. They are, moreover, to be regarded as subdivisions of the whole range of temperature, divided into twenty equal parts, the interval between any two being the same in amount. The sooner the observer can leave the expressions, except for occasional reference, and work with the numbers alone, the better will be his determinations.

Certain observers will be conscious in themselves of a tendency to run to extremes, sometimes possibly to one extreme, while others are conservative, and resist change; one class giving to each degree too small, and the other too large a value. These natural dispositions of the mind should be recognized, and combated, not in special cases but in a general way.

No observer should attempt to correct by violent elevation or depression of the number used, any supposed constitutional peculiarity of his own; but should invariably record that which best expresses *his* personal estimate of the sensible heat or cold affecting him.

In using the list of "Remarks" one letter from each of three divisions should be selected, giving in this manner the most characteristic description of the weather, which so concise a method admits of. The observations entered in this column should always be made after those expressing sensible temperature, inasmuch as they are quite secondary and subordinate.

Observers will promote the general accuracy and final value of observations of this kind, by suggesting any interpolation or alterations which occur to them, calculated to extend, or give definiteness to the "Scale of Observations."

N. B. *Observers are requested to read these instructions carefully and thoroughly* ONCE EVERY WEEK.

The number of persons who assisted me in the prosecution of these observations has been considerable, averaging about twenty-eight up to the present time. They do not all observe at the four periods, most of them making three entries only.

As was to be expected, their observations do not always coincide for the same day and period. This is due to the fact that the system at best is but approximate, that individuals differ in their estimates of temperature, and that living in different localities, some exposed to one wind, and some to another, they are not affected similarly. Nevertheless the means obtained from this large mass of somewhat crude material, have exhibited so remarkable a degree of consistency and harmony as to justify great confidence in the method,

which certainly furnishes information relative to climate that has not been hitherto obtained.

The larger diagram here exhibited gives both graphically and in figures the mean reading for each of the four periods of each day for nine weeks; and also the daily means, maximum, minimum, and range. The smaller diagram gives weekly means for the same. A glance at the first shows the undulations in sensible temperature at morning, noon, afternoon, and night. It will be seen that with a few exceptions there is a remarkable similarity in the curves for each of these periods; that of the morning often foreshadowing all the others. For several of the weeks this is very strikingly the case. When a marked deviation occurs, there is generally some obvious cause, such as rain, alteration in the wind, etc. This is well seen in the first week (ending, June 10), where the sudden rise of Saturday morning and noon, is suddenly checked by a thunder-storm before the afternoon reading. A similar disturbance results from a like cause before the afternoon observations on June 17th, July 10th, and at other times. These curves also show the three hot weeks ending July 1st, 8th, and 15th, and the gradual rise to them from Wednesday, June 21st. The weekly means show the three hot weeks, and the general harmony of the curves in a still more beautiful and perfect manner. When it is remembered that the ladies and gentlemen who made these observations (to whom I am greatly indebted for the kind and generous interest they have taken in the work), could have had no idea of the conformity to be elicited from their labors; that in fact many of the observations now brought in juxtaposition were made by different persons; it will be certainly admitted that the results are striking and full of interest.

In relation to the progress which a number of observers collectively can make towards accuracy, it may be well to state that at the end of every week's work the observer's mean range was determined for purposes of comparison. It is evident that the range obtained by subtracting the mean of all the minima from that of the maxima would, if the observations were correctly made at one locality, be equal to zero. This, for obvious reasons, it never can be; but it is fair to assume that the smaller the mean range derived from all the individual readings, the less will be the danger of error in the reduced results, arising from any disturbance of the compensation which a large number of observers insures.

The following table gives these quantities for nine weeks.

OBSERVER'S WEEKLY MEAN RANGE.

1876. WEEK ENDING	7.35 A.M.	NOON.	4.35 P.M.	11 P.M.	MEANS.
June.................. 10	2.37	2.37	2.65	2.79	2.54
" 17	2.20	2.05	2.58	2.18	2.25
" 24	1.82	1.87	1.77	1.73	1.79
July.................. 1	2.16	1.88	2.04	2.62	2.17
" 8	2.05	1.79	1.65	2.16	1.91
" 15	2.34	1.76	1.98	2.92	2.25
" 22	1.91	1.99	2.04	2.11	2.01
" 29	1.62	1.75	1.80	1.97	1.79
August................ 5	1.64	2.01	1.88	1.20	1.68

This table shows a progressive improvement until the change to the very hot weather of the week ending July 1st, produced uncertainty and difference of opinion. Since the three warm weeks the numbers are again gradually decreasing up to the present date.

For the information of those who wish to test this method for themselves it may be stated, that while it is decidedly injurious to endeavor in any way to constrain observers by anything like dictation, much can be effected by general advice and criticism. The best commentary is undoubtedly a comparison of their own work with the means, which they can make for themselves if facilities are offered. It has been my rule to furnish each person with a manifold copy of the means at the earliest possible day, and to urge every observer to make a proper comparison of them with their own figures, of which they always keep a copy.

I hope to be able to prosecute these observations, and on a future occasion to compare the results so obtained with ordinary meteorological records, as well as with the readings of the instrument referred to at the commencement of this paper.

The Accurate Graduation of Thermometers by Comparison.

By J. W. Osborne, of Washington, D. C.

The manufacture, for scientific research, of thermometers having no correction, is both difficult and expensive. As usually conducted it involves the possession of a large stock of extra good tubing, and the selection, after a number of trials, of pieces sufficiently perfect to secure the degree of accuracy required. This selection is made by the process of calibration, a tedious operation which requires the forcing of a short column of mercury from step to step through the tube, and its accurate micrometric measurement in every position with a view to ascertain whether or not equal lengths of the bore have an equal capacity.

After such a tube is filled and sealed, the melting point of ice and the boiling point of water have to be marked upon it; both very difficult operations requiring numerous precautions, and a thorough familiarity with such manipulations as well as with the causes of error which are always present.

It is evident that physicists and chemists generally cannot themselves attempt the manufacture of standard instruments; indeed, as a matter of fact, they are only made by a very few of those habitually engaged in such work, costing them from fifteen to twenty-five times the price of ordinary thermometers.

The object of the present paper is to explain a method by which very serviceable instruments can be made by any one capable of using them.

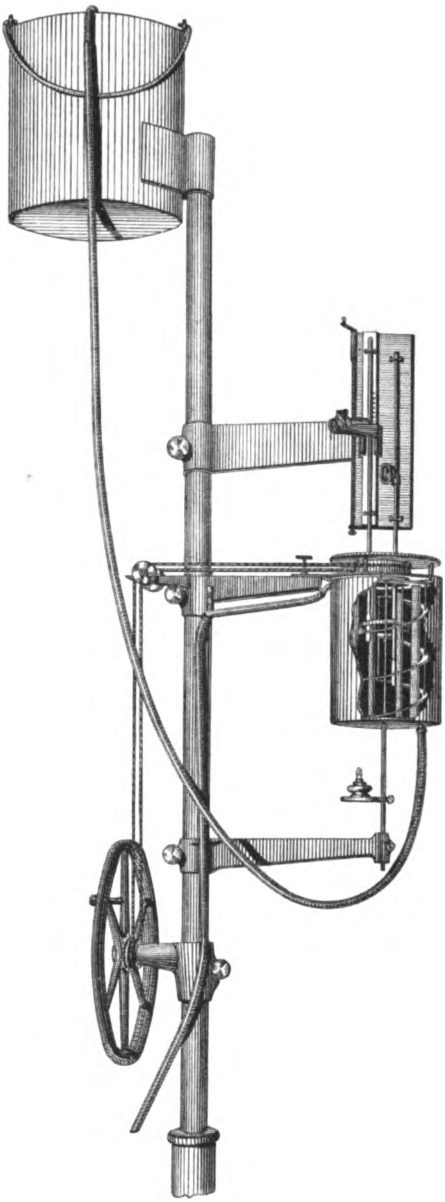
It is not difficult to get from any respectable maker an ungraduated thermometer for which a good, though by no means perfect tube has been used. The problem is to provide such a one with a scale in which the degrees shall express equal increments of heat, with no correction, or one so small that it may be disregarded.

This is accomplished by comparison with a Kew or other standard, on which perfect reliance can be placed. The method of comparison in its simple and obvious form is by no means new. It is constantly used for spirit, and short mercurial thermometers; but as ordinarily practised even in careful hands it is exceedingly unsatisfactory, and is employed from necessity, never by preference for the production of exact work.

The success of the method now to be described depends chiefly on the perfect and continuous agitation imparted to the water in which both bulbs are immersed; on the certainty with which the position of either column is read; on the absolute control over the rate at which the temperature rises or falls; and finally, on the precision with which the scratch can be made at the proper place.

To secure these advantages an apparatus is used consisting of an upright pole or staff, to which three brackets are clamped at suitable places. The upper one carries the standard and new thermometer; to the second a cylindrical vessel of thin brass to hold water, about five inches in diameter, and eight inches high is attached; while the lower arm, by means of an upright rod at its end, helps to support and steady the cylinder, carrying at the same time a small spirit lamp which slides up and down upon the rod. The way in which thorough agitation is sustained in the water contained in the cylinder, is similar to that employed in the meteorological instrument submitted by me to the section last year (1875) for the determination of sensible climatic temperature. The cover of the cylinder has a round opening in its centre, of about three inches diameter, in which revolves horizontally a short ring or collar, the upper edge of which is grooved as a pulley, while that part which passes into the cylinder is connected with a flat spiral ribbon making three revolutions to the bottom of the vessel. This spiral sweeps the inside of the cylinder without touching it. Inside the spiral rise perpendicularly from the bottom to which they are fastened, four narrow blades of thin brass: these are set radially, and leave a large space unoccupied in the centre of the cylinder to receive the stems of the thermometers. When the collar is set in motion horizontally by a little belt, which passes over two upright guide pulleys on the end of the bracket furthest from the cylinder, and down to a suitable driving pulley and fly wheel attached to the upright, a current of water is sent down the inside surface of the brass vessel, and up its centre impinging in a continuous current upon the bulbs of the thermometers, while innumerable lateral eddies strike them from the sides caused by the resistance which the stationary blades offer to the revolution of the water. This method of agitation is so complete that it is not necessary for the bulbs to be close together to experience precisely the same temperature; anywhere under the surface of the water will be found sufficient. It may be remarked in passing that it is a very difficult thing to maintain any fluid at a uniform temperature throughout its whole mass. I know of no other satisfactory way

THE GRADUATION OF THERMOMETERS BY COMPARISON.



in which it can be done, and upon success in this respect depends the value of the method forming the subject of the present paper.

The introduction of water into the cylinder is effected through a small opening in its bottom furnished with a little conical nozzle outside, and a valve consisting of a strip of rubber inside, opening upwards. Then by gravitation from a movable cistern which can be placed on the top of the upright staff, through a piece of rubber tubing, the water lifts the valve and enters, while that which is displaced finds its way by an overflow tube to a vessel below.

The thermometers are attached in any suitable way to the upper bracket, and their stems pass through perforations in a sheet of thin rubber strained upon a ring, standing with short feet upon the top of the cylinder; the object being to prevent the condensation of water upon the tubes above, which would interfere with accurate readings. The position of the mercury in the new tube is followed by a microscope with cross hair, worked up and down upon a slide by means of a long screw or rack. Close by this tube a strip of thin metal is fastened to receive the markings to be made at intervals; and on this strip, fast to the sliding part of the microscope, travels a short ruler or index, the upper edge of which is coincident with the cross hair in the microscope. Along this edge the scratch is made with an exceedingly fine needle point. The next requisite is to read the Kew standard correctly. It is impossible to do this without some contrivance which will obviate the error arising from parallax, nor can a magnifying glass be used unless its position in relation to the tube is fixed and constant throughout. As instruments of this class are invariably divided on the tube, a very simple arrangement will suffice. A small strip of thin brass is bent double, so as to fit and clasp the tube on both sides with a gentle pressure, and slide steadily up and down. A light ring soldered to the folded edge of the slide with its plane parallel to the tube, and about three-fourths of an inch distant from it, carries a watchmaker's eye-glass. Between the lens and tube, and in the axis of the former, an index projects for setting the slide to each degree, and a cross-hair is stretched a little nearer to the observer by means of which the position of his eye is determined.

The manner of using this instrument is as follows :—

The cistern above being filled with hot water it is run into the cylinder till the maximum temperature required is slightly exceeded; the agitator being maintained in motion the while by an assistant. If this temperature is near or above the boiling point, the addition of some suitable salt to the water may be necessary. The operator now waits for the temperature to fall to near the point at which the first scratch is to be made. As the mercury would in all probability pass it too rapidly, he retards the rate of cooling for the last fraction of a degree by lighting the spirit lamp, and raising the little bracket on which it stands, till the mercury appears to stand still, but yet with a positive tendency downwards: this is very easily done, a little experience enabling him to make the last one-tenth of a F. degree take several minutes to fall. As the column in the standard falls he follows that in the new tube with the cross hair, and at the moment when the transit takes place, the index on the slide of the microscope is in the position where the scratch is to be made. But it is better at once to repeat this operation, by raising the spirit lamp with the fingers for a moment till the mercury has appeared above the line upon the standard, and then watch its gradual descent a second or even a third time. When satisfied that the transit is simultaneous in both tubes, a short line should then be carefully drawn upon the temporary strip of metal. The spirit lamp is now to be depressed or removed, and the slide and index upon the standard lowered to the next place at which a reading is desired. What the interval should be depends upon the degree of accuracy required; five to five degrees F. will give excellent results, ten to ten will be quite enough for many purposes, and even larger intervals may be used. At first the time which it is necessary to wait from one interval to another will be short; but as the temperature falls it will become inconveniently long. It is easy, however, to curtail it to any extent by filling the cistern above with cold water and admitting it to the cylinder through the rubber tube. By compressing the tube between the finger and thumb, the fall of temperature is so thoroughly under control that it can be rapidly brought down to any desired reading of the standard, as with hot water it can be made to rise. When the temperature of the cylinder approaches that of the room, and from that to about 40° F., ice-water in the cistern, and ice applied to the outside of the cylinder must be used: from that temperature downwards the water from snow and salt will be necessary. For all low temperatures the readings must be made with a rising column. It is needless to say that during all these operations. the agitator must be kept in motion, increasing the speed considerably at each critical period.

Having obtained all the coincidences required, a comparison of the length of the intervals on the temporary scale will furnish definite information as to the quality of the tube, and if it be good, and great exactness is wished, I strongly recommend the repetition of the whole operation from the beginning. The short scratched lines in this case should be made without looking at the old series, and as a continuation of the latter, not superimposed upon them. The two series should not differ more than from $\frac{2}{100}$ to $\frac{3}{100}$ of a degree, and will often be found perfectly coincident if the work be carefully done. It now remains to transfer, and in doing so to subdivide the intervals upon the temporary strip of metal, to the permanent scale in the dividing engine, or by the hand of a careful engraver if the greatest accuracy is not required. If the division is wished upon the tube it must be covered with etching ground, and the transfer made to it, after which it is subjected to the action of hydrofluoric acid in the usual way.

It will be readily seen that the apparatus here described is also well adapted for the rapid and exact comparison of ordinary thermometers with a standard, and the determination of their corrections.

Description of a Tide Gauge, for use in cold climates. By John M. Batchelder, of Cambridge, Mass.

At the meeting of the Association in August, 1870, I presented a description of a tide gauge for use in cold climates. Several of the parts that were used in that apparatus may be dispensed with, and the more simple form of the present instrument adopted.

The parts that relate to the record of the height of the tide are those of the "Saxton Tide Gauge," the same now in common use at the tidal stations of the United States Coast Survey.

In that instrument the long roll of record paper, about one foot in width, is moved forward by clock-work and driving cylinders, while a carriage and attached pencil traverses at right angles with the paper and records in a curved line the varying height of the tide. In the present instrument the chain or cord that moves the pencil carriage is attached to a hollow box made of sheet copper, which floats upon the top of a column of glycerine contained in a vertical iron tube. This tube is about three inches in diameter and is firmly secured by bolts and clamps to the wharf on which the whole apparatus is placed. The bottom of the tube should be two feet below low water-mark and the top about the same distance above the level of the highest tides. It must be strong enough to withstand the pressure and friction of floating ice.

A nipple is inserted near the closed bottom of the tube, and to this the neck of an india-rubber bag is cemented and firmly secured. The bag rests upon a suitable support or platform, and is filled with glycerine, a sufficient quantity being introduced to allow it to stand at about one foot in depth in the tube, at extreme low water.

A chain or cord fastened to the top of the float is carried upward, and at a short distance above the iron tube passes around a pulley upon a shaft, which also carries a smaller fixed pulley, and from this a chain leads horizontally to the traversing platform that holds the pencil.

The glycerine rises and falls within the iron tube, in proportion to the varying height and pressure of the column of water above the rubber bag, the difference in the height of the two columns being in proportion to the difference of the specific gravity of the water and the glycerine.

A mixture of pure glycerine, and an equal quantity of water congeals at about fifty-seven degrees below zero F. The kind now commonly used in gas-meters is suitable for the tide gauge, and I find that the india-rubber bag retains its strength and flexibility after three year's exposure to the action of the glycerine.

The scale of the record made by the pencil, may be one-tenth of the actual range of the tide, and this ratio is determined by a few observations with the common tide staff, the exact adjustment being made by increasing or reducing the size of the small pulley that holds the chain leading to the traversing platform.

TITLES OF OTHER PAPERS READ IN SECTION A.

PHYSICS OF THE GULF OF MEXICO AND THE MISSISSIPPI RIVER. By C. G. Forshey, of New Orleans, La.

ON THE MOLECULAR CHARACTER OF STEAM. By S. M. Allen, of Boston, Mass.

ON THE DISTRIBUTION OF ERRORS IN NUMBERS WRITTEN FROM MEMORY. By F. E. Nipher, of St. Louis, Mo.

DISCUSSION OF THE GENERAL PRINCIPLES OF CONSTRUCTION OF ORDINARY AND PERFECT MAGIC SQUARES. By J. D. Warner, of Brooklyn, N. Y.

PROPOSED METHOD OF EVOLUTION. By J. D. Warner, of Brooklyn, N. Y.

SOLAR INFLUENCE ON THE DEGRADATION OF SOILS BY AQUEOUS ACTION. By Tyler McWhorter, of Aledo, Ill.

THE SPECIFIC GRAVITY OF LEAD.[1] By Paul Schweitzer, of Columbia, Mo.

ON THE METEORITES OF AMANA, IOWA CO., IOWA.[1] By Gustavus Hinrichs, of Iowa City, Iowa.

COÖRDINATE SURVEYING.[2] By H. F. Walling, of Boston, Mass.

A NEW FUNDAMENTAL METHOD IN GRAPHICAL STATICS. By H. T. Eddy, of Cincinnati, Ohio.

CERTAIN NEW CONSTRUCTIONS IN GRAPHICAL STATICS. By H. T. Eddy, of Cincinnati, Ohio.

[1] For the papers by Professors Schweitzer and Hinrichs, see the December (1876) number of the "American Chemist."
[2] For this paper see the March (1877) number of the "Journal of the American Society of Civil Engineers."

ON KENT'S TABLE OF ONE-QUARTER SQUARES OF NUMBERS. By S. J. Coffin, of Easton, Pa.

OBSERVATIONS ON THE DIURNAL VARIATION IN THE HUMIDITY OF THE AIR. By Hugo Hamberg, of Upsala, Sweden.

ON THE INCREASE OF INDEX OF REFRACTION ACCOMPANYING CHANGE OF TEMPERATURE. By T. C. Mendenhall, of Columbus, O.

DIELECTRIC POLARIZATION. By Elihu Root, of Amherst, Mass.

SPECTROSCOPIC OBSERVATIONS ON THE SUN'S ROTATION. By C. A. Young, of Hanover, N. H.

ON SOME RECENT SPECTROSCOPIC OBSERVATIONS OF THE ZODIACAL LIGHT. By Arthur W. Wright, of New Haven, Conn.

VOLATILIZATION OF METALS BY THE ELECTRICAL DISCHARGE. By Arthur W. Wright, of New Haven, Conn.

THE THEORY OF THE CONVERTIBILITY OF HEAT INTO MOTION NOT CONTRADICTED BUT CONFIRMED BY THE MODERN ARTIFICIAL ICE-MAKING AND REFRIGERATING MACHINES WORKED BY POWER. By P. H. Van der Weyde, of New York.

PHENOMENA PRODUCED BY THE UNION OF TWO SOUNDS. By Rudolph Kœnig, of Paris, France.

ON THE IOWA WEATHER STATIONS. By Gustavus Hinrichs, of Iowa City, Iowa.

ON THE PRACTICABILITY OF COOLING THE AIR OF BUILDINGS DURING HOT WEATHER. By Simon Newcomb, of Washington, D. C.

NOTE ON THE RADIOMETER. By T. C. Mendenhall, of Columbus, O.

RELATIVE MARKET PRICES OF GOLD AND SILVER, AND THEIR INFLUENCE ON THE METALLIC MONETARY STANDARD OF THE UNITED STATES. By E. B. Elliott, of Washington, D. C.

PRICES OF THE BONDED SECURITIES OF THE UNITED STATES AND THE CORRESPONDING RATES OF INTEREST REALIZED TO INVESTORS. By E. B. Elliott, of Washington, D. C.

EXHIBITION OF CAPILLARY COKE, FROM TRACY CITY, TENN. By N. T. Lupton, of Nashville, Tenn.

The following were read in the Subsection of Microscopy.

RESULTS OF MEASUREMENTS OF ELEVEN OF *Möller's Diatomaceen Probe Platten.* By E. W. Morley, of Hudson, Ohio.

MICROMETRIC MEASUREMENTS OF RULINGS ON GLASS BY Mr. ROGERS. By E. W. Morley, of Hudson, Ohio.

MICROMETRIC MEASUREMENTS OF RULINGS ON GLASS BY Mr. RUTHERFURD. By E. W. Morley, of Hudson, Ohio.

(1) ON A NEW SYSTEM OF FINDER FOR THE MICROSCOPE. (2) ON A FEW SIMPLIFICATIONS OF THE POLARIZING, AND OF THE SPECTROSCOPIC MICROSCOPE. (3) ON SOME MODIFICATIONS AND SPECIAL ATTACHMENTS TO THE MICROSCOPE FOR CHEMICAL RESEARCH. By P. H. Van der Weyde, of New York.

RESULTS OBTAINED BY DOUBLE STAINING OF MUSCULAR TISSUE OF *Amphiuma* WITH PICRIC ACID AND CARMINE. By George Beatty.

REMARKS ON SOME AMERICAN CONTRIBUTIONS TO THE DEVELOPMENT OF THE MODERN MICROSCOPE. By R. H. Ward, of Troy, N. Y.

SIMPLE MEANS OF ADOPTING THE BINOCULAR MICROSCOPE TO DEFECTS IN THE EYE. By W. H. Bullocks.

A METHOD FOR COMPARING THE BLOOD CORPUSCLES OF VARIOUS ANIMALS. By C. L. Mees, of Columbus, Ohio.

ON THE COMBINED COMPRESSION AND STAGE-FORCEPS CARRIER. By R. H. Ward, of Troy, N. Y.

PERMANENT

SUBSECTION OF CHEMISTRY.

ADDRESS

OF

PROFESSOR GEORGE F. BARKER,

CHAIRMAN OF THE PERMANENT SUBSECTION OF CHEMISTRY.

FELLOW MEMBERS OF THE ASSOCIATION—

LADIES AND GENTLEMEN:—

BY a resolution passed at the Detroit meeting of this Association, it was made the duty of the Chairman of the Subsection of Chemistry to prepare an address for the present occasion upon some subject of interest to the section. Before complying with this request, however, and before asking your attention to the few thoughts which I have to offer for your consideration, I desire to congratulate you upon the arrival of the One Hundredth Anniversary of our existence as a nation, and upon the very successful celebration of this important event which is now in progress in the International Exposition at Philadelphia. While the illustrations of chemical industry there exhibited are not as extensive, for obvious reasons, as those which have been seen at previous exhibitions, yet the presence of vanillin and coniferin, of resorcin, fluorescein and eosin indicates great chemical progress even since 1873. Moreover, the excellent character and the magnificent display of pharmaceutical and industrial chemicals from our own country is a matter of pride, as foreshadowing a most brilliant future.

It has also been a matter of great satisfaction to us to receive and to welcome so many chemists of eminence from foreign countries, who came here officially either as commissioners or jurors. The names of Odling and of Lowthian Bell, of Von Wagner, Vogel and Martius, of Nordenskiold and Akerman, of Paterno, von Baumhauer, De Wilde, Kjerulf, Kittary and Kuhlmann (fils) are well known to us in the literature of our science, and have been worthily enrolled upon the pages of the exhibition record. While it is our sincere regret that so many of them have already been called away by the pressure of home duties, we warmly welcome here to-day those of them who have remained to participate in our meeting.

Another event has taken place, which is of especial interest to the members of this subsection. I allude to the formation of the American Chemical Society. The movement originated in the city of New York and the preliminary meeting was held on the 6th of April last. At a subsequent meeting held on the 20th, a constitution and by-laws were adopted and a list of officers elected. The new society commences its existence under the most favorable auspices, about two hundred resident and non-resident members being already enrolled upon its books. The most cordial relations exist between the society and this subsection. To continue these relations, it might be desirable to hold the August meeting of the society jointly with that of this subsection of the American Association.

With these preliminary remarks, I pass to the consideration of the subject which I have chosen to present to you, and which I trust may prove of interest to the chemists present. I ask you to consider with me some of the ideas which exist to-day in science concerning the Molecule and the Atom, especially as they appear when viewed from the chemical standpoint. Said Professor Cannizzaro, in his Faraday lecture:[1] "I do not hesitate to assert that the theory of atoms and molecules ought to play in the teaching of chemistry a part analogous to that of the theory of vibrations in the teaching of optics." Clearness of conception on our part, being essential to clearness of statement before those whom we instruct, an attempt to state this theory of atoms and molecules cannot fail to be of service in so far as it is successful.

The best evidence for the existence of matter is its necessity. Hence eminent authority defines it as "that which is essential to the existence of the known forms of energy, without which, therefore, there could be no transformations of energy."[2] The divisibility of matter is at once a metaphysical and a physical question. It is obvious everywhere that masses of every form of matter known—saving, perhaps, the light-ether—are capable of division with more or less readiness. But is there any limit to this division, other than the imperfection of the means employed? Here physics and metaphysics are at variance, and the former boldly avers that there is a limit. A fragment of salt for example, sustains subdivision only to a certain extent. Divide it but once again, and the salt, as such, disappears, and in its place we have the two new substances, sodium and chlorine. This limiting particle is called a molecule. It is the smallest particle of any substance which can exhibit the chemical properties of that substance. The aggregation of molecules constitutes a mass; hence the molecule is the physical unit—the ultimate particle or centre of the physical forces.

What now in the light of modern physics, is the molecule? What is its size and weight? And is it at rest or in motion? The relative size of material molecules has long been known and forms the starting point of chemical investigation. The law of Avogadro, enunciated by him in 1811,[3] but more recently placed by Boltzmann on a surer foundation,[4] asserts that in equal volumes of all gases there is the same number of molecules. The proof of this law is found jointly in that of Boyle, published in 1662,[5] and in that of Charles published a short time after.[6] The former of these laws states that the volume of any gas is inversely as the pressure to which it is subjected; and the latter that the co-efficient of dilatation by heat is nearly the same in all permanent gases. Since all gases are alike in these respects, they must be alike in their molecular constitution upon which the phenomena depend. But if it be true that equal volumes of all gases contain the same number of molecules, it is also true, as a necessary deduction, that all gaseous molecules must be of the same size. The relative size of molecules is therefore determined.

The determination of the absolute size of a molecule, even approximately, would have been but a few years ago an entirely hopeless task. But now the problem has been attacked and solved, and numbers are given for the sizes of molecules which agree excellently well even when deduced from widely different data. The best results of this sort which have been obtained are those of Thomson and Maxwell.[7] To comprehend their reasoning it is necessary to premise the heterogeneity of matter. "A body is called homogeneous," say Thomson and Tait,[8] "when any two, equal, similar parts of it, with corresponding lines parallel and turned towards the same parts are undistinguishable from one another by any difference in quality."* But if we apply this definition to matter without any limitation as to the size of the parts, there is no reason to believe that any form of matter known to us is homogeneous. More than thirty years ago Cauchy showed from a mathematical investigation of dispersion, "that in palpably homogeneous bodies such as glass or water contiguous portions are not similar when their dimensions are moderately small fractions of a wave length of light."[9] This result, it is evident, is the same with that obtained above when by continual sub-division of a mass, the molecule was reached as a final particle. The heterogeneity of matter then in this sense, lies in the fact that it is made up of molecules separated by intervening spaces; a structure without which dispersion is impossible. Now by the discovery of Cauchy, just given, inasmuch as we know the length of a wave of light, we may by assuming the small fraction spoken of, arrive at an approximation to the size of a molecule. Taking the wave length at 1-2,000 of a millimeter, and assuming that the distance from the centre of one molecule to the centre of the next cannot be less than 1-10,000 part of this value,[10] we have one twenty-millionth of a millimeter as the diameter of the molecule; *i. e.*, the measure of the coarse-grainedness of matter.

A second method of determining the approximate size of material molecules, is one which we owe to Sir William Thomson.[11] It is founded on the important fact, asserted by Volta, but denied until put beyond dispute by Thomson, that when a zinc and a copper plate are placed in contact, either directly or by means of a connecting wire, the zinc is positively and the copper negatively electrified. Since, therefore, the two bodies attract each other, it is clearly possible, by measuring the attraction, to calculate the amount of work which would be done by their coming together. A plate of zinc and a plate of copper one centimeter square in metallic connection separated by one hundred-thousandth of a centimeter, attract each other with a force of two grams. The work done in bringing these plates into this position is two hundredthousandths of a centimeter-gram. The work done by electric attraction in forming a pile of fifty thousand such plates would be two centimeter-grams. But by the law of Joule, this work is equivalent to an amount of heat sufficient to heat the mass 1-16,120 of a degree. If, however, the space between the plates be reduced to a hundred-millionth of a centimeter, the heat generated would raise the mass by 62°; and if to a four-hundred-millionth, by 990 times the amount required to warm it 1 °C. Now as this is a far greater amount than that actually produced by their chemical union, the inference is a fair one that we have exceeded the limit of the heterogeneity in these metals. Hence the coarse-grainedness of this matter, and hence the molecular magnitude, does not probably fall below one thirty-millionth of a millimeter.

A third calculation of the size of molecules is founded on data obtained from the soap-bubble film, and is also due to Thomson.[12] The force with which such a film contracts — shown commonly by using the stream of air issuing from the stem of a pipe on the bowl of which is such a bubble, to blow out the flame of a candle — is a measure of the work done in stretching it in units of force per unit of breadth. In the case of pure water, this contractile force is about sixteen milligrams weight per millimeter of breadth. Hence the work done in stretching it, measured in millimeter-milligrams, is sixteen times the increase of area in square millimeters, provided only that the contractile force is not reduced by this diminution in thickness of the film. To prevent the fall of temperature which would necessarily accompany the drawing out of this film, it has been demonstrated that about half as much more energy in the form of heat must be supplied to it. Hence for every square millimeter added to the area of a film of water whose temperature remains constant, its total energy is increased by twenty-four milligram-millimeters. A film one millimeter thick requires, to extend its area ten thousand and one fold, an expenditure of work for each square millimeter of the original film — or each milligram of its mass — of 240,000 millimeter-milligrams. Its thickness would thus be reduced to a ten thousandth of a millimeter, and the temperature of the whole would be raised by the heat-equivalent of this work, by only half a degree centigrade. But experiment proves that there is no diminution of the contractile force with this thinness of film, and hence that the number of molecules in its thickness is considerable. If, however, we extend the film yet more, so that its thickness is reduced to a twenty-millionth of a millimeter, the work expended in doing this is two thousand times greater, and its heat-equivalent would be 1,130 times that required to raise its temperature by 1°C. Now since far less work than this would be sufficient in the form of heat to destroy the liquid as such (and of course its contractile force) and convert it into vapor, it is clear that the contractile force of a water-film diminishes greatly before it attains a thickness of one twenty-millionth of a millimeter. As such a diminution is inconceivable so long as there are several molecules in the thickness of the film, it follows that there are not several molecules in the twenty-millionth of a millimeter.

The fourth and last method is one based by Thomson,[13] and subsequently by Maxwell,[14] on the phenomena of gaseous diffusion. It is well known that if one gas be placed in presence of another, the lighter being uppermost, after a time, longer or shorter according to the density of the individual gases mixed together, they will be found thoroughly intermingled. Loschmidt,[15] in 1865, from his experiments, and subsequently Clausius,[16] on theoretical grounds based on the theory of molecular motion in gases, which we shall presently refer to, has proved that "the average length of the free path of a particle from collision to collision, bears to the diameter of each molecule, the ratio of the whole space in which the molecules move, to eight times the sum of the volumes of the molecules." Hence the number of molecules in the unit of volume is equal to the square of this ratio, divided by the volume of a sphere whose radius is equal to the average length of free path. If we assume with Maxwell, that in a liquid, the volume of any substance is nearly that occupied by the molecules themselves in contact, we see that, since experiment renders doubtful the condensation of any known gas to one forty-thousandth of its volume without reducing it to a liquid, the ratio of the volume of a gas to the combined volume of all the molecules contained in it is as 40,000 to 8 or as 5,000 to 1. But this by the statement of Clausius above given is the ratio which the length of the free path of a molecule of gas bears to the diameter of this molecule. If then we accept one ten-thousandth of a millimeter as the length of the free path, which is the value given by Joule and Maxwell, it follows that as this length is five thousand times that of the diameter of the molecule, the diameter of the molecule must be one fifty-millionth of a millimeter.*

Moreover, the number of molecules in a cubic centimeter of any gas, being the quotient of the square of the above ratio, $(5{,}000)^2 = 25{,}000{,}000$, divided by the volume of a sphere whose radius is one hundred thousandth of a centimeter, cannot be greater than six thousand million million million (6×10^{21}). In the case of liquids and solids, which vary in density from five hundred to sixteen thousand times that of atmospheric air, the number of molecules in a cubic centimeter may vary from three million million million million to a hundred million million million million $(3 \times 10^{24}$ to $10^{26})$. And the distance from center to center of these molecules, assuming that they are arranged in the form of a cube, would be from one fourteen-millionth to one forty-six-millionth of a millimeter.

From the rather remarkable coincidence of the results which have just been obtained from independent and widely different data, it may be concluded with a high degree of probability that in ordinary liquids or solids the diameter of the molecule is less than the ten millionth, and greater than the two hundred millionth of a millimeter.

The next point of interest concerning the molecule, is its weight. The relative weight of molecules is readily determined and is frequently used in the fixing of rational formulas. From the deduction from Avogadro's law already given, that equal volumes of all gases contain the same number of molecules, it necessarily follows that the weight-ratios of equal volumes of all gases must be also the weight-ratios of their molecules, which is Gay Lussac's law.[17] Because for example, a liter of hydrogen weighs ·0896 gram, and one of oxygen weighs 1·4298 grams—the weight-ratios here being 1:16—it follows that the molecule of oxygen must be sixteen times as heavy as the molecule of hydrogen. If the hydrogen molecule be assumed as a standard, the molecular weight of any gas will be represented by its weight-ratio. Since, for reasons presently to be given, the molecular weight of hydrogen is called two, the molecular weight of any other substance in the state of gas will be twice the weight-ratio; *i. e.*, twice the density. As

*Loschmidt (loc. cit.) gives one-millionth of a millimeter as the diameter of an air molecule. Maxwell (loc. cit.) gives one two-millionth of a millimeter as the diameter of a hydrogen molecule.

the molecule does not lose or gain matter when the physical state is changed, the molecular weight of a body in the liquid or solid state is the same as in the gaseous. As to the absolute weight of a molecule, it may be readily obtained, of course, by dividing the weight of a cubic centimeter of the substance in the gaseous state, by the number of molecules contained in that volume of hydrogen, as already given. This has been done for several gases by Professor Maxwell.[18]

There is one circumstance connected with this question of molecular weight which deserves some attention. I refer to physical isomerism. Several years ago in his excellent researches on radiant heat, Tyndall observed discrepancies between the absorptive power of gases for heat as obtained by experiment, and that which would have been predicted on the theory of molecular structure thus far assumed.[19] If it be true, as the modern theory supposes, that radiation is but the communication of molecular vibrations to the ether, and absorption, which is its precise correlate, only the reception of motion from it, then it is obvious that radiating and absorbing power should be increased by molecular complication. And this in general is what Tyndall's experiments showed to be true; olefiant gas being a better absorber than marsh gas, marsh gas than carbon dioxide, and carbon dioxide than nitrogen or any simple gas. But there are exceptions to the rule; thus the absorption of hydrogen being unity, that of chlorine is 60, that of bromine is 160, and that of hydrobromic acid is 1005, although the molecular structure of all these bodies is commonly regarded as similar. A consistent application of the theory of molecular complication would require us to suppose aggregations of simple molecules which exist and act as the absorbing and radiating particles. This opens the question at once: Is the chemical molecule, which is defined as the least collection of atoms which can exist in the free state, identical with the physical molecule? Or are there varieties of the latter made up of several of them united into one? The assumption that there are such varieties, is the basis of the theory of physical isomerism. It has been invoked to account for the different rotatory action on polarized light of chemically identical forms of amyl alcohol and other bodies.[20] But the most remarkable instance of it, and one which, it must be admitted goes far to establish the hypothesis, is the recent discovery by Laubenheimer[21] of four forms of nitrometachlornitrobenzene, three of which are solid and one liquid. All these forms are chemically identical, and indeed are capable of conversion by very simple means, the one into the other. But physically they are entirely distinct. Of the solid forms, two crystallize in the monoclinic system, though with different axis-ratios and different angles of inclination, the other crystallizes in the orthorhombic system. The α form fuses at 36 ·3°, the β form at 37 ·1°, and the γ form at 38 ·6°. The discoverer explains these phenomena very properly upon the hypothesis of Naumann,[22] that the crystal molecule of the more stable modification is formed from a greater, the less stable from a smaller number of chemical molecules.

A final question remains to be asked concerning molecules—are they at rest or have they relative motions among themselves? Just at the close of the last century, Rumford proved most conclusively that work could be converted into heat.[23] And as the conversion seemed perpetual, he argued most acutely that heat was not matter but motion. It is entirely unnecessary for us to stop here to give the proofs of the dynamic theory of heat. The magnificence of the science of Thermo-dynamics which rests upon it, is the best evidence of its truth. But there is one phenomenon worth considering for a moment, because it furnishes direct proof of this motion. This is the phenomenon of diffusion. The interpenetration of one gas by another, even through a porous partition, is due, according to the kinetic theory of gases of Clausius,[24] to the actual passage of molecules from one into the other in virtue of their actual velocities. In gases, all molecules move in straight lines, and hence impinge against each other and against the walls of the containing vessel. The pressure then which a given volume of gas exerts upon a given surface is a function both of the number and of the velocity of the molecules. If we increase the number of molecules in a given volume, *i.e.* the density, we increase the pressure proportionately. This is the law of Boyle.[25] If we increase the velocity of the molecules, *i. e.* the temperature, we cause expansion, since the number of impacts is increased. Moreover since all gases have the same number of molecules in equal volumes, they all expand equally for an equal increase of temperature. This is the law of Charles.[26] In liquids as well as in gases, we have the phenomenon of diffusion due to the same cause. But since the molecular motion is very much restricted, the diffusion takes place with corresponding slowness.

Granted now the motion, what are its quantitative relations? How fast do molecules move? The first answer to this question was given by Joule[27] who sought to ascertain by calculation what the molecular velocity must be in hydrogen to produce in a given volume the pressure observed. The result showed that in this gas at the ordinary temperature and pressure, the velocity of mean square of the hydrogen molecules was 6,097 feet per second, or about seventy miles a minute. As they move in straight lines, and as the length of the free path between two successive encounters is only from one twelve thousandth to one twenty thousandth of a millimeter, it may easily be calculated that the number of collisions made by each hydrogen molecule in a single second is seventeen thousand seven hundred million. Maxwell has calculated these values for oxygen, carbonous oxide and carbonic dioxide.[28] Calling the velocity of hydrogen 1,859 meters per second, that of oxygen is 465 meters, that of carbonic oxide 497, and that of carbonic dioxide 396. The number of collisions made by each molecule per second is, for oxygen 7646, for carbonic oxide 9489, and for carbonic dioxide 9720 millions. In air the number of collisions made by each molecule is only one-half of that above given for hydrogen, and the average molecular velocity one-fourth as great as for the latter gas.

Having now discussed pretty fully the molecule, we come to ask of it as we did before of the mass — is it divisible? In the case of the salt, to use our previous illustration, we observed that so soon as the molecule was reached, further subdivision produced particles of matter entirely unlike salt, called chlorine and sodium. The evidence that these particles are really smaller than molecules, is found in Hofmann's argument for the composition of hydrochloric acid.[29] Suppose a certain volume of hydrogen to contain 1,000 molecules; then the same volume of chlorine will contain 1,000 molecules also. If now these two volumes be mixed and then caused to combine, there will result two volumes of hydrochloric acid gas containing 2,000 molecules. If now the new gas be analyzed, each molecule of it will be found to contain both hydrogen and chlorine. The particles of chlorine and the particles of hydrogen must therefore be less than the molecules of which they are parts. As each molecule of the compound gas contains one atom of chlorine and one of hydrogen, the 2,000 molecules must contain 2,000 of each. But the 2,000 atoms of hydrogen came from the 1,000 molecules, and the same is true of the chlorine; hence a molecule of hydrogen is composed of two atoms. An atom then is the smallest portion of matter which can be reached by nature's processes of subdivision. It is generally defined as the smallest particle of simple matter which can enter into the composition of a molecule.

Let us pause here a moment to say that no metaphysical conception at all attaches to the modern idea of atom. With the question whether it can be divided, chemistry does not concern itself. The word atom came into use to express a universally conceded fact expressed in the law of definite proportions; namely, that a certain definite quantity of matter by weight combines with a similar definite quantity of some other matter. The smallest quantity of any substance which is found ever to enter into combination, is called an atom. No real objection, it would seem, can lie against the idea of atom when defined in this way. If we concede that the molecule has as real an existence as a mass, I see no reason for not conceding the same to the atom.

The first point of chemical interest about an atom is its weight. The relative weight of an atom referred to that of hydrogen as the unit is called its atomic weight and is one of the most important of chemical constants. To ascertain the atomic weight of an element, two distinct processes are required: First, the ratio in which it combines with some other substance whose atomic weight is known, is necessary; and second, the molecular weight of the compound analyzed, must be obtained. Thus, given marsh gas, to fix the atomic weight of carbon. Analysis shows that in 100 parts of marsh gas there are seventy-five parts of carbon and twenty-five of hydrogen; which is in ratio of 3 : 1. Three parts of carbon then unite with one of hydrogen. By the balance one liter of marsh gas weighs 0·716 gram, or eight times as much as a liter of hydrogen. Hence its molecule must be eight times as heavy; and as the hydrogen molecule weighs two, the molecular weight of marsh gas is sixteen. Now of these sixteen parts, we have shown above that three-fourths is carbon and one-fourth hydrogen. One molecule of marsh gas contains, therefore, twelve parts of carbon and four parts of hydrogen. On comparing this quantity of carbon with that contained in other single molecules into which carbon enters, it is found to be the smallest. By our definition therefore, twelve being the smallest quantity by weight in which carbon enters into the formation of a molecule, is its atomic weight.

The absolute weight of an atom is a datum of no practical value at present in chemistry. Yet as a matter of curiosity as showing the minuteness of the quantities with which we are dealing, it may be worth while to calculate it from the data already given concerning molecules. If we take Thomson's estimate, a cubic centimeter of hydrogen cannot contain more than six thousand million million million molecules. As now this cubic centimeter weighs ·0896 milligram, it is obvious that the weight of one molecule of hydrogen is ·0896 divided by (6×10^{21}) milligrams. This is equal to $\cdot 015 \div 10^{21} = \cdot 000.000,000.000.000,000,000,015$. As an hydrogen atom weighs one-half as much as its molecule, we infer that its weight cannot be less than ·000,000,000,000,000,000,000,0075 milligram (being seventy-five ten million million million millionths of a milligram). On Maxwell's hypothesis,[30] that a cube centimeter of hydrogen contains but nineteen million million million molecules, the weight of an atom would be considerably more; namely, two hundred and thirty-five hundred thousand million million millionths of a milligram or about three hundred times as great.*

Our second question concerning the atom refers to its motion—have the atoms within any molecule a motion independent of that of the molecule itself? Since, while all molecules in the gaseous state are of the same size, they contain widely different numbers of atoms, and this without interfering apparently with each other, it follows that the spaces separating the atoms within the molecule must be far greater than the diameter of the individual atoms. The *a priori* argument therefore, renders highly probable the existence of atomic motion within the molecule. And the actual facts observed justify this conclusion. Clausius has shown on the dynamical theory of heat that the total energy of all the motions in a gas is proportional to the absolute temperature.[31] But the total energy of a gas is made up of that due to the progressive motion of the molecules as a whole, and of that arising from the vibratory and other motions of the constituent atoms. And as these are parts of a connected system, it is evident that there must be a definite ratio between the total energy of any gas and the energy of the progressive motion of its molecules, which is the measure

* *Annaheim*, Ber. Berl. Chem. Ges., ix, 1151, 1876.

of the temperature. This ratio has been actually measured in the case of air and of several other permanent gases. The most satisfactory evidence, however, of the existence and of the nature of atomic motion is afforded by the phenomena of spectrum analysis.[32] Since within certain limits the atoms within any molecule do not part company, it is clear that their motion must be of the nature of a vibration or a rotation, while that of a molecule is progressive. It is obvious then that molecular motion must be excessively complicated and its expression entirely beyond the powers of analysis. But on the other hand, atomic motion is simple, being either directly harmonic, or capable, by Fourier's theorem, of resolution into a definite number of harmonic motions. Consider now what takes place in a gas at ordinary temperatures. The molecules, moving with great velocity, are continually impinging upon each other. The shock of the impact not only alters the direction of the moving molecule, but it sets into motion its atomic system; and this vibration which is harmonic, preserves its character during the whole of the free path it traverses, or until the next encounter. While, therefore, the amplitude of the vibration is determined by the force of the collision, the period is fixed by the constitution of the molecule. If, now, the temperature of the gas be raised, the velocity of its molecules will be increased, and with the increased force of collision, the amplitude of the atomic vibration will become greater. And if this vibration, when its amplitude becomes sufficient, be communicated to the surrounding ether, it will produce therein waves of a definite length and refrangibility. These, when analyzed by a prism, will give a spectrum consisting of bright lines corresponding to these refrangibilities. The spectrum lines of the elements then, represent simply disturbances of the ether by regularly vibrating molecules. Hence the spectrum becomes a test for the rates of vibration of the atoms within the molecules; and, therefore, since the period of vibration is fixed for every atom, a test for the atoms themselves wherever situated, provided only their light can reach the eye. The period of the hydrogen atomic motion in the laboratory, coincides exactly with that which takes place on the sun, on Sirius, or even in the remotest nebula of space.

The greater the tenuity of the gas, the longer obviously is its free path, and the greater the time during which the harmonic vibration is uncomplicated by collisions. Hence the purity of the spectra of rare gases such as hydrogen, and of all gases when rarefied artificially. If, on the other hand, the density of a gas be increased, the free path is shortened, the collisions become more frequent, the regularity of the vibration is disturbed, and the spectrum lines widen to bands. Finally, the molecule has little or no free path, its motion becomes complicated, waves of all lengths are produced, and as Frankland and Lockyer have shown,[33] the spectrum becomes continuous. The same result is attained by an increase of temperature. The increased velocity of the molecule increases the shock of collision and consequently the amplitude of the vibration. At the same time waves of shorter period are developed. So that the final result is a continuous spectrum as before. In the case of liquids and solids, as we have seen, the molecules have scarcely any free paths. Whence it follows that liquids and solids when incandescent, can give only continuous spectra. This is a well known fact.

A third argument for the existence of atomic motion is based on the phenomenon of dissociation and on the mechanical theory of electrolysis. The opinion has been expressed by Clausius[34] that under certain circumstances, the atomic vibration developed by molecular collision may be so intense as to overcome the attraction by which these atoms are held together, so that they go wandering about seeking new partners. If the conditions are such that during a given interval of time, as many of these atoms unite as separate, and this in the same way, then evidently there will be no change in the composition of the gas. But if the temperature is so high that there are more separations than unions, the unions being less complex, then there is dissociation. The same assumption Clausius makes use of to explain electrolytic decomposition.[35] If, while this rapid decomposition and recomposition of molecules is taking place, some directive force be introduced, tending to urge certain of the atoms in one direction, and others in the opposite, there will obviously be a tendency for similar atoms to unite since they find themselves associated. Hence electrolysis produces simpler molecules from complex ones. Wiedemann has adopted this hypothesis and has extended it, by comparing the phenomena of electrolysis to those of diffusion.[36] He believes that the electric conductivity of an electrolyte depends upon the coefficient of diffusion of its constituent elements through each other.

The power of atoms to enter into combination with each other is usually ascribed to a force of attraction resident in them. The combining power of an atom is distinguished: first, by the quality of its action; and second, by the quantity of this action. The first of these was prominently recognized in Berzelius's electrochemical system,[37] and the elements were classified as relatively positive and negative, according as they were evolved in electrolysis at the negative or the positive pole. The entirely distinct character of these two parts which the elements may play, and the wide differences of properties which are apparently due solely to this difference of quality in the combining power, go far to give it a prime importance. But why, for example, the union of hydroxyl to chlorine should yield an acid, and to potassium a base, we cannot as yet form any idea. The quantity of the combining power possessed by an element, as at present used in chemistry is somewhat ambiguous, it being employed in two different senses. In the first place, it signifies the quantity of a standard element with which the atom can combine; and under this definition, is commonly called equivalence. Its second signification refers to the strength of the attraction with which an atom holds another of whatever kind; this conception is fixed in the word chemism. There is at present no known relation between the two quantities now defined. An atom may have a large equivalence and at the same time its chemism may be very feeble. Indeed from certain facts it would appear that in a certain sense, the chemism of a body is inversely as its equivalence.

The equivalence of an atom is measured always by the number of hydrogen atoms with which it can combine or which it can replace. But as the whole value of the idea depends upon the invariability of the standard, and as there is no good reason why the hydrogen atom alone of all the sixty-four at present known, should have an invariable equivalence, it cannot be long before the conception of equivalence will be merged in some higher generalization. There is no question that the theory now under discussion has been and still is, of the greatest service in chemical science, the whole superstructure of organic chemistry resting on the tetrad character of carbon first established by Kekulé.[38] Notwithstanding all this, it is impossible to form any conception of its real essence. Why, for example, should the equivalence of an atom vary by two, so that when once even it always remains even? Again, what shall determine the upper limit of equivalence? The highest equivalence of any atom which has thus far been directly ascertained is six in tungsten, when it forms the hexachloride; though indirect evidence renders seven, and even eight probable. Clarke, in a paper read before this section last year,[39] came to the conclusion, from geometrical considerations, that the highest equivalence possible to any atom is twelve.

We have already defined chemism as an attraction between atoms in view of which they unite with one another. Relatively, approximate measures have been made of the value of the chemism for different atoms; but no determination of its absolute strength has yet been given. One of the most successful of the early attempts to measure the chemism of atoms is that of Berthollet. In his "Statique Chimique"[40] he formulates the two laws known by his name, which have been of much service, especially as modified by Dumas.[41] He rejected the idea of an "elective affinity," and showed that when a base in solution was acted on by several acids it was divided among them, the activity of each acid being proportional to its chemical mass; or, as we should say to-day, to the product of the reciprocal of its equivalent by the number of equivalents.[42] In the light of present science, we see that he mistook physical for chemical phenomena, and that his laws are founded upon molecular rather than upon atomic attraction. Let me observe here that the tables of affinity so-called, found in text-books, though now fortunately more rare than formerly, are useless for the same reason.

For nearly all the light which has been thrown in recent years upon chemism, we are indebted to electrical and thermal investigations. The magnificent development given to the science of energy during the past one or two decades, while it has not enabled us to approach nearer to the essence of attraction, has yet enabled us to measure it accurately in terms of work. The strength of gravity is expressed in units of work or foot-pounds, the force being greater according as more work must be done against it in moving a body of given weight through a given distance. But as Joule has shown that one heat-unit is mechanically equivalent to 772 foot-pounds,[43] it is evident that the force may be measured in terms of the heat absorbed in overcoming it, or in terms of any other force so employed.

Considering in the first place, electrical investigations in this direction, we find that the force of chemism has been shown by

Thomson to be capable of expression in a perfectly definite measure, that measure being electromotive force.[44] The numerical value, he says, of the electromotive force which expresses the resultant chemism involved in a given reaction, is equal to the mechanical value of the whole heat evolved during this reaction, when one electro-chemical equivalent of each substance enters into the combination. The electro-chemical equivalent here referred to is the same as that derived by Faraday from electrolysis, and according to which he classified the elements.[45] It may be defined as that quantity of matter which has the same combining or replacing power as one part of hydrogen by weight. If for example an electric current of precisely the same strength acts upon hydrochloric acid, water, and ammonia, for every molecule of hydrochloric acid decomposed, one-half of a molecule of water and one-third of a molecule of ammonia will suffer decomposition. It will be noticed that the quantity of hydrogen set free is the same in each case, and hence that that of the other constituent — being the quantity which combines with one part of hydrogen — is its electro-chemical equivalent. By this rule, therefore, the quantities represented by H, Cl, $O_{\frac{1}{2}}$, $N_{\frac{1}{3}}$, $(SO_4)_{\frac{1}{2}}$, are electro-chemical equivalents of these bodies. Precisely as, in accordance with the theory of energy, force is evolved in chemical union, so in electrolytic separation, it is absorbed. Hence there is developed in the circuit a counter electromotive force which is proportional exactly to that fraction of the energy which is transformed into chemism.

In case there is no resistance in the external circuit—and consequently no work done there by the current — all the heat evolved appears in the battery cells themselves; while, if chemical decomposition takes place in the external circuit, there is less heat in the battery, the kinetic energy of heat being converted into the potential energy consumed in overcoming chemism.

But the most light which has been thrown on chemism, has been that derived from thermo-chemical investigations, particularly those of Berthelot[46] and Thomsen.[47] In these investigations the object has been to make a careful study of the thermal changes which take place during chemical reactions. The results which have been obtained are formulated in three most important laws by Berthelot, which are as follows:[48]

1st. The amount of heat set free in any chemical reaction whatever is a measure of the total work, both chemical and physical, accomplished in the reaction.

2d. If a system of bodies, either simple or compound, taken under definite conditions, undergoes physical or chemical changes capable of bringing it to a new state without producing any mechanical effect exterior to the system, the amount of heat which is set free or absorbed as the effect of these changes, depends only on the initial and final state of the system; and remains the same whatsoever be the nature or the order of the intermediate stages.

3d. Every chemical change which is effected without the aid of foreign energy, tends to the production of that body or system of bodies which evolves the most heat.

The value of these laws to the elucidation of all questions of chemical dynamics, can hardly be overestimated. We have now, however, to concern ourselves only with the third, in illustrating the subject under discussion. In the first place, it should be observed that this law is a necessary consequence of the theory of energy. The amount of energy which a body has is a measure of the force with which it can combine. If such a body in combining does not part with all its heat, it will have the power of entering anew into combination; *i. e.*, it will be unstable. Hence the larger amount of heat which the body loses in combining, the more stable the compound which is formed. The analogy with mechanical equilibrium is complete.

The illustrations of this third law, which are given by Berthelot, are numerous and interesting. Considering, in the first place, direct combinations, he shows that while the union of nitrogen dioxide with oxygen to form the trioxide evolves ten calories, its union with oxygen to form the tetroxide evolves seventeen. Hence the latter is always formed in the presence of an excess of oxygen. Tin, in forming stannous oxide, evolves 36.9 calories, in forming stannic oxide, 72.7; the latter is the body universally formed. Hydrogen sets free, in forming water, 34.5 calories; but in forming hydrogen dioxide, only 23.5; hence it is the former which hydrogen gives on burning. In confirmation of this statement is the fact that the formation of the dioxide is attended with an absorption of heat; and hence that it cannot be produced from water and oxygen without the intervention of foreign energy. On the other hand, the oxides of nitrogen and of chlorine, nitrous chloride, acetylene, and marsh gas are all formed from their elements with the absorption of heat. None of them can be formed by the aid of the energy contained in their elements alone; the assistance of foreign energy is required, electrical in the case of acetylene and marsh gas for example, and the case of the oxides of chlorine, energy derived from the simultaneous formation of a metallic chloride which evolves heat. Chemical decompositions also furnish many striking illustrations of this law. If the formation of a body which absorbs heat cannot take place without the aid of foreign energy, so a body formed from its elements with the evolution of heat cannot decompose without the aid of foreign energy; either that of heat, light, electricity, chemism, or of physical disaggregation. If the compound has been formed with absorption of heat, however, it is either spontaneously decomposable—like the chlorine oxides and nitrogen chloride—or it is readily susceptible of undergoing changes attended with the evolution of heat—as acetylene, cyanogen and nitrogen dioxide. In relation to the question of substitution, the law says if a body A, in uniting with a metal produces more heat than another B, then A will displace B. Because chlorine, in uniting with the metals, sets free more heat than bromine or iodine, it displaces these bodies from their metallic combinations. Whenever a metal displaces another from a compound it is because the production of the new salt corresponds to an increased evolution of heat. The phenomena of double decomposition give rise to some curious applications of the law. Dry hydrochloric acid gas acting on dry mercuric cyanide decomposes it at once since it evolves 5·3 calories. But in solution, hydrocyanic acid decomposes at once mercuric chloride, since in this reaction 15.5 calories are set free. So silver chloride and hydriodic acid give silver iodide and hydrochloric acid, a total decomposition, whether gaseous or in solution; while silver iodide and chlorine give at once silver chloride and iodine, an apparent reversal of attraction. But on the one hand, chlorine evolves fifteen calories more than iodine in uniting with silver; and on the other hydriodic acid evolves 31·8 calories liquid, and 51·3 gaseous in acting on silver oxide, hydrochloric acid evolving only 20·6 and thirty-eight calories respectively. We must not, however, extend this portion of our subject to any greater length. Enough has been said to show the controlling agency of thermo-chemical considerations in any theory we may form of chemism.

It must be confessed, however, that notwithstanding all the assistance which physics has rendered chemistry, in helping it to a rational conception of its fundamental attraction, the real underlying essence of this attraction remains as completely hidden as before. But this fact should by no means be considered a discouraging one when we remember that we know absolutely nothing of the cause of any other form of attraction, nor even whether the force resides in the attracting body itself or is caused by outside agencies, as is now more than suspected. Indeed, the cause of the attraction of gravitation is itself unknown. But two modes of accounting for it, consistent with present ideas, are suggested; the one that it is due to differences of pressure in a substance continuously filling all space, the other that it is due to the impact of particles. The latter hypothesis suggested long ago by LeSage of Geneva, has received a large share of attention in recent times.[49] It supposes that the particles of matter are subject to bombardment by an infinite number of still minuter particles which are darting about with inconceivably great velocities. As each material particle screens its fellow on one side from this rain of particles, the final effect is to force them together. It may be easily shown that the result of this action is an apparent attraction which follows exactly the law of gravitation; *i. e.*, varying as the inverse square of the distance, and—admitting the free passage of most of the particles through the matter—being directly as the product of their masses. Chemical and physical attraction are bound up in the same category with the attraction of gravitation; he who succeeds in explaining the one will have found the key of the whole.*

Finally, we have to ask what is this atom about which we have said so much? Has science any conception of such a thing? This brings us to the most important atomic theory of the day, the theory of vortex-atoms of Helmholtz, as worked out by Thomson.[50] In his investigations upon the laws of fluid motion, Helmholtz[51] examined exhaustively the equations of motion of an incomprehensible frictionless fluid admitting that this motion could be rotational. He proved that any portions of such a fluid which possess rotation must possess it forever, and are thus isolated particles;

* "If such heterogeneity [that of matter] were only pronounced enough, it appears that the law of gravitation would be capable of accounting for at least the greater number of effects at present attributed to the so-called molecular forces and the force of chemical affinity." *Stewart* and *Tait*, loc. cit., 95.

and that these portions must be arranged in filaments whose direction at each point is the axis of rotation, these filaments being endless, or terminating in the free surface of the fluid. Accepting these results, Thomson conceived the hypothesis that what we call matter may consist of the rotating portions of a perfect fluid which continuously fills space. If any portions of such a fluid have this vortex-motion communicated to them, they will retain it forever, thus constituting what Thomson calls a vortex-atom. Moreover, since the only necessary condition of the existence of a vortex-atom is that the filament should rotate on its axis and should be endless, it is plain that there may be various kinds of such atoms, according to the shapes of the vortices.[52] The simplest form is a plain ring, like a smoke-ring. But the filament may undergo any number of knottings and twistings, and yet comply with the definition. As nothing short of creative power could annihilate such an atom, so nothing short of this could launch it into existence. Moreover, such an atom would have actually the properties of the metaphysical atom; it cannot be cut, from its very nature. This marvellous hypothesis is as yet, of course, only a matter of speculation. Much more work must be done upon it before it can even be accepted as probable. But it is not too much to say of it now that it explains most satisfactorily many vexed problems in molecular and atomic science.

It only remains for me now, after this lengthy discussion, to ask what are the bearings of the facts and of the generalizations from them, which have now been stated, upon chemical science? What is the conception which we should now hold of the atom, and of the various changes in which it may perform a part? In the first place, it seems to be clear that in the opinion of modern science, the atom has a real existence; that there is a fixed portion of matter, definite for each element, which is the smallest quantity of it capable of taking part in a chemical change. If this be true, then the restiveness shown in certain directions in using the word atom, and the attempts made to substitute some other idea for it, are entirely needless.

In the second place, it is evident that our equations representing chemical changes need to be made more significant, by including in them the changes of energy involved.[53] The equations of Stahl, of phlogistic memory, did this. Thus the (metallic calx+phlogiston)=metallic calx+heat. This Lavoisier translated thus:

metal + (oxygen + heat) = metallic oxide + free heat. To-day we should say: (metal + m units of energy) + (oxygen + n units of energy) = (metallic oxide + p units of energy) + (exterior energy represented by $m + n - p$ units). The power of prediction in chemistry, the power which characterizes an exact science, appears to lie in a clear comprehension of the transformations of energy which take place during chemical action.

Again, the real existence of atoms occupying space, forbids the assumption too often based upon our present system of graphic formulas — but unfairly as it seems to me — that the atoms in a molecule all lie in one plane; and thus gives rise to a chemistry of three dimensions. This idea was distinctly expressed by Kekulé in 1866, in his Lehrbuch[54] where he gives perspective views of the constitution of the benzene atom. And in Clarke's paper, already referred to, the conditions under which solid molecules are possible, are satisfactorily discussed. I by no means wish to imply that graphic formulas are useless. I would have the idea clearly held that they only express the mode in which the atoms are combined with each other, considered statically, and not the manner of their arrangement in space.

Lastly, the fact is clearly before us that the atoms are not at rest within the molecule, and therefore that they cannot occupy positions fixed in space. We have seen that it is the belief of eminent authority that they even exchange molecules in their rapid motion; moreover, this conception of atomic motion is strengthened by the fact observed by Hofmann,[55] that one isomeric form of an aromatic monamine may be converted into another by what he calls a wandering of the atoms within the molecule. Indeed so obvious is this atomic motion, and so necessary is it to take account of it in chemical theory, that Kekulé has founded upon it his explanation of equivalence.[56] According to him, the equivalence of any atom is the relative member of impacts which it receives from other atoms in a unit of time. The view of Michaelis is analogous to this.[57] He supposes that during the vibration of the atom, it attains certain positions at each of which it is capable of exercising chemical action upon other atoms. The number of such positions constitutes the equivalence of the atom. If we suppose the total force unequally distributed, the atom will exert different amounts in its different positions; thus accounting for variable equivalence. And if the total force has no fixed ratio to the number of positions, the fact is accounted for that there is no necessary relation between chemism and equivalence.

The task which I have allotted to myself is now accomplished. I have attempted to set before you, as clearly as I was able, the conception which the science of to-day holds concerning the molecule and the atom. If I have contributed, in doing this, to make more precise the ideas which are commonly held upon this subject, I shall feel that an essential service has been rendered to a science which more than any other, calls to its aid creative energy, and produces continually new forms of matter to astonish and delight mankind.

APPENDIX.

NOTES AND REFERENCES.

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[4] *Boltzmann*, Ber. Ak. Wien, II, lxiii, 397–418.

[5] New Experiments, physico-mechanical, etc. Second edition. Oxford, 1662. See also Tait's Thermodynamics, Preface, p. iv, 1868.

[6] Quoted by *Gay Lussac*, Ann. Chim. Phys., xliii, 157.

[7] *Thomson*, Nature, i, 551, 1870; *Maxwell*, Phil. Mag., IV, xlvi, 453, 1873.

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[13] Nature, i, 552, 1870.

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[50] Trans. Roy. Soc. Ed., xxv, 217, 1868.

[51] Ueber Integrale der hydrodynamischen Gleichungen welche dem Wirbelbewegungen entsprechen, Crelle Ann., 1858; Phil. Mag., 1867, 1.

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[54] Lehrbuch der Organischen Chemie, II, 515, 1866.

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CONTRIBUTIONS TO THE CHEMISTRY OF HYDROGEN. By ALBERT R. LEEDS, of Hoboken, N. J.

1. *On the Preparation of Phosphoretted Hydrogen:*—While engaged upon the preparation of Phosphoretted Hydrogen, by the ordinary reaction of a solution of caustic potassa upon phosphorus, the difficulty experienced in first filling a plain retort with the solution, and the inconvenience and possible danger resulting, on interrupting the operation, by the reflux of liquid into the retort, led to the following modification of the process. This consisted in the substitution of a tabulated retort, connecting with a hydrogen generator, by an india-rubber tube which was opened and closed by means of a nip-cock. By first filling the retort with hydrogen, before beginning the operation, and opening the connection with the generator on interrupting it, any difficulty was obviated. Illuminating gas may be used still more conveniently. This device, I afterwards found had been employed by others.

2. *Reaction of Ammonic Hydrate on Phosphorus:* — When concentrated ammonia is substituted for solution of caustic potassa, the excess of ammonia gas driven over, is absorbed by the water of the wash-bottle or pneumatic trough, and the residual bubbles of non-condensible gas are quite small. They consist of a mixture of hydrogen and phosphoretted hydrogen, the latter being in sufficient quantity to render them spontaneously inflammable. The phosphorous remaining in the retort consists of the white and red modification, and the same remark applies to that which distils over in the current of steam and escaping gas.

3. *Circumstances affecting the Spontaneous Inflammability of Phosphoretted Hydrogen:*—On adding fatty matters to the contents of the pneumatic trough, even a thin film upon the surface, the spontaneous inflammability of the phosphoretted hydrogen is destroyed. On addition of excess of caustic potassa it is restored, but not by solution of ammonia.

4. *Formation of Phosphoretted Hydrogen by Nascent Hydrogen:* —When zinc, in filings or thin sheets, is heated with a solution of caustic potassa, the latter is decomposed with the formation of a Zincate of Potassa which remains in solution, and the liberation of hydrogen. If phosphorus be added to this solution, a mixture of hydrogen and phosphoretted hydrogen is evolved, the latter

being attributable in part to the combination of the phosphorus with the nascent hydrogen.

5. *Color of the Phosphoretted Hydrogen flame in Air and pure Oxgyen.*—When the bubbles of phosphoretted hydrogen prepared in the ordinary manner, are allowed to escape into an inverted receiver filled with air, they soon cease to ignite spontaneously, and long before the oxygen in the air has been exhausted. But after the ignition has ceased to be visible in diffused daylight, it is visible in a darkened room, and is attended with the production of a *greenish* light. But when the air in the receiver is replaced by an atmosphere of pure oxygen, the ignition is so intense that the flashes become painful from their brilliancy. They are also much less diffused than when ignition takes place in air, and were it not that the ignition takes place with almost explosive violence, the experiment would be a very attractive one. The color of the flame in this case is perfectly white, in the former greenish; a difference due to the presence of nitrogen.

6. *Properties of Hydrogen evolved from Alkaline Solution:*— In order that no traces of fatty matters might be present, sheet zinc was repeatedly digested with solution of caustic potassa and afterwards washed with distilled water. All india-rubber connections were done away with, to exclude possible contamination of the gas by sulphur compounds, and the zinc prepared as above was heated with solution of caustic potassa. When the hydrogen thus prepared was collected over water, and ignited in a darkened room, it burnt with a blue flame where the flame was in contact with air, and with a greenish flame beneath the blue. On repeating the experiment, the flame appeared altogether blue. This blue color was of a much deeper tint than is noticeable in the flame of hydrogen, when care has been taken to exclude contamination by sodium salts. It is probable that some of the hydrogen may unite with zinc to form a small portion of Zinciuretted Hydrogen, but as yet this has not been demonstrated.

The literature of Zinciuretted Hydrogen in fact, is eminently unsatisfactory, and even the existence of this gas has not been satisfactorily established. According to Gmelin, the manner in which it was first prepared by Vanquelin was, by heating four parts of roasted Zinc Sulphide with one part of powdered coal, a method assuredly not calculated to give any one gaseous product in a pure condition. The gas so produced, according to the same authority, is lighter than air and heavier than hydrogen, with an unpleasant smell. It is ignited by a burning body in contact with air, with a bluish and yellow-white flame, forming a white cloud of Zinc Oxide and depositing also some metallic zinc. When mixed with chlorine, it explodes by contact with flame, forming Hydrochloric Acid and Zinc Chloride. It is not decomposed by Hydrochloric Acid, and is not absorbed by water. According to Ruhland, a constant stream of Zinciuretted Hydrogen may be obtained by electrolysis with a pole of Zinc Amalgam (Schw. 15, 148) while the results obtained by Cameron in experiments in which he caused dilute acids to react upon zinc, and Hydrosulphuric Acid upon Zinc Amalgam, led him to conclude that Zinciuretted Hydrogen does not exist (Jahresbericht, 1860, 181).

With regard to what has been done hitherto, it should be remarked that the existence of a Hydride of Zinc, is antecedently highly probable from considerations founded on the chemistry of the other metals. And, moreover, the researches above detailed, are not sufficiently exhaustive to establish a final conclusion in the negative.

As the amount of gas evolved in this manner is small, the well-known device of adding a more electro-negative metal like Iron or Platinum to the contents of the retort was made use of, but the gain though decided was not sufficient to secure a plentiful supply. The dissolved zinc all went into solution as Zincate of Potash, no insoluble residue remaining in the retort. This was the case, whether concentrated or dilute solution of caustic potassa was employed, and even when the solution was evaporated to dryness, and afterwards taken up with water. It is probable that if the solution of zinc in potash had been boiled for some time, and more especially if the solution had been considerably diluted with water, that the zinc oxide would have been precipitated. In the first experiments, where sheet-zinc alone was dissolved in caustic potassa, a large amount of insoluble white powder was formed. This precipitate was like sand, and after boiling with distilled water three times, decanting, washing, and drying, it was found to contain not a trace of potassa. To determine whether it was composed entirely of anhydrous zinc oxide, an analysis was made on the precipitate very carefully dried, with the result of showing that it was not homogeneous, but was a mixture of Anhydrous Oxide with Zinc Hydrate in the proportion of 93·94 parts of the former and 6·06 parts of the latter. Other insoluble precipitates obtained in like manner differed somewhat in their properties, filtering with great difficulty and settling slowly, a difference probably due to their having been boiled for a shorter time and containing a larger proportion of Zinc Hydrate.

An attempt was made to examine the spectroscopic reactions of the gas obtained from zinc and caustic potassa, by collecting it in a glass vessel provided with a platinum exit-tube and jet. No india-rubber connections were used. After standing for twelve hours over water, it was allowed to flow in a slow stream through a narrow slit and ignited. The flame was almost invisible, a very faint bluish tinge only being apparent, and it is hardly necessary to say that by this method of examination, no spectroscopic reaction could be obtained. From the fact that the coloration of the gas, when burnt immediately after collection, was so much more intensely blue than that of the gas which had been standing over water and purified by repeated washing, it appears that the constituent, which contributed the blue color to the flame, is largely soluble in water.

It is noteworthy in this connection, that while the older chemical books attribute to the hydrogen flame a yellowish coloration, those published since flame-reactions have been so extensively studied, describe it as faintly luminous and bluish. In reference to this point the following experiments were made. First, hydrogen was prepared by the reaction of pure sulphuric acid upon zinc, and purified by washing successively with distilled water, solution of argentic nitrate, concentrated sulphuric acid, dilute nitric acid, potassic permanganate, solution of chemically pure caustic potassa, fused caustic potassa purified from alcoholic solution, and amianthus saturated with nitrate of silver. It was collected over mercury and ignited at the terminal slit of a platinum tube. The flame was slightly blue and faintly luminous.

Secondly, hydrogen prepared in a eudiometer chemically clean, by the electrolysis of distilled water acidulated with pure sulphuric acid, was burnt at the terminal slit of a chemically clean platinum tube, with the same results.

There are several sources of error in these experiments, however, which should be obviated before a satisfactory conclusion can be arrived at. In the first place, it has not been demonstrated that even after taking these precautions, the hydrogen is absolutely pure. Moreover when the combustion takes place in air the flame is in contact with the innumerable particles of solid matter and motes diffused throughout the atmosphere, and finally, the combustion takes place under an atmospheric pressure. The latter circumstance would tend to exalt the luminosity. It will be necessary in order to settle these points, to burn a mixed jet of hydrogen and oxygen both chemically pure, expanding in equivalent proportions, under a minimum of pressure, into a vacuum.

7. *Upon the Purification of Hydrogen:*—In an examination into the purity of the hydrogen as ordinarily prepared for laboratory use, some facts were determined which have an important bearing upon this, and connected subjects. The zinc employed was the spelter manufactured by the Passaic Zinc Works at Jersey City. Its composition, according to a communication made me by Dr. Gideon E. Moore, the chemist of the Passaic Zinc Co., is

Zinc	99·953 per cent.
Lead	0·027 " "
Iron	0·020 " "

The spelter was cast into bars of three-quarters of an inch square section, and these cut by a chisel into blocks of the same length. This crystalline spelter is so tough, that it is otherwise very difficult to obtain blocks of a convenient size to introduce into a Kipps Generator, and if granulated zinc is employed, a less regular and economical evolution is obtained. Dilute sulphuric acid, which had been carefully tested and found to be free from impurities, was used. The gas was washed and then passed through a glass tube of four m-m bore, and ten decimeters long, which was bent to and fro upon itself, and heated in such a manner as to expose fifteen centimeters square of red-hot interior surface. After a slow passage of the gas for twelve hours, not the faintest blackening was visible, on the inside of the tube, beyond the heated portion.

Moreover, separate portions of this zinc had been repeatedly used in the ordinary laboratory practice for four years, in testing for arsenic, not of course in cases of suspected poisoning, and no trace had been found. Moreover, the New Jersey Calamine, Willemite, and their accompanying minerals, from which this spelter is smelted, do not, so far as I am aware, contain arsenic, so that except accidentally, this American zinc should not be contaminated with it. Finally, a U-tube filled with pumice stone saturated with argentic nitrate, and which had been employed for two years, in the purification of all the hydrogen employed in the laboratory for reductions, and for similar purposes, was tested for arsenic with a negative result. Hydrogen therefore, made from Passaic zinc, is not contaminated by arseniuretted hydrogen.

In the same piece of apparatus just described, the wash-water gave an abundant reaction for zinc, the Oil of Vitriol also, after dilution, to precipitate out a small percentage of Plumbic Sulphate, and removal of a trace of Iron, gave on the addition of Ammonic Chloride, and colorless Ammonic Sulphide, an abundant white cloud due to the formation of Sulphide of Zinc.

These results had been anticipated from the results which had been obtained by the reaction of zinc in alkaline solution, and the probability of the formation of a Hydride of Zinc, when hydrogen in the nascent condition, was liberated in dilute acid solutions. The following experiments were instituted, in order to detect with still greater precision, the presence of a trace of Zinciuretted Hydrogen. The contents of the wash bottles and U-tube were replaced by fresh portions, and a Geissler Absorption apparatus attached. Some Hydrochloric Acid was diluted with ten parts of water, and divided into two equal portions. One was employed to fill the Geissler apparatus and the other poured into a tall narrow beaker. Hydrogen was allowed to flow through the apparatus, bubble by bubble, for fifteen hours. The contents of the Geissler apparatus were then emptied into a beaker similar to the first. Both liquids were rendered alkaline by ammonia, and when they were at rest, freshly prepared colorless Ammonic Sulphide was poured down the sides of the beakers, and allowed to flow over their surfaces. A white cloud was formed along the boundary in the beaker containing the contents of the Geissler apparatus, while the other remained perfectly limpid. A mixture of Nitric Acid one part, and water five parts, was divided into two portions and treated in like manner, with identical results. There was the possibility of the presence of silicic acid and alumina in the reagents employed, due to the solution of these bodies from the walls of the glass vessels, but in this case the reaction attributed to the presence of zinc would have been equally manifest in the companion experiments. This was not the case. The Zinciuretted Hydrogen is decomposed by the acids with the formation of Zinc Chloride and Nitrate, the presence of which is revealed by the formation of white Sulphide of Zinc.

8. *Upon the Occlusion of Hydrogen by Zinc:*—A decomposing cell was formed by plunging a strip of sheet zinc six inches in breadth and coiled in a spiral, into a pint jar. A strip of platinum foil of similar dimensions was introduced as a cathode, and the jar filled with a saturated solution of caustic potassa. The terminals of a battery of six cups were passed through a cork closing the jar, and also an exit tube for collecting the escaping gas. An abundant bluish gray metallic mass was speedily thrown down upon the platinum. It was beautifully crystallized in leaf-like masses, presenting a delicate fern-like appearance. It did not dissolve perfectly in hydrochloric acid, a white precipitate of plumbic chloride being left behind.

To avoid this and other possible impurities, a second strip of platinum was substituted for the zinc anode, and the decomposing cell was filled with a solution of caustic potassa in which pure zinc oxide had been dissolved to the point of saturation. The precipitated zinc was washed rapidly with cold water until all alkaline reaction had disappeared, then with absolute alcohol and ether, and dried as completely as possible by a Bunsen filter-pump. It was heated for seven hours at 120° c, at the expiration of which period, it was assumed that every trace of moisture had been dissipated. This was not the case, for on heating a portion of the material in a matrass to low redness, very faint traces of moisture were condensed in the cooler part of the tube.

To determine whether the precipitated metal contained hydrogen, the following method of analysis was resorted to. If a combustion is effected in dry oxygen, its contents of water should be given off and also its hydrogen, in the form of water. If another portion is ignited in a current of dry hydrogen, its contents of water should be given off, and also its hydrogen, but *not* in the form of water. The difference, therefore, between these two results should give by calculation the percentage of hydrogen present. The first analysis was as follows:—0·8620 grms. submitted to combustion in oxygen yielded 0·0252 grm. water, or 2·92 per cent.

1·0257 grms. ignited in hydrogen, yielded 0·0252 grm. water or 2·45 per cent. The difference is 0·47 per cent. of water, which is equivalent to 0·052 per cent. Hydrogen. The Hydrogen was replaced in the tubes, by careful aspiration with dry air, until the absorption apparatus had ceased to change weight. The temperature of ignition was at first low, but at the end of the operation was brought to a red heat. No traces of moisture were visible on the sides of the absorption tube.

Second analysis:—The material had been prepared in a similar manner as the first, but dried at 120°C for sixteen hours. 1·0652 grms. submitted to combustion in oxygen yielded 0·0335 grm. water, or 3·15 per cent.

1·4586 grms. ignited in hydrogen, yielded 0·0320 grm. water, or 2·19 per cent. The difference is 0·96 per cent. of water, which is equivalent to 0·11 per cent. of Hydrogen. It is evident on inspection of these analyses that they are not altogether conclusive. It was anticipated that no water would be given off on ignition of the material, if absolutely dry, when ignited in hydrogen. Yet one analysis gave 2·45 per cent., and the other 2·19 per cent. In defense of them it may be urged that the material was dried for an unusual number of hours at a temperature considerably above the boiling point of water, and that every care was taken to dry the oxygen, hydrogen, materials and tubes employed. In fact, the gases had been passed through the combustion and ignition tubes, just prior to each analysis, which was undertaken only when it was found that the absorption bulbs employed did not change weight.

The fact remains however, that according to the first analysis ·052, and according to the second 0·11 per cent. of Hydrogen, were occluded or combined with the zinc. This result will have to be verified by repeated trials before it can be fully received. It was arrived at in the month of June, just prior to the summer vacation, and further experiments were necessarily postponed until resumption of college work in the fall.

Upon the Reduction of Silver at ordinary temperatures in the presence of Free Nitric Acid. By Albert R. Leeds, of Hoboken, N. J.

In a prior communication entitled "Contributions to the Chemistry of Hydrogen," I have alluded to some unexpected results, obtained in an inquiry into the purity of Hydrogen, generated by the action of dilute chemically pure sulphuric acid, upon the spelter manufactured by the Passaic Zinc Co. It was shown that the evolved gas contained a trace of Zinciuretted Hydrogen, which could be detected in the water, oil of vitriol, dilute hydrochloric, and nitric acids employed in washing it. An examination of the Argentic Nitrate U-tube, made use of as a purifier, showed no trace of Arsenide of Silver, but the fact of some change was evident from the film upon the sides of the tube, the pumice in places, and a deposit in the excess of liquid collected at the bottom of the tube. This film and deposit proved to be metallic silver. Moreover, on the pumice at the surface of the Argentic Nitrate nearest the generator, some crystals had formed which were $1\frac{1}{2}^{mm}$ in height and 1^{mm} in breadth. They were of a delicate lemon-yellow color, with acute octahedral terminations and rhombic base—apparently rhombic octahedra. They were insoluble in 1,000 parts of water, but dissolved in dilute nitric acid on heating. The solution yielded no trace of chlorine, but only silver. By hydrogen at red heat, they were reduced to metallic silver, with the form of the original crystals. They were crystallized Argentic Nitrite.

Here we were presented with two unexpected cases of Reduction—1st, of silver from a solution of the nitrate in the form of metal; 2nd, of the nitrate itself—to the nitrite—and both at ordinary temperatures. At that time I did not know that the fact of the reduction of silver, from its neutral solution in nitric acid, had been noted, but was looking for the effects which would presumably take place if the hydrogen was at all zinciuretted. And while considering whether it was necessary, in case of zinc like this, which yielded so small an amount of carbonaceous matter on solution, to examine into the possible contamination of the gas by Hydrocarbons, I found a note on The Purification of Hydrogen Gas, by Ch. Violette (Compt. Rend. lxxvii, 940, 942), decisive of the question. According to this author, it is generally stated that the hydrogen obtained by the action of sulphuric acid upon commercial zinc contains carbon hydrides, and that the latter are not removed by the ordinary process of purification. To test the accuracy of this statement, M. Violette conducted the hydrogen purified by passing through plumbic nitrate, argentic sulphate (?), caustic potassa, and oil of vitriol, over red hot cupric oxide. The water formed, amounting at least to thirty-five grms., was collected in a U-tube, and the gaseous products passed through baryta water. The former gave no acid reaction, and the latter no opacity from the presence of carbonic anhydride. It was concluded, therefore, that hydrogen prepared in the ordinary manner, after proper purification, was free from any hydrides of carbon.

In all questions concerning the luminosity of the flame, the contamination of the hydrogen by hydrocarbons is of the highest importance, and the fact that the flame of pure hydrogen was found to be almost colorless and non-luminous, while that which was supposed to contain zinciuretted hydrogen was strongly blue, is an argument for the existence of the latter hydrogen compound. But so far as the fact of the reduction of a metal in solution by hydrogen at ordinary temperatures is concerned, the presence of zinciuretted hydrogen is of far greater importance than that of carburetted hydrogen. When the greater fact, that hydrogen alone is capable of effecting such a reduction, is settled, it will be easy enough to determine whether a similar reduction is effected by gaseous hydrides of carbon.

In the first train of purifiers which was fitted up to test this question, water, argentic nitrate, oil of vitriol, dilute nitric acid, permanganate of potassa in solution of potassa, caustic potassa from alcohol, and argentic nitrate were employed. The arrangement was partly due to convenience, some portions of the apparatus being already in readiness. A remarkable reduction ensued in the case of the potassic permanganate. A saturated solution of caustic potassa (from alcohol) was saturated with potassic permanganate and allowed to stand for two days, at the expiration of which period no change of its purple color had occurred. But when the gas was passed through it, it became dark green from the formation of manganate of potash. This could not have resulted from any of the dilute nitric acid having been carried over into the permanganate, because dilute nitric acid does not decompose the permanganate even when neutral, much less when in strongly alkaline solution. It is reduced, however, by nitrous acid, becoming successively dark purple, bluish-purple, bluish-green, and finally dark green from the production of potassic manganate, while at the same time a reddish-brown precipitate is formed, apparently of hydrated sesquioxide of manganese. The further study of this reduction was postponed, as well as the consideration of the reducing action of zinciuretted hydrogen, on account of encountering, at this point of the investigation, such remarkable quantitative results connected with the reduction of silver from its solutions by means of hydrogen, as to make them relatively of much less interest. After making some progress in these determinations I encountered a very valuable research by Dr. J. W. Russell (Ch. Soc. Jour. [2] 12, 3), upon the same subject. As the method of experimentation, however, was somewhat different, and the results are not quite parallel, I shall quote as much of my own work as appears to have an independent value. And the more so, because for very unsatisfactory reasons and upon most inconclusive experimental data, M. Pellet, in the Compt. Rend. (78, 1132), has denied the accuracy of Dr. Russell's labors.

The gas evolved from the generator was passed through an acid solution of ferrous sulphate, sulphuric acid, over potassic hydrate (from alcohol), and over twenty feet of pumice and amianthus contained in suitable vessels and saturated with nitrate of silver. If every trace of zinciuretted hydrogen were not absorbed after passage through such a train, it would be altogether inadequate to explain the surprising results obtained. A quantity of crystallized Nitrate of Silver was prepared, and was found to manifest a distinctly acid reaction toward Alizarin, although it was inert toward Litmus. It was brought to incipient fusion, and after cooling still manifested a very faintly acid reaction toward alizarin. Three U-tubes, with the curve of the U blown into a cylindrical bulb 1½ c. m. in diam. and 20 c. m. in length, were connected with the termination of the purifying apparatus and with one another. They were filled with 20 c. c. of a solution containing 0·32304 grms. argentic nitrate per c. c., or 6·4608 grms., equivalent to 4·104 grms. silver. They were tilted up at such an angle, that the gas passed through nearly the same depth of liquid, and was in contact with about the same amount of surface in each U-tube.

(a) Exposed to current for 5½ hrs., (b) for 22 hrs., (c) for 21 hrs. (1) Precipitate remaining in U-tube and dissolved in nitric acid. (2) Precipitate collected on filter and dissolved in nitric acid. Temp. during experiment 24° to 30° C.

a^1 = 0·0522 grm. Silver.	b^1 = 0·2973 grm. Silver.	c^1 = 0·2167 grm. Silver.
a^2 = 0·0232 " "	b^2 = 0·0477 " "	c^2 = 0·0639 " "
a = 0.0754 " "	b = 0·3450 " "	c = 0.2806 " "

It will be noticed, in the first place, that the amount of silver floating about in the liquid, or so loosely attached that it could be removed by washing and be collected on the filter, was in all cases very small, while that which attached itself as a coherent precipitate was large, amounting in (b) to 0·3 grm. In the second, that the amounts of silver precipitated are nearly although not exactly proportional to the times. Thus the total amount of silver precipitated in the three U-tubes being 0.701 grm. silver, the amounts proportionate to 5½, 22, and 21 hours would be 0·079, 0·318, and 0·303 grms.; the amounts actually precipitated were 0·0754, 0·345, and 0·2806 grms. It will be noted that the amounts corresponding to a uniform rate of precipitation are less than those actually found for the first two intervals, and greater for the third.

Two similar U-tubes, d and e, were arranged as above, each containing in 20 c. c., 6·4608 grms. Argentic Nitrate, and the gas passed for 2¾ hours.

d^1 = ·048 grm. Silver.	e^1 = ·039 grm. Silver.
d^2 = ·011 " "	e^2 = ·025 " "
d = ·059 " "	e = ·064 " "

The amount of silver precipitated in similar solutions, exposed to the same current of gas, for the same length of time, is not precisely identical. This is probably due to the fact, that the tubes were not arranged with so much care, that the depths and surfaces of the liquids were precisely the same. But it is strikingly evident that the hydrogen after effecting a large amount of reduction in one tube, does not have its reducing power in the slightest degree diminished. It reduces as much of the second solution as the first, as much of the third as the second, and so on. This would not be the case, were the fact of the reduction dependent upon the presence of some impurity like zinciuretted hydrogen.

What connection exists between the amount of silver precipitated, and the amount of silver nitrate in solution? This has already been answered by Dr. Rus e l, who found that it was directly proportional. He dissolved 30, 10, and 2 grms. of argentic nitrate, each in 120 c. c. of water, and passed hydrogen through the solutions for a fortnight. There was then found to be a precipitate which weighed 0·365 (1·22 p. c.) in the 30 grm. solution, one weighing 0·1229 (1.23 p. c.) in the 10 grm. solution, and an amount too small to weigh in the 2 grm. solution.

To determine what would be the limit of the precipitation, and whether reduction would take place at the same rate as we approached that limit, the following determinations were made. The filtrates from d and e were exposed to a slow current of hydrogen for 36 hours at a temperature varying from 24° to 28° C., and collected as before. They were termed f and g.

f^1 = 0·3593	grm.	Silver.	g^1 = 0·3495	grm.	Silver.
f^2 = 0·0955	"	"	g^2 = 0·0699	"	"
f = 0·4548	"	"	g = 0·4194	"	"

The filtrate from f was further exposed to the current for 18 hours, and the precipitate was labelled *h*. It contained

h^1 = 0·100	grm.	Silver.
h^2 = 0·014	"	"
h = 0·114	"	"

That from g was exposed in the same manner for 36 hours, forming precipitate entitled k.

k^1 = 0·113	grm.	Silver.
k^2 = 0·041	"	"
k = 0·154	"	"

The series of determinations gives the following rates of precipitation: —

In the first	2¾ hrs.,	0·062 grm. of Silver precipitated,	or 0·0225	grm. per hour.
" " following	36 "	0·4372 " " " "	" 0·0122	" " "
" " "	18 "	0·114 " " " "	" 0·0063	" " "
" " "	18 "	0·040 " " " "	" 0·0022	" " "

It is evident that herein we have the basis of a mathematical discussion of the relative intensities of the force concerned in the precipitation of the silver, and of the forces concerned in arrest-ing the same. By laying off on the line of abscissas the times, and on the line of ordinates the amounts of precipitate, we should obtain a curve, whose properties should indicate the relations sought for. By constructing this curve for different degrees of concentration of the solutions, for different temperatures, pressures, and volumes of the gas, we should be able to make an exhaustive analysis of those relations, and so mathematically connect the phenomena observed, as to use this as a method in the higher analysis of chemical science.

No attempt has been made as yet to use the above data in this manner. The experiments were preliminary, and the temperatures, pressures and volumes of gas during each hour, varied in the course of every determination. It will be necessary, so to arrange the apparatus as to eliminate these accidental variations, and then to multiply the number of determinations, especially toward the beginning of the experiments, as to supply a sufficient number of points to locate the curves with the needful accuracy. There is no practical difficulty in doing this, after the precipitation has gone on for an interval sufficient to obtain an amount adequate to make an accurate estimation possible, and by repeating the series, and interpolating the determinations for different intervals, as many points in the curves can be verified as is desirable. The important results obtained by Dr. Russell in this connection will illustrate the action which takes place. He says, "When a saturated solution is used, hydrogen must be bubbled through it for nearly half an hour before precipitation takes place; if the current be continued, a dull grayish substance separates out at first, afterwards the precipitate is perfectly crystalline and bright, exhibiting a beautiful appearance. If, for instance, 10 grms. of argentic nitrate were taken, dissolved in about 8 c. c. of water, and hydrogen bubbled through the liquid for eighteen hrs., only about 0·1308 grm. of silver, would be precipitated. At first the amount of silver precipitated is nearly proportional to the time to which it has been exposed to the action of the hydrogen, but after the first forty-eight hours the amount of silver precipitated increases at a slower and slower rate, and after a time the amount of silver present will often begin to diminish." In this experiment, in which 1·25 grms. of argentic nitrate per cubic centimeter were used, the precipitate per hour amounted to 0·0073 grm. silver. This is less than in determinations d, e, f, and g, in which a solution containing ·32304 grm. per c. c. was used, and when the average rate for the first two and three-fourths hours was 0·0225 grm., and for the succeeding thirty-six hours, 0·0122 grm. silver. This shows that in order to make the experiments comparable an identity of the conditions under which they are performed, must first be arrived at.

But with regard to the appearances manifested at the beginning of the reaction, those exhibited in the manner of experimentation above described, differ strikingly from the phenomena observed by Dr. Russell. As soon as the hydrogen had bubbled through the solution, for a sufficient length of time to partly change the atmosphere above it, an interval of a few minutes only, an extremely delicate coloration ensued which deepened to a pronounced bluish-purple tint. This presently became turbid and rapidly changed to gray, after which precipitation occurred. The appearance of the silver is very beautiful, especially after the operation has continued for some time. So delicate is this coloration, that it is worthy of further examination, to determine whether a neutral solution of argentic nitric might not, under certain circumstances, be employed as a *Test for the presence of Hydrogen.*

The precipitation of silver by hydrogen was found to be independent of the action of light. And Dr. Russell, who quantitatively determined the results in the two cases, found that when "two similar solutions, both containing 20 grms. of argentic nitrate were acted on by hydrogen, the one in bright light, the other in the dark, all other circumstances being made as similar as possible; from the solution in the light, 0·2680 grm. of silver was precipitated, and from the one in the dark, 0·2703 grm.

I cannot further occupy your time with other topics connected with this research, as

The effect of heat and cold upon the rate of precipitation.

The effect of pressure upon the rate of precipitation.

The effect of prolonged exposure to the gas in sealed vessels, the bye-products and the end-reactions. Nor with the action of hydrogen upon solution of silver-nitrate containing an excess of oxide of silver, or upon ammoniacal nitrate of silver, nor upon other salts of silver and other metals. These I hope to illustrate in a future communication.

On the Relations of Structure, Density and Chemical Composition in Steel. By John W. Langley, of Ann Arbor, Michigan.

At the present day much attention is being given to the importance of studying the connection between the Chemical composition and physical properties of matter, meaning by physical properties such characteristics as crystalline form, color, hardness, specific gravity, etc. The following paper is offered as a slight contribution to this department of knowledge.

There are two methods of investigating this subject; first, to take bodies whose chemical nature is intimately known, and to commence an examination of their physical character. The second, to take a body long known and studied from the mechanical side, and to investigate its chemical composition. The latter is the method here followed.

Steel, from its great industrial importance, is the best known of all alloys, and its behavior under mechanical forces has been most extensively studied both by individuals and governments; but unfortunately, these elaborate tables of tensile strength, elasticity, etc., have not been supplemented by correspondingly thorough chemical analyses, because it has only very recently been surmised that slight variations in the composition of steel affect its behavior more radically than do all the processes of the rolling-mill or the machine shop. Within the last eighteen months the United States have appointed a commission to study in detail the connection between the strength, elasticity, etc., of steel and iron, and their chemical composition as shown by analysis. The research, which is the basis of this paper, was commenced before the organization of the U. S. Commission, but after the general government decided to take up this subject and explore it for the benefit of engineers, all that part of the original design was, of course, abandoned, and our attention has been chiefly directed to a study of the chemical and molecular structure of steel; a field which it is probable will not be entered upon by the Government Commission.

In such an undertaking the number of facts to be acquired, and of subjects to be pursued in detail is very great. They are thus far in a very incomplete state, and I am, therefore, able to furnish a record of a portion only of the work done, that which merely serves as a foundation for future research.

In March, 1874, Messrs. Miller, Metcalf and Parkin, steel manufacturers of Pittsburgh, selected eight samples of steel which were believed to form a set of graded specimens, the order being based on the quantity of carbon which they were supposed to contain. They were numbered from one to eight. On ana ysis the quantity of carbon was found to follow the order of the numbers, while the other elements present, silicon, phosphorus and sulphur, did not do so. As the method by which these samples were selected has an important bearing on the subject in hand it will not be out of place to describe it.

The steel is melted in black-lead crucibles capable of holding about eighty pounds. When thoroughly fluid it is poured into cast iron moulds, and when cold the top of the ingot is broken off exposing a freshly fractured surface whose plane is approximately at right angles to the axis of the ingot. The appearance now presented is that of confused groups of crystals, all appearing to have started from the outside and to have met in the centre, and this general form is common to all ingots of whatever composition; but to the trained eye, and only to one long and critically exercised, a minute, but indescribable difference, is perceived between varying samples of steel, and this difference is now known to be owing almost wholly to variations in the amount of combined carbon, as the following table will show. This consists of twelve samples selected in April, 1875, by the eye alone, and the analyses were made from drillings taken direct from the ingot before it had been heated or hammered.

TABLE I.

INGOT NO'S.	IRON BY DIFFERENCE.	CARBON.	DIFF. OF CARBON.	SILICON.	PHOSPHORUS.	SULPHUR.[1]
1	99·614	·302		·019	·047	·018
2	99·455	·490	·188	·034	·005	·016
3	99·363	·529	·039	·043	·047	·018
4	99·270	·649	·120	·039	·030	·012
5	99·119	·801	·152	·029	·035	·016
6	99·086	·841	·040	·039	·024	·010
7	99·044	·867	·026	·057	·014	·018
8	99·040	·871	·004	·053	·024	·012
9	98·900	·955	·084	·059	·070	·016
10	98·861	1·005	·050	·088	·034	·012
11	98·752	1·058	·053	·120	·064	·006
12	98·834	1·079	·021	·039	·044	·004
MEAN			·071			

Here the carbon is seen to increase in quantity in the order of the numbers, while the other elements, with the exception of total iron, bear no relation to the numbers, on the samples.

It has been long known that the structure of cast steel, as visible to the eye, bore some relation to the quantity of carbon present, and a rough classification by this method has been in practical use, but the above analyses show a very close connection between composition and structure, for differences of carbon so slight as seven-hundredths of one per cent. will impress such a change in the crystalline appearance of the metal that the eye of the expert can detect it, rarely ever making a mistake where the total carbon rises to a half per cent. or upwards; in very mild steels the discrimination is less perfect.

The appearance of the fracture by which the above twelve selections were made can only be seen in the cold ingot before any operation, except the original one of casting, has been performed upon it. As soon as it is heated or hammered the structure changes in a most remarkable manner so that all trace of the primitive condition *appears* to be lost. This is very well shown by this specimen which exhibits on one side large coarse crystals, and on

[1] The determinations of sulphur were made by Prof. A. R. Leeds, of Hoboken, N. J.

the other a fine grained non-crystalline fracture, the one having been changed into the other by a brief elevation of the ingot to a red heat. But although the crystalline form thus seems to be lost by heating or rolling, it can again be rendered evident by a special mode of treatment.

Another method of rendering visible to the eye the molecular and chemical changes which go on in steel is by the process of hardening or tempering.

When the metal is heated and plunged into water it acquires, as every one knows, an increase of hardness, but also suffers a loss of ductility. If the heat to which the steel is raised just before plunging is too high, the metal acquires intense hardness, but it is so brittle as to be worthless; the fracture is of a bright, granular, or sandy character. In this state it is said to be *burned*, and it cannot again be restored to its former strength and ductility by annealing; it is ruined for all practical purposes, but it is in just this state that it again shows differences of structure corresponding with its content in carbon.

The general nature of these changes induced by heat and tempering are sufficiently marked to be visible to an untrained eye. The following set of specimens has been prepared to illustrate them; the six samples show at one extremity the effect of *burning*, in the centre that of a heat which gives the maximum of hardness together with strength, and at the other end the fracture of the bar in the state it left the rolls.

The great molecular changes thus rendered evident are probably accompanied by changes of a chemical character between the iron and carbon. I am not yet prepared to assert it as a fact. It has, however, been stated by Caron, that chemical changes do really occur under these circumstances.

There is a physical property which is well known to be intimately connected with chemical structure, viz., density, and in the case of union between gases it has risen to be sometimes the criterion of combination even.

The specific gravity of steel and iron has been taken many thousand times before this, but not usually in conjunction with analysis, and it is believed that a study of the densities of a series of steels, under varied conditions, and as a sequel to analytic work, would develop facts of interest.

Accordingly samples were taken from the above twelve ingots by boring out a piece with a crown drill, breaking off the core left by the tool, and then grinding and polishing the surface smooth. Also six bars drawn from the ingots were heated to *burning* at one end and were left cold at the other, then plunged into water, thus forming sets like the fractures just exhibited. Each bar was then broken into six pieces and the ends rendered smooth so that their specific gravity could be taken.

In the following table the results are given. The upper horizontal line contains the numbers belonging to the ingots, the left hand vertical column gives the order of the pieces broken from the bars.

TABLE II.

Report of the Specific Gravities of twelve samples of Steel from the ingot (unheated and not hammered). Also of six hammered bars, each bar being overheated at one end and cold at the other; in this state plunged into water, and then broken into pieces of equal length.

NO'S.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
INGOT	7·855	7·836	7·841	7·829	7·838	7·824	7·8194	7·8181	7·813	7·807	7·803	7·805
BAR Order of samples from bar.												
Burned 1			7·818	7·791		7·789		7·752		7·744		7·690
2			7·814	7.811		7·784		7·755		7·749		7·741
3			7·823	7·830		7·780		7·758		7·755		7·769
4			7·826	7·849		7·808		7.773		7·789		7·798
5			7·831	7·803		7·812		7·790		7·812		7·811
Cold 6			7·844	7·824		7·829		7·825		7·826		7·825

Temperature to which the densities are referred is 60° F.

It is thus seen from the table that the density decreases with the increase of carbon from No. 12 to No. 5, which contains $\frac{8}{10}$ of one per cent. of carbon. Below this number the influence of various physical conditions, such as rapidity of cooling, degree of fluidity before casting, etc., influence the specific gravity in an apparently erratic manner, though the numbers still continue to run parallel with the decrease in the quantity of carbon, in a general sense. Also if the influence of temperature on density is noted with regard to the sets of hardened samples, it will be seen that taking the numbers from 12 to 6 (or those containing the highest amounts of carbon) the specific gravity is lower, the higher the temperature applied.

On a Silicious Deposit from the Interior of a Hollow Mass of Limonite, with Observations on the Molecular Movements of Finely Divided Matter. By N. T. Lupton, of Nashville, Tenn.

While engaged recently in the examination of a Silicious Deposit found in a nodular mass of Limonite known among miners as "pot ore," it occurred to me that the substance in question was of sufficient importance to justify a record of the analysis, and of the subsequent investigation to which it led. This variety of ore consists of hollow concretionary masses of various sizes, the cavities being frequently filled with water.

Fossils are sometimes found adhering to the inner surface, or imbedded in the silicious matter contained within the mass. The external surface of these concretions is rough and irregular, while portions of the smooth interior often exhibit the most beautiful iridescent colors.

The "iron pot" referred to contained, when broken open, a quantity of water from which the silicious sediment had evidently settled. This sediment presented to the eye a brownish, earthy appearance, with minute glistening particles of silica. It was soft to the touch, easily pulverized by the fingers, and was given to me for examination on the supposition that it consisted of infusorial earth capable of use as a polishing powder. When heated in a platinum crucible, it blackened and evolved gaseous combustible matter which burned with a yellowish flame.

After the combustion was completed, a grayish-colored ash remained, consisting chiefly of silica. An analysis yielded the following result:

Moisture at 100° c	1·22
Volatile and combustible matter	18·31
Silica	66·65
Ferric oxide	14·20
Total	100·38

The microscope revealed no infusorial shields, but with a magnifying power of 300, this substance exhibited so well the molecular movements common to finely divided matter, that it led to an investigation of this phenomenon with results, in some respects, different from those hitherto recorded. These so-called Brownian movements, described by Robert Brown in 1827, have not, so far as I know, been satisfactorily explained. They are common to minutely divided matter whether organic or inorganic, and so life-like under favorable circumstances as to be readily mistaken by the inexperienced microscopist for vital movements.

Various explanations have been given to the phenomenon. It has been attributed to currents in the liquid in which the matter is suspended, to heat, electricity, magnetism, and even to the natural molecular motion of matter itself.

Dr. Carpenter attributes these movements to heat; he says, "the movement is not due (as some have imagined) to evaporation of the liquid, for it continues without the least abatement of energy in a drop of aqueous fluid that is completely surrounded by oil, and is, therefore, cut off from all possibility of evaporation; and it has been known to continue for many years in a small quantity of fluid enclosed between two glasses in an air-tight case. It is, however, greatly accelerated and rendered more energetic by heat; and this seems to show that it is due either directly to some calorical changes continually taking place in the fluid, or to some obscure chemical action between the solid particles and the fluid, which is indirectly promoted by heat."

Dr. Beale speaks of these movements as taking place "in a fluid not viscid."

In my investigations, various substances were used, such as the silicious earth above mentioned, the finely divided sediment from river water, finely divided silica, various precipitates, and finely pulverized Limonite from which the water of combination had been expelled. The first and last substances enumerated furnished the most satisfactory results, which are as follows: —

1. Each moving particle seems to have an independent motion, and does not appear to be attracted and repelled by surrounding particles. Occasionally particles come in contact and again separate, but not as though influenced by mutual attraction and repulsion. The motions are not rhythmical, nor are they confined to oscillations and rotation, but include also changes of place in the field of view.

2. Heat does not appear to increase or diminish the rapidity of the movements, nor does it, so far as I could see, have any effect upon them. The material was suspended in boiling water, and closely observed through an immersion lens magnifying 800 diameters, while the water gradually cooled to the temperature of the surrounding air; it was then cooled by means of ice with no apparent diminution of the movements. Similar results were obtained by gradually warming the liquid.

3. While neither heat, magnetism, nor chemical reagents seem to have any effect, there is an apparent increase in the rapidity and amplitude of the movements, especially in the larger particles when under the influence of strong light, indicating a connection between this force and the movements in question.

4. It is not necessary for the better production of these movements, as is stated in Griffith and Henfry's Micrographical Dictionary, that the specific gravity of the matter, and of the liquid in which it is suspended should be "as nearly as possible coincident." Finely divided Ferric Oxide produced results equally as good as lighter substances.

5. These movements are not confined to water and other lighter liquids, but can be seen when the matter is suspended in a viscid liquid such as glycerine. In this case, only the smallest particles were observed in motion, and the amplitude of the movements was much less than in water.

On the Chemical Composition of a Saline Efflorescence occurring at Goat Island. By Edw. W. Morley, of Hudson, Ohio.

This efflorescence occurs on small shelves and other projections of rock at the base of the precipice between the American and Canadian Falls. These shelves are tolerably well protected from rain by the over-hanging rock above them. The quantity of the efflorescence is small: only two grammes were obtained for examination. It was grayish white, pulverulent, yielded water in the closed tube, was two-thirds soluble in water, and nine-tenths soluble in dilute acids. The insoluble residue had the appearance, under the microscope, of fine splinters and worn grains of quartz. There were determined in one portion; the water given off over sulphuric acid, at 110 degrees Centigrade, and at 170 degrees; the part soluble in water, that insoluble in acids, and that soluble in acids by difference; all the acids and bases of the part soluble in water; and the bases of the part soluble in acids; the determination of the acids of this part was lost by accident.

The part soluble in water consists of calcium and magnesium sulphates, with a small quantity of potassium and sodium sulphates. The atoms of calcium and magnesium are in the ratio of 81 and 100.

The result of the analysis of the efflorescence is as follows:

Calcium sulphate, with water of cryst.	23·9	
Magnesium sulphate, with water of cryst.	41·9	
Sodium and potassium sulphates	2·1	
Soluble in water		67·9
Calcium and magnesium carbonates, etc.	21·6	
Alumina and oxide of iron	2·7	
Soluble in acids		24·3
Insoluble residue	7·8	
	100·0	

TITLES OF OTHER PAPERS READ IN THE PERMANENT SUBSECTION OF CHEMISTRY.*

On the Limit of Reliability in the Indirect Estimation of Sodium and Potassium Chlorides. By H. W. Wiley, of Lafayette, Ind.

Some Modified forms of Apparatus. (*a*) Flue with artificial draft. (*b*) Flue with artificial draft and air bath. (*c*) Apparatus for sugar and fat extraction. By H. W. Wiley, of Lafayette, Ind.

Some Indigenous Woods of Indiana. Their specific gravity, per cent. of ash in wood and bark. By H. W. Wiley, of Lafayette, Ind.

A Note upon the Rocks of the Galapagos Islands. By F. A. Gooch, of Cambridge, Mass.

Notes of a Mineralogical Tour in Western North Carolina, made under the auspices of the State Survey. By Alexis A. Julien and H. Carrington Bolton, of New York, N. Y.

On the Chemical Composition of Pennsylvania Petroleum. By S. P. Sadtler, of Philadelphia, Pa.

On the so-called Alkali of the Western Plains. By B. S. Hedrick, of Washington, D. C.

A Sugar Analysis. By Alfred Springer, of Cincinnati, Ohio.

A Method for the Analysis of Milk. By E. H. Von Baumhauer, of Haarlem, Netherlands.

*Nearly all the papers read in the Chemical Section are printed in full in the *American Chemist* for November and December, 1876.

Action of Moderate Heat on Bituminous Coal. By E. T. Cox, of Indianapolis, Ind.

Nitrates in Natural Waters, and in Lechauweki Spring-water. By W. H. Chandler and E. H. S. Bailey, of Bethlehem, Pa.

Determination of Nitric Acid. By W. H. Chandler and E. H. S. Bailey, of Bethlehem, Pa.

Disposition of Phosphorus in the Blast Furnace. By W. H. Chandler and Frank Johnston, of Bethlehem, Pa.

SECTION B,

NATURAL HISTORY.

ADDRESS

OF

PROFESSOR EDWARD S. MORSE,

VICE PRESIDENT, SECTION B.

LADIES AND GENTLEMEN: —

IT would be pleasant indeed if only a lecture or an essay were expected from the presiding officer of the Section; but an address implies a great deal more, and the giver of it is not only expected to be entertaining, where perhaps he never entertained before, but instructive upon grounds on which, perchance, he has made but partial survey. Among the many questions of sustaining interest, a number of subjects intrude themselves. A general review of the work accomplished since the last meeting of the Association would seem an appropriate subject for discourse. Yet beyond my special studies I feel quite incompetent to scan so broad a field. In this year of centennial reviews, one might naturally fall into an attempt to sketch the growth of science and the work accomplished within the last hundred years, but that would not only be too vast a field, but would on the whole be unprofitable, since time-boundaries, like the surveyor's lines bordering a state, have no definite existence in Nature. The natural boundaries of oceans and sierras do indeed isolate and impress peculiarities of thought and action upon man, as upon the creatures below him, and for this reason we may with propriety examine the work of our nation in any line of investigation. Never before has the study of animals been raised to so high a dignity as at present. While chemistry could point to its triumphs in the arts, and geology to the revelations of hidden wealth in the rocks, zoölogy was for the most part a mere adjunct to geology, or a means to thwart the ravages of insects. Now, however, it is the pivot on which the doctrine of man's origin hinges. The worlds themselves are too old to study, though the spectroscope reveals the existence of celestial protoplasm as their physical basis. The rocks are too rigid and the time too immense to come within the compass of our minds, but the living facts of evolution are with us to-day in these graceful forms and their constant changes, while the records more or less preserved in past times give us a clew to things hinted at in the earlier changes of present existing forms. It seems, therefore, at the present time, that a review of the work accomplished by American students for the doctrines of natural selection might be acceptable for several reasons, and first among them might be mentioned the fact that thus far no general review of the kind has been made; and, secondly, that with few exceptions all the general works upon the subject are from English or German sources, and filled with the results of work done there oftentimes to the exclusion of work done elsewhere. The oft-repeated examples in support of the derivative theory belong to Europe. The public is familiar with these facts only, and comes naturally to believe that these examples alone exist, and from their remoteness do not carry the weight of equally or perhaps more suggestive facts which lie concealed in the technical publications of our own societies. A review of the work accomplished by American students bearing upon the doctrine of descent must of necessity be brief. Even a review of a moiety of the work is beyond the limits of an address of this nature. And for obvious reasons I must needs here restrict it to one branch of biology, namely, zoölogy. For material, the scientific publications of the country have been scanned, and an attempt has been made to bring together the more prominent facts bearing upon natural selection. In this review the zoölogical science of the country presents itself in two distinct periods: The first period, embracing as to time-limits the greatest portion, may be recognized as embracing the lowest stages of the science; it included among others a class of men who busied themselves in taking an inventory of the animals of the country, an important and necessary work to be compared to that of the hewers and diggers who first settle a new country, but in their work demanding no deep knowledge or breadth of view. And so the work to be done in tabulating the animals has more often been done by specialists who neither knew nor cared to know the facts lying beyond the limits of their studies; a work often prompted by the same spirit that one sees among children in the collection of birds' eggs and postage-stamps. The workers in this class were compared by Agassiz to those who make the brick and shape the stone for the edifice, an indispensable work, but with it was raised not the edifice but an almost insuperable barrier against the acceptance of views more in accordance with reason and common-sense. So thoroughly interwoven with this work were certain conceptions believed to be infallible, that overpowering indeed has been the argument to render as coadjutors the very men who so thoroughly opposed Darwin at the outset. It seems unnecessary to point out the mode of work adopted by the class above described. Their honor involved as soon as their name had been attached to a supposed new species, and any deviation from the type oftentimes persistently overlooked, what wonder, when every local variety received a new name and that name stamped upon a supposed valid creation—what wonder, I repeat, that whole groups of animals have been so thoroughly scourged by such work that few have the courage to engage in the task of revision?

Emerson's reflections on the science of England in 1847 would apply with far more propriety to our country even at a much later date, where in his words "one hermit finds this fact and another finds that, and lives and dies ignorant of its value." With the noble examples of Dana, Wyman, Leidy, and Burnett, before them, they did not profit. In fact, the labors of these honored men, and early in the century Lesueur and others, gave the country its largest claim to recognition abroad. The second period dates from the advent of Agassiz in this country. With his presence a gradual but entire change took place. He rendered the study a dignity rather than a pastime. No longer were the triflers to fling their loose work before the academies unrebuked. The protests he uttered in this Association were the means of elevating the tone of the communications. In fact, nothing indicates the poverty of our attainments in zoölogy more than an examination of the volumes preceding Agassiz's presence and the succeeding volumes. With his honest repudiation of all that was bad, he frightened away the lighter chaff, and there was but little solid work left to take its place. Agassiz made men, and his example, and the methods of work taught by him, spread to other parts of the country. He brought the American student into intimate acquaintance with the classical work of European naturalists. In his public lectures the names of Cuvier, Von Baer, Leuckart, and others, became familiar. The public caught the enthusiasm of this great teacher, and money was lavishly given by the citizens and the State in aid of his scientific undertakings. Agassiz's earnest protest against evolution checked the too hasty acceptance of this theory among American students. But even the weight of his powerful opposition could not long retard the gradual spread of Darwin's views; and now his own students, last to yield, have, with hardly an exception, adopted the general view of derivation as opposed to that of special creation. The results of his protest have been beneficial in one sense. They have prompted the seeking of proofs in this country, and now our students are prepared to show the results of their work in evidence of the laws of progressive development, and it is mainly this work that I wish to review.

So much is claimed for birthplace that, in the way of history, it may not be amiss to call attention to the fact that the first clear premonition of the theory of natural selection came from this country.

William Charles Wells, born in this country, at Charleston, South Carolina, in 1757, in a paper read before the Royal Society, in 1813, first substantially originated the theory to account for the black skin of the negro. He limits his application to races of men and certain peculiarities of color, correlated with an immunity from certain diseases; in proof of it he cites domesticated animals, and the selection by man in precisely the same line of argument urged by Darwin. In the preface to the last edition of the "Origin of Species," Darwin refers to Wells's essay as entitled to the credit of containing the earliest known recognition of the principle. Dr. Wells first shows that varieties among men as among animals are always occurring, and having cited the way in which man selects certain qualities among domesticated animals and thus secures different breeds, calls attention to the well-known fact that the black as well as the white races are differently affected by certain diseases peculiar to the countries which they inhabit. He finds a coincidence between the immunity from certain diseases and the black color of the skin, though why this is so he does not attempt to explain. He thinks that, through the successive survival of dark skins, the dark variety of the human race has become fixed. Referring to man's selective action regarding domesticated animals, he says: "But what is here done by art seems to be done with equal efficacy, though more slowly, by Nature, in the formation of varieties of mankind fitted for the country which they inhabit." These sentences have such a Darwinian sound that, when we remember they were dragged from obscurity by Mr. Darwin himself, we can share in what a recent writer[1] happily calls "Mr. Darwin's evident delight at discovering that some one else had said his good things before him, or has been on the verge of uttering them." As early as 1843, Prof. Haldeman[2] discussed some of the arguments brought forward by the opponents of the Lamarckian theory, and offered certain views in favor of the transmutation of species. While he does not hint at the laws of natural selection, he recognizes fully the value of varieties and their persistency and ultimate divergence. He says, "Although we may not be able artificially to produce a change beyond a given point, it would be a hasty inference to suppose that a physical agent acting gradually for ages could not carry the variation a step or two farther, so that instead of the original one we will say four varieties, they might amount to six, the sixth being sufficiently unlike the earlier ones to induce a naturalist to consider it distinct."

In the year 1850, Dr. Joseph Leidy, in a paper on entophyta in living animals, wrote as follows: "The essential conditions of life are five in number, namely: a germ, nutritive matter, air, water, heat, the four latter undoubtedly existing in the interior of all animals."[3] Dr. Leidy affirms his belief that very slight modifications of these essential conditions of life were sufficient to produce the vast variety of living beings upon the globe. In an early stage of the controversy, Prof. Jeffries Wyman expressed his views in regard to the Origin of Species in the following unmistakable language. He said "we must either assume on the one hand that

[1] Gray's "Darwiniana," p. 284.

[2] "Journal of the Boston Society of Natural History," vol. iv. p. 368.

[3] "Proceedings of the Philadelphia Academy," vol. iii, p. 7.

living organisms commenced their existence fully formed, and by processes not in accordance with the usual order of nature, as it is revealed to human minds, or, on the other hand that each species become such by progressive development or transmutation; that, as in the individual so in the aggregate of races, the simple forms were not only the precursors, but the progenitors of the complex ones, and that thus the order of nature, as commonly manifested in her works was maintained."[4]

The theory of derivation based upon the principles of natural selection demands the following admissions: that species vary, that peculiarities are transmitted or inherited, that a greater number of individuals perish than survive, and that the physical features of the earth are now and have been constantly changing, and that precisely the same conditions never recur. These are admitted facts. Now comes the theoretical part of natural selection, namely, that the varieties which survive are those which are more in harmony with the environments of the time. These propositions, with minor ones, form the theory of Darwin. Lamarck and others had recognized the gradual enhancement of varieties into species, but had not struck the key-note of natural selection, though Wells in the beginning of the century had clearly recognized it in a pertinent example. If we look impartially at these propositions, we need no demonstration to prove the inheritance of characters the most minute, and even the perpetuation of the most subtile features.

On general principles, too, the proposition, that those individuals best adapted to their surroundings survive, need only be stated to be accepted by a reasonable mind. In truth, to deny it would be to deny, as Alphonse de Candolle says, that a round stone would roll down-hill faster and farther than a flat one. Indeed, this eminent botanist affirms that natural selection is neither a theory nor an hypothesis, but the explanation of a necessary fact.

The constant physical changes in the past and present condition of the world are incontrovertibly established. It seems, then, that the prime question resolves itself into whether each species as a whole has something inherent which prompts it to vary irrespective of its environments, or whether a correlation can be established between the variation of species and certain physical

[4] "Am. Jour. Sci. and Arts," second series, vol. xxxvi. No. 107, Sept. 1863, p. 296.

conditions inducing these variations, and here let me add that of all groups of animals from species through genera to higher divisions, that group of individuals recognized as a species has the most tangible existence. And, as a proof of this, there need only be mentioned the fact that many naturalists, while regarding species as clearly distinct, have on the other hand looked upon classification as an artificial method to facilitate the study, and hence the innumerable schemes and the successive interpolation of subclasses, sub-orders, sub-families, and sub-genera, which simply circumscribed smaller groups than had before been recognized.

The rapid multiplication of some of these groups has already formed a serious obstacle to the study of systematic zoölogy.

What would good Dr. Mitchell have said if he could have foreseen the generic lists of to-day! In an article on the "Proteus of Lake Erie," he expressed his aversion to multiplying names in zoölogy, and lamented the tendency. He protested as follows, fifty years ago: "By some, these innovations have been so wantonly introduced, as almost to threaten in the end the erection of every species into a distinct genus."[5] Though these words were undoubtedly aimed at Rafinesque, they were none the less prophetic.

Whatever may be said of the existence in nature of other groups, there can be no question that species have the most definite existence, and it would seem then that nothing more need be proved for the theory of descent as opposed to the theory of special creation, than the establishment of the fact that species assume the characters of new species, or disappear altogether with a change of surroundings. As examples might be cited, the transplanting of Alpine seeds to warmer regions below, and an accompanying change of the plant into another species before known in the warmer region, or, more remarkable still, the change of a species of *Crustacean* which lives in salt water, to another species with a partial freshening of the water, and this freshening slowly persisted in, the form changing into another genus, and in so doing losing one of its segments. In the first case we see the effect of temperature, and in the second case the physical influence of salt and water in different proportions.

Now, these and hundreds of similar examples can be incontestably proved.

[5] "American Journal of Sciences and Arts," vol. vii, 1829.

Even the prolonged existence of the form of some animals, like *Lingula*, may be referred to an inherent vitality which enables them to survive changes that caused the death of thousands of others.

In an early discussion of Darwin's theory,[6] Prof. Agassiz cited the persistence of *Lingula* as fatal to the theory, and Prof. William B. Rogers replied that the vital characters of some animals would enable them to survive above others. Ten years later, I had an opportunity of studying living *Lingula* on the coast of North Carolina, and brought specimens home alive in a small jar of water, and kept them in a common bowl for six months without the slightest care. Their power of surviving under changed conditions—their vitality, in other words—seems incredible.[7] (For further details, *see* reference below).

It has for a long time been suspected that the species of Mollusca, described in such profusion in this country, would be reduced when the slightest attention to their habits had been made. Dr. James Lewis[8] long ago observed that a certain species of fresh-water mussel, described as *Alasmodonta truncata*, is only the truncate form of another species, *A. marginata*. From a careful study of the conditions surrounding the first form in the Mohawk River, he had reason to believe that the rapid currents which pass over it bear along substances that, coming in contact with the exposed edges of the shell, break them down, thus retarding the growth of the shell at this point, and the animal concentrates its growth-powers to the repairs of the broken portion. The same gentleman also shows that the so-called species *Lymnæa elodes*, *catascopium*, and *marginata*, "are modifications of one type or species, influenced by locality and temperature varying the method of development." [9]

A. G. Wetherby[10] calls attention to the variation in form of a group of fresh-water snails, found in the greatest abundance in certain streams of Tennessee and North Alabama. In showing the varied influences to which they are subjected, he cites the rapid currents of the channels, and the greater liability of the snails being torn from the rocks. He shows that they are exposed

6 "Proceedings of the Boston Society of Natural History," vol. vii, p. 231, December 15, 1860.

7 Ibid., vol. xv, p. 315.

8 Ibid., vol. v, p. 121.

9 Ibid., vol, v, pp. 121-128.

10 "Proceedings of Cincinnati Society of Natural Sciences," No. 1, June, 1876.

in various ways to the effects of these currents, with all their changing impetus of high and low water—exposed also to privation of food from the scouring sand removing the confervæ, upon which they subsist, from the rocks. He takes into account temperature, chemical action, and the like, and says, "No greater vicissitude can be imagined than this growth in an unstable element." Coincident with these diverse conditions he finds an enormous variety of forms, and frankly acknowledges that many of those described as distinct species must be reduced to synonyms.

George W. Tryon, in his large work on the American Melanians, published by the Smithsonian Institution, having finished his manuscript in 1865, says, under date of 1873, when the work was finally published, "A more enlarged acquaintance with fresh-water shells convinces me that a much greater reduction of the number of species than I have attempted must eventually be made."

If we now look upon the definition of a species, as given by a gentleman foremost in the ranks as a describer of species, we find it formulated as follows: A species represents "a primary established law, stamped with a persistent form (a type) pertaining solely to itself, with the power of successively reproducing the same form, and none other;" and this gentleman has not hesitated to base these "primary organic laws" upon the evidence of a single specimen, and in some cases even the fragments of one have offered him a sufficient inducement!

But it has been argued by some that a wide variation may be the case with many species. Prof. Agassiz,[11] at a meeting of the American Academy, reiterated his opinion that what are called varieties by naturalists do not in reality exist as such. He found a great abundance of diverging forms in Echinoderms, which, without acquaintance with connecting ones, would be deemed distinct species, but he found they all passed insensibly into each other.

Prof. Parsons suggested that more extended observations might connect received species by intermediate forms, no less than so-called varieties; and Prof. Gray remarked that the intermediate forms, connecting by whatsoever numerous gradations the strongly divergent forms with that assumed as a type of a species, so far from disproving existence of varieties, would seem to furnish the

[11] "Proceedings of the American Academy," vol. v, p. 72.

best possible proof that these were varieties. Without the intermediate forms they would, it was said, be taken for species; their discovery reduced them to varieties, between which (according to the ordinary view) intermediate states were to be expected.

Recognizing, then, the existence of varieties, and of varieties sufficiently pronounced to have led careful naturalists to regard them as distinct species, what shall we say when it is found that these marked forms are correlated with certain physical conditions, many of which have originated within comparatively recent times? Dr. J. G. Cooper,[12] after a careful study of the California land snails, ascertained that "species, sub-species, and varieties, living in cool, damp situations, become more highly developed (but not always larger) than the others; the shell assuming a more compact (imperforate) form, and losing those indications of immaturity referred to, viz., sharp, delicate sculpture, bristles, and angular periphery. These characteristics, however, remain more or less permanently for indefinite periods, and give that fixedness to the various forms, even when living under the same conditions, which enables us to retain them as sub-species differing from varieties in permanency, and from races in not inhabiting distinct regions." It may be added that Stearns, Bland, and Binney, have likewise observed the same peculiar variations associated with aridity.

In a broader field, and compassing different classes, Prof. Spencer F. Baird, Mr. J. A. Allen, and Mr. Robert Ridgway, have severally shown that marked and specific changes are seen in birds and mammals corresponding to differences in their surroundings. Prof. Baird, in a paper entitled "The Distribution and Migration of North American Birds,"[13] has shown that birds in high altitudes and those bred at the North are larger than those born South and at low altitudes; that Western birds of the same species have longer tails than eastern examples, and that the bill increases in size in those birds occurring in Florida as compared with those found north of that State, and that on the Pacific coast the birds are darker in color than those found in the interior.

Mr. J. A. Allen[14] has made a more special study of this matter, and his work ranks among the most important contributions to

[12] "Proceedings of the California Academy of Natural Science," vol. v, p. 128.
[13] "American Journal of Science and Arts," vol. xli, January and March, 1866.
[14] "Proceedings of the Boston Society of Natural History," vol. xv, p. 156.

this science. Mr. Allen finds that there are marked geographical variations in mammals and birds. He shows that northern mammals of the same species are more thickly and softly furred, and that toward the south the peripheral parts, such as the ears and feet, are more developed. The same law holds good in birds, a diminution in size being observed toward the south, and the individuals being darker in color.

As one goes south he meets with the same species of birds, whose bodies are shorter, but whose beak, tail and claws, are longer. On the Plains, also, he found the birds with plainer tints, while southward the colors became more intense. On drawing up a table indicating the regions of lighter varieties, and comparing it with a chart of mean annual rainfall, Mr. Allen found the lighter forms occurred in dry regions, and the dark forms in relatively humid regions. To sum up: Mr. Allen finds in latitudinal variation climatic influences affecting color as well as altering the size of bill, claw, and tail, while longitudinal variation usually affects color alone.

He states that these laws are now so well known that a species may be predicted to assume a given color if under certain specific climatic conditions.

Mr. Robert Ridgway[15] has in a similar way called attention to the relation between color and geographical distribution in birds as exhibited in melanism and hyperchromatism, and has shown that red areas "spread" or enlarge their field in proportion as we trace certain species to the Pacific coast, and that in the same proportion yellow often intensifies in tint.

The results of these investigations can be easily understood. Nearly if not quite one hundred and fifty species of birds, which were recognized as distinct, are at once reduced to varieties, though less than twelve years ago they were looked upon as good species, with which no external influence had anything to do. Nearly if not quite a fifth of the number of species of birds have been reduced by the investigations of Baird, Allen, Coues, and Ridgway.

The mammals, through the same study of geographical variation, will have been reduced at least one-fourth. Already Mr. Allen[16] has studied the geographical variation of the squirrels, and the

[15] "American Journal of Science and Arts," vol. iv, December, 1872, p. 454, and vol. v. p. 39.

[16] "Proceedings of the Boston Society of Natural History," vol. xiv, p. 276.

result is that a reduction has been made of one-half the number of species before recognized. Prof. Baird, in his monograph of North American squirrels, reduced the number from twenty-four, as acknowledged by Audubon and Bachman, to ten well-established species and two doubtful varieties. Allen, with still greater advantage in the shape of a mass of material from the Western surveys, reduced the ten species to five species, with seven geographical varieties.

Should it be urged that the present tendency toward reducing species be taken as an evidence that species had not before been properly defined, then it offers a stronger argument still in favor of the fact that species are even more variable than had before been supposed, leaving the greater possibility of larger numbers of these ultimately surviving. Again, the assumption that the limitation of specific variation had not been properly indicated, shows how reprehensible has been the work of some of those who have burdened our literature with their bad species.

The fact is, the work has in a measure been justifiable, and is not to be wholly condemned. The workers in this field have followed the teachings of their masters. A group of individuals removed from an allied group of individuals by an extra dot or darker shade, perpetuating their kind from generation to generation, marked with persistent characters, and in every way coming up to the standard recognized as specific, had the right to be judged as such. It is only when a whole series of forms are collected, and climatic influences are seen to affect these in the same way that they affect other groups of species even in different classes, that the mere influence of moisture and temperature is shown to be the sole cause of many of these supposed specific characters.

Dr. A. S. Packard, in his remarkable monograph of a group of moths, the Phalænidæ, published under the auspices of the Hayden Survey, finds that with some species there are changes analogous to those pointed out by Baird and Allen; and while he does not find enough to establish a law, yet to his mind enough is seen "to illustrate how far climatic variation goes as a factor in producing primary differences in faunæ within the same zones of temperature," and he admits that varietal and even specific differences may arise from these climatic causes alone. Dr. Packard, in the same work, under the head of "Origin of Genera and Species," says, "The number of so-called species tends to be reduced as our specimens and information increase." The genera also "are as artificial creations as species and varieties. The work of the systematic biologist often amounts to but little more than putting Nature in a strait-jacket."

An application of the influence of temperature is here proper, as explaining, on a rational ground, the persistence of peculiar arctic forms of animals and plants on the summits of Mount Washington and other high peaks. With a knowledge of glacial phenomena, we are capable of judging of the condition of things which must, of necessity, have existed directly after the recedence of the great ice-sheet: its southern border slowly retreating, and, with the encroachment of the warmer zone, the arctic forms dying out, or surviving under changed conditions; but, in high plateaus and mountains, local glaciers flourished for a while, and at their bases arctic forms flourished, and, lingering too long, were ultimately cut off by the retreat of the main field. This interpretation of arctic forms on high peaks, though attended to by several American naturalists, is not new. Oswald Heer, in discussing the origin of certain animals and plants, coincides with De Candolle that Alpine plants are relics, as it were, of a glacial epoch. Prof. Gray[17] had also independently arrived at the same conclusions, based on a comparison of the plants of Eastern North America and Japan. In the position he maintained regarding the derivation of species from preëxisting ones, he stood far in advance of his brother naturalists in this country, for this was before Darwin's great work had appeared, and before Heer had developed the host of fossil plants from the arctic zone. Mr. S. I. Smith, in speaking of mountain faunæ, points out the gradual encroachment of glaciers, and the drawing down of northern forms; and, as the glaciers retreated, these forms were caught, "the mountain-summits being left as aërial islands." Dr. Packard and Mr. Scudder have severally called attention to the same thing.

Prof. A. R. Grote has more fully dealt with the subject in a paper read before this Association, and in a graphic way shows that the "former existence of a long and widely-spread winter of years is offered in evidence through the frail brown Œneis butterflies, that live on the top of the mountains within the temperate zone." I have been thus explicit, in order to contrast these more

[17] "Memoirs of the American Academy," vol. vi, pp. 377-458 (1859).

rational views with those formerly entertained by eminent naturalists, whose minds were imbued at the time with the idea of special creation. Mr. Samuel H. Scudder[18] read before the Boston Society of Natural History an account of distinct zones of life on high mountains, as illustrated in the insect-life of Mount Washington. He called attention to certain insects which he supposed peculiar to the summit, and not found farther north, though showing a remarkable correspondence to certain arctic forms. Prof. Wyman asked whether all the facts might not be accounted for on the theory of migration northward after a glacial epoch, and Prof. Rogers suggested that the facts might be accounted for on the migratory theory if we added thereto the supposition of subsequent variation induced by isolation. Yet these views were persistently opposed by the other naturalists present. The mass of evidence already contributed, as to the extraordinary variation in color, markings, and size of species coinciding with their physical surroundings, though perhaps trivial in itself, becomes important when the proofs are grouped together, and all bear upon the theory of derivation. So slight a thing as change of food is found to influence certain animals even to a degree usually regarded specific. The late Dr. B. D. Walsh[19] discovered some very curious features among insects connected with a change of food. First, he established the fact that insects accustomed to one kind of plant could acquire a taste for another kind, and he has shown that in thus changing the food of the insect a change took place in the appearance of either the larva, pupa, or imago, and sometimes all three stages were affected. Dr. Fitch had observed that changing an insect's larva from the leaf to the fruit affected the appearance of the larva. It would be impossible to give even an abstract of Dr. Walsh's remarkable essay. It may be said, however, that his investigations led him irresistibly to the conclusion that the present species have been derived from preëxisting ones, and in numberless cases he is capable of showing the successive stages from the dawn of a plant-eating variety, where the changes are slightly seen in the larva only, to the plant-eating species in which profound changes are seen in the larva, pupa, and imago.

The minor factors of natural selection, such as protective color-

18 "Proceedings of the Boston Society of Natural History," vol. ix, p. 230.

19 "On Phytophagic Varieties and Phytophagic Species," "Proceedings of the Entomological Society of Philadelphia," vol. iii, p. 403.

ing and mimicry, have been variously illustrated by Mr. R. E. C. Stearns, Dr. Kneeland, Prof. Cope, Dr. Charles C. Abbott, and others. In a special paper on "The Adaptive Coloration of Mollusca," [20] I have endeavored to show not only a wide-spread application of this feature to mollusks, and especially those exposed by the tide, but in some cases a mimicry of inanimate objects, as the accumulation of clay or grains of sand upon the shell.

Wallace's theory of birds'-nests finds interesting confirmation in the observations of Dr. Abbott, who made a special study of a large number of robins'-nests, and found the widest variation among them. He studied also the nests of the Baltimore Oriole, where, according to the theory of Wallace, a concealing nest should be made, the bird being exceedingly bright-colored. He found that, away from the habitations of man, the orioles built concealing nests; but in villages and cities, on the other hand, where they were in no special danger from predatory hawks, the nests were built comparatively open, so that the bird within was not concealed.[21]

The differences in the habits of animals of the same species are noticed in different parts of the country, and such facts militate against the idea that certain unerring ways were implanted in them at the outset. Indeed, such facts go to show that these various creatures not only become adapted to their surroundings, but that individual peculiarities manifest themselves. The observations of Dr. Coues, Mr. Allen, and Mr. Martin Trippe, go to prove that certain birds change their habits in a marked degree. In their behavior, too, certain birds, which are wild and suspicious in New England, are comparatively tame in the West. In their nesting-places they show wide individual variation.

Prof. A. E. Verrill,[22] on the supposed eastern migration of the cliff-swallow, traces historically its first appearance in various places in the East, and is inclined to the opinion that as the country became settled by Europeans the birds left their native haunts for barns and houses, and increased in number to a greater extent than before on account of the protection invariably furnished by man.

Rev. Samuel Lockwood[23] records a curious case of the Baltimore Oriole acquiring a taste for the honey-sacs of bees, tearing

[20] "Proceedings of the Boston Society of Natural History," vol. xiv, p. 141.
[21] "Popular Science Monthly," vol. vi, p. 481.
[22] "Proceedings of the Boston Society of Natural History," vol. ix, p. 276.
[23] "American Naturalist," vol. vi, p. 721.

off the heads of those insects, and, having secured the honey-sacs, rejecting the rest of the body.

Prof. Wyman[24] observes a curious case in Florida, of a colt and a number of pigs and cows thrusting their heads under water and feeding on the river-grass, in some cases remaining with their heads immersed for half a minute.

Hon. L. H. Morgan[25] observes the widest difference in the habits of the same species of beaver in the Lake Superior region and in the Missouri, constructing their dams and ways differently, and meeting the varied conditions, not by a blind instinct, but by a definite intelligence manifested for definite purposes.

All of these facts, simple in themselves, together go to prove that animals do vary in their habits, and with a persistent change in habits arises the minute and almost insensible pressure to swerve and modify the animal.

So much does the influence of season, with its accompanying peculiarities of food, temperature, humidity, and the like, affect certain animals developing coincidently with its different phases, that it is instructive to note that in certain species of insects two or three different forms occur. Thus Mr. Edwards[26] has in an elaborate way worked up the history of a polymorphic butterfly (*Ephiclides ajax*), showing that there are three forms heretofore regarded as distinct species, which are only varieties of one and the same species, but appearing at different times of the year, and consequently confronted by different influences as to temperature, moisture, food, and the like. These forms are known under the names of *Walshii*, *Telemonides*, and *Marcellus*, and both sexes are equally affected. The first form mentioned represents the early spring type, *Telemonides* the late spring type, and *Marcellus* the summer and autumn type (*see* also Mr. Scudder's paper).[27] If these influences affect species, we should expect to see the greatest variety of forms in a country possessing the widest diversity of conditions.

Some suggestive paths of investigation have been pointed out by Prof. N. S. Shaler[28] on the connection between the development of the life and the physical conditions of the several continents,

[24] "American Naturalist," vol. viii, p. 237.
[25] "The American Beaver and his Works."
[26] "Butterflies of North America," part ix.
[27] "American Naturalist," vol. viii, p. 257.
[28] "Proceedings of the American Academy," vol. viii, p. 349.

showing first that the greatest amount of shore-line in proportion to the internal areas indicates a greater diversity of surface within.

Another proposition he attempts to establish: that in proportion to the shortness of the shore-lines, or, in other words, to the want of variety in their surfaces, will be the diversity of animal life in the continent. He then proceeds from Darwin's standpoint, and follows out many curious and instructive lines of thought regarding increased amount of influences in diversified surfaces — a level plain having the same conditions throughout, but a mountainous region having for each one thousand feet of elevation a new condition of things, in the form of streams, winds, humidity, and the like. In areas of simple outline and unvarying surfaces we do, in fact, have a less diversity of forms.

Recognizing the mutation of continents through past geologic ages, we again see the accompanying physical changes in not only modifying forms, but in selecting them afterward by succeeding changes.

The widely-diversified nature of the facts bearing on the doctrine of natural selection baffles all attempts at a systematic classification of them. Of such a nature are many of the valuable communications of Prof. Wilder.

At the meeting of this Association[29] he has, among other matters, confirmed in a young lion the discovery of Prof. Flowers that, in the young dog and probably in other carnivora as well, the scapho-lunar bone has at the outset three centres of ossification, and that these really represent the *radiale*, *intermedium* and *centrale* of the typical carpus. By the study of a fœtal manatee, Prof. Wilder is able to determine its affinities, and to point out the probable retrograde metamorphosis of some ancient ungulate animal, and that the manatee is widely removed from the whales with which it has been associated.

Mr. William K. Brooks[30] has published a very remarkable paper on certain free swimming tunicates, the *Salpa*, giving for the first time a clear and comprehensive history of certain obscure points, and has at the same time applied the principles of natural selection theoretically in showing the origin of Salpa from sessile tunicates, and making clear the peculiar modification of parts which accompany these changes.

[29] "Proceedings of the American Association Adv. of Sci.," vol. xxii, p. 301.
[30] "Bulletin of Museum of Comparative Zoölogy," Vol. 1, p. 291.

In the field of entomology, some capital work has been done, both practical and theoretical.

Prof. Riley's demonstration of the yucca-moth is unique in its way. Dr. Engelmann has discovered that the yucca depends upon insects for fertilization; and Prof. Riley, by patient study, not only discovered the moth which fertilizes the flower, but finds an anomalous change in the maxillary palpi of the insect, by means of which the moth collects bundles of pollen, which it inserts into the stigmatic tube, and during this peculiar act deposits her eggs in the young fruit. Prof. Riley has reasons to believe that this is the only insect engaged in the fertilization of this plant. A mutual dependence is here met with of extreme interest. The yucca, unfertilized, forms no fruit, and the larva of the moth consequently perishes.

Prof. Augustus R. Grote, in an examination of butterflies, finds successive gradation in their structures, and shows that as these organs "become less serviceable to the insect they become more rigid and in position more elevated above the head in the butterfly, while in the moth they are more whip-like and directed forward." While protesting against the separations which have been made in the order based upon the antennæ, he directs attention "to the real differences in antennal structure between the butterflies and moths, while showing that the antennæ are modified by desuetude in the higher and former group." Prof. Grote,[31] in dealing with a family of moths, the *Noctuidæ*, calls attention to the unequal value of *Acronycta*, and is forced to admit that these differences become clear through the theory of evolution. He says: "Where in *Acronycta* there is a general prevailing uniformity in the appearance in a single group of species and generally broad distinctions between the larval forms, it is a not unreasonable conclusion that these larval differences are gradually evolved by a natural protective law, which intensifies their characters in the direction in which they are serviceable to the continuance of the species."

Those who have believed in types as fixed laws, rigidly impressed at the outset of life, are those also who have recognized in the cells of a honey-bee, as well as in the arrangement of leaves about the axis of a plant, a perfect mathematical adjustment of parts, which were stamped at the beginning, and have so continued to exist

[31] "Proceedings of the Buffalo Society of Natural Sciences," vol. i, p. 130.

without deviation. For nearly two hundred years it has been believed that the instinct of a bee guided it to shape a cell which of all other forms should use the least amount of material. A theory having been established as to the constant shape of a bee's cell, namely, that it was an hexagonal prism with trihedral bases, each face of the base being a rhomb with certain definite angles, a mathematician was given the problem to construct similar cells, and to determine the best possible form with the use of the least amount of material. The coincidence between theory and observation and experiment was so remarkable as to settle apparently for all time the question as to the perfectly-implanted instinct of the bee with its unconscious power of accurate work. Prof. Jeffries Wyman,[32] to whose memoir I am indebted for the above facts has, by an ingenious study of the cells of bees, shown first, that a cell of this perfection is rarely if ever attained. Furthermore, that, while the honey-cells "are built unequivocally in accordance with the hexagonal type, they exhibit a range of variation which almost defies description;" that the worker-bees, from incorrect alignment and other causes, build cells, the measurement of which shows the widest limit of variation; that the drone-cells are liable to substantially the same variations, while the transition-cells, namely, those in which drones and worker-cells are combined in the same piece of comb, are extremely irregular. As the drone-cells are one-fifth larger than worker-cells, "a transition cannot be made without some disturbance in the regularity of the structure." And Prof. Wyman states distinctly that the bees do not have any systematic method of making the change, adding that "the cell of the bee has not that strict conformity to geometrical accuracy claimed for it," and the assertion, like that of Lord Brougham, that there is in the cell of the bee "perfect agreement between theory and observation, in view of the analogies of Nature, is far more likely to be wrong than right, and his assertion in the case before us is certainly wrong." Prof. Wyman closes his essay by saying that "much error would have been avoided if those who have discussed the structure of the bee's cell had adopted the plan followed by Mr. Darwin, and studied the habits of the cell-making insects comparatively, beginning with the cells of the humble-bee, following with those of the wasps and hornets, then with those of the Mex-

[32] "Proceedings of the American Academy," vol. vii, p. 68.

ican bees, and finally with those of the common hive-bee; in this way they would have found that, while there is a constant approach to the perfect form, they would at the same time have been prepared for the fact that even in the cell of the hive-bee perfection is not reached. The isolated study of anything in Nature is a fruitful source of error."

The remarkable ingenuity, so characteristic of Prof. Wyman's experiments, is fully shown in this memoir. He made plaster-casts of the comb, and then sawed transverse sections, and by slightly heating the plaster the wax was melted and absorbed, leaving the delicate interspaces representing the partitions. From these sections electrotypes were taken, and thus the veritable figures were used to illustrate the absolute structure of the comb. The results of these brilliant researches were published in the "Proceedings of the American Academy of Sciences."

In the "Memoirs of the American Academy of Sciences" may be found a profound mathematical essay "On the Uses and Origin of the Arrangement of Leaves and Plants,"[33] by the lamented Chauncey Wright. After discussing the laws of phyllotaxy, and showing that the botanist is wrong in supposing this a law at the outset, Mr. Wright states "one of the utilities, so to speak, in the apparently undeviating arrangement of leaves, to be the distributing of leaves most rapidly and thoroughly around the stem, exposed more completely to light and air, and provided with greater freedom for symmetrical expansion, together with more compact arrangement of bud;" and he asks, "What has determined such an arrangement of vital forces? Theory of types would say, their very nature, or an ultimate creative power. Theory of adaptation would say, the necessity of their lives, both outward and inward; or the conditions, both past and present, of their existence.

Whatever tends to show modification in the markings, color, size, food, or change in the variety of habits manifested by animals, furnishes just so many indications of the unstable character of what had before been considered stable, and gives an infinitely wider field for those unconscious selections whose operations are coincident with every change in the physical features of the earth. On the theory of derivation additional confirmation is given to the deductions of geologists based upon the stratigraphical and pale-

[33] "Memoirs of the American Academy," vol. ix, p. 379.

ontological evidences of the rocks. The survival of a marine crustacean in the deeper waters of Lake Michigan, as discovered by Stimpson, coupled with similar occurrences in the lakes of Sweden, suggests the past connection of these waters with the ocean. In the same way the persistence of arctic forms on high mountain-tops indicates the existence in past times of wide-spread glacial fields. The interesting discoveries of Mr. Ernest Ingersoll, in the Rocky Mountains, of the occurrence of two species of marine mollusks and living crabs belonging to marine forms, and tiny air-breathing mollusks peculiar to the Gulf coast and West Indies, point as distinctly to the past connection of that region with the ocean as the records of marine life left in the rocks. And more than this, the survival of these few forms gives us a conception of the thousands of animals which have succumbed to the changed conditions. Connected with the evidences of recent elevation of this region are the discoveries of Marsh in finding that, when the gill-bearing salamander *Siredon* is brought down from the colder waters of the Rocky Mountains to the warmer waters below, a complete change takes place in a loss of the gills and the conversion of the animal into the air-breathing genus *Amblystoma*.

This exhibits on a wider scale the experiments often performed in keeping tadpoles in the dark and cold, and indefinitely retarding their development, thus forcing them, as it were, to retain their earlier condition. Among the many millions of individuals of Amblystoma, some must have presented the anomaly of a premature development of their ovaries before the larval stage had passed away (similar cases being observed among insects), and thus it has been possible for them to perpetuate their kind in this stage. The Axolotl, having the longest persisted in this mode of growth, has become, as it were, almost fixed in these retrograde characters, only a few examples being known in which the creatures have lost their gills and assumed the mature characters of Amblystoma, but with Siredon a change takes place with a proper change of surroundings.

To American students we are indebted for most valuable contributions regarding the effect of cave influences on animals living within their boundaries. Looking at the cave fauna with its peculiar assemblage of animals, it would seem that here, at least, the question as to the effects of certain external influences, of the absence of others in modifying structure, might be found.

Many years ago the editors of "Silliman's Journal" addressed a letter to Prof. Agassiz respecting the blind fishes of the Mammoth Cave, and asked his opinion as to whether their peculiar structure was due to their cave life, or whether they had been specially created. Agassiz's[34] reply is consistent with his belief. He says, "If physical circumstances ever modified organized beings, it should be easily ascertained here." He then expresses his conviction that "they were created under the circumstances in which they now live, within the limits over which they range, and with the structural peculiarities which characterize them at the present day," adding frankly, however, that these opinions are mere inferences.

With the contributions on cave insects by the eminent zoölogist Schiödte, and our own naturalists as well, we have now overwhelming proof that the blind fishes and numerous other cave animals are marked with peculiarities impressed upon them by the unusual environments to which they have been subjected.

In a work on the animals of the Mammoth Cave, by Dr. A. S. Packard and Mr. Putnam, the first-named writer quotes the results of Schiödte, wherein he shows the existence of twilight animals in which but slight modification occurs, while in darker places the changes become more profound.

Dr. Packard[35] sums up the results of his work as follows: "We then see that these cave animals are modified in various ways, some being blind, others very hairy, others with long appendages; all are not modified in the same way in homologous organs, another argument in proof of their descent from ancestors whose habits varied as their out-of-door allies do at present."

Prof. E. D. Cope,[36] in an article on the fauna of Wyandotte Cave, in commenting on the loss of eyes in cave animals from absence of light, and consequent disuse, says that, to prove it, "we need only to establish two or three propositions: 1. That there are eyed genera corresponding closely in other general characters with the blind ones. 2. The condition of the visual organs is in some cave type variable. 3. If the abortion of the visual organs can be shown to take place coincidently with general growth to maturity, an important point is gained in explanation of the *modus operandi*

[34] "American Journal of Science," second series, vol. xi, p. 128.
[35] "Life in the Mammoth Cave," p. 27.
[36] "American Naturalist," vol. vi, p. 415.

of the process." He then proceeds to point out a number of related genera in which the external ones present eyes, while the cave forms are blind. As to variability, he cites the blind siluroid fish from Conestoga, Pennsylvania, showing that, while all of several specimens were blind, the degree of atrophy was marked not only in different fishes, but even on different sides of the same fish. In some the corium was perforate, in others it was imperforate. In some the ball of the eye was oval, in others collapsed.

We have in the meagre fauna of the caves convincing proof of the gradual undoing of parts—so to speak—on the withdrawal of influences favorable to them; even so exquisite a structure as the eye as a result of selection almost inconceivable, yet not only becoming rudimentary, but almost disappearing, by the withdrawal of those influences which were in part conducive to its building up. So distinct are these undoing stages that, were we sure of the stable variability of all of them, we could with certainty indicate the relative age of each cave inhabitant.

Prof. Alpheus Hyatt and Prof. E. D. Cope almost simultaneously established a number of propositions relating to certain large groups of animals which had never been recognized before. The theory of acceleration and retardation in which certain groups acquire rapidly new characters, while corresponding groups acquire the same characters more slowly, forms a portion of the theory of these naturalists. Prof. Hyatt has shown among Ammonites a parallel between the life-stages of the individual and similar stages in the group based upon an examination of suites of specimens as studied by him in Europe and America. It is utterly impossible to do the slightest justice to the thoroughly original views of these gentlemen without the aid of explanatory diagrams. While reluctantly abandoning the attempt, I must at the same time express the regret that neither of these investigators has seen fit to present to the public an illustrated and simple outline of the main features of their theories and the facts: Prof. Cope basing in part his propositions on groups of animals, many of which comprise fossil forms brought to light in the West, of which but few restorations have yet been made; and Prof. Hyatt basing his work on fossil Ammonites from the Jurassic and adjacent beds of Europe, of which but one complete collection is to be found in this country.

Surely, with this unfamiliar material, an excuse may be offered in not attempting a popular presentation of propositions and laws, some doctrinal and others theoretical, which must yet be looked upon as profound and permanent additions to the philosophy of evolution. A reference may be made to Prof. Cope's essays, entitled "Origin of Genera," "On the method of Creation of Organic Types," "Consciousness in Evolution," "On the Theory of Evolution," and numerous other memoirs from which may be gathered the author's views on the subject. The essays of Prof. Hyatt, "On the Parallelism between the Different Stages of Life in the Individual and those in the Entire Group of the Molluscan Order Tetrabranchiata," "Reversions among Ammonites," Evolution of the Arietidæ," "Genetic Relations of the Angulatidæ," "Abstract of a Memoir on the Biological Relations of the Jurassic Ammonites," are altogether too technical to condense into an address of this nature. It need hardly be mentioned that in these memoirs invaluable contributions are made to the doctrines of natural selections.

And now we come to the most difficult part of our work; to compass within the limits of a few pages the magnificent discoveries of Leidy, Marsh, and Cope, in the rich fossiliferous beds of the West. The wonders are so unique and varied; they have been poured upon us with such prodigality of material and illustration, that one is baffled in an attempt to compass their characters, or to picture them as realities. When Darwin offered the imperfection of the geological record as possibly accounting for the absence of intermediate forms which might have existed, he was at once met by a series of protests so strenuous, and at the same time so specious, that they had their full weight in staying the force of that prophetic chapter. Darwin, in this chapter, distinctly stated that not only were there forms which had never yet been seen, owing to the imperfection of the geological record, but that time might possibly bring them to light, and, when discovered, we should have revealed to us intermediate characters which would connect widely-separated groups as they are recognized to-day.

Behold the prophet! Animals have been discovered, not only showing the characters of two widely-separated groups, but in some cases of three groups as they now appear. How distinct the hoofed quadrupeds, the carnivora, and the rodents, appear to-day! Yet here are discovered ancestors of these widely-separate groups, in which are contained in one individual the characters of all three! Of the ungulates with the perissodactyle foot, there have been discovered a large number of tapiroid forms allied to Paleotherium; others which, like Anchitherium, wonderfully fill the gap between the horse and forms lower down; a large suite of rhinocerotic creatures of strange character and enormous size; a great number of species of three-toed horse, some no larger than foxes, and with these a perplexing maze of deer, antelopes, sheep, camels, hippopotami, and pig-like animals, ruminant-like beasts, some of them not larger than an ordinary squirrel: a curious group, comprising a large number of species with characters intermediate between the pigs and ruminants. Prof. Flower, the great English osteologist, confesses that these forms completely break down the line of demarkation between them, and adds that "a gradual modification can be traced in the characters of the animals of this group, corresponding with their chronological position, from the earlier more generalized to the latest comparatively specialized forms, thus affording one of the most complete pieces of evidence that are known in favor of a progressive alteration of form, not only of specific, but even of generic importance through advancing ages." The probable home of the Camelidæ has been revealed in the discovery of llama-like creatures, gigantic mammals, in some cases exceeding the elephant in size, but with a diversity of characters hitherto unseen either in recent or fossil forms, combining as they did the characters of perissodactyle and proboscidian.

A numberless variety of Carnivora, many of them embracing the most generalized groups, have been brought to light, such as creatures between the wolf and the opossum, generalized dogs, and sabre-toothed cats.

A great many species belonging to the Rodentia, Insectivora, and Chiroptera, have been identified; still more wonderful is a group of creatures so unlike any beast heretofore known that Prof. Marsh has made a new order to include them under the name of *Tillodontia*. They combine the characters of several distinct groups, namely, the carnivores, ungulates, and rodents, and some of them in size equalling the tapir. Of great interest also is the discovery of fifteen new genera, belonging to low forms of primates. All of these creatures, embracing hundreds of species, are generalized in a high degree. New orders have been erected to embrace some of them. One has only to understand the specialization of modern animals to appreciate the generalized character of these early forms.

Prof. Marsh has shown that all the ungulates in the Eocene and Miocene had upper and lower incisors; and, again, that all the Eocene and Miocene mammals, including the Carnivora, had two of the wrist-bones, the scaphoid and lunar, as distinct bones.

The class of birds so long represented as a closed type can no longer occupy that isolated position. The proper interpretation of Archæopteryx has, in the discoveries of Marsh, new interest. He has discovered a number of species of birds, for which a new sub-class is made. This sub-class will embrace two sub-orders, one in which the creatures had teeth contained in grooves in the jaws; the other had true teeth in sockets. The first were swimming-birds of gigantic size, with rudimentary wings; the second embraeed small birds, with powerful wings and bi-concave vertebræ.

Prof. Cope has also brought to light a remarkable gigantic bird from the Eocene of New Mexico; its size indicates a species with feet twice as large as those of the ostrich. He shows it to be distinct from any of the genera of *Struthionidæ* or *Dinornithidæ*. Besides all these wonders, a host of new forms of reptiles and fishes have been discovered by these indefatigable explorers—huge pterosauria discovered by Marsh with a spread of wing of twenty-four feet; and of more special interest is the fact that no trace of teeth can be found in the jaws.

It is impossible for me to more than allude to these remarkable additions to our knowledge of these early forms, and until they have all been figured with natural outlines, and perplexing questions as to priority in discovery rectified, it will be difficult in some cases to accredit individual work. But in the light of these profound revelations, how blind seem the attempts to establish a classification on the forms heretofore familiar to us, and to rear these into circumscribed groups between which it was asserted no forms of intermediate kinds were to be expected! With the twenty-five or thirty species of fossil horses at our command, some with four toes, others with three, in various stages of reduction, it is interesting to bring back to mind the earnest Geoffroy St. Hilaire painfully endeavoring to trace the genealogy of the horse, with a few widely-separated forms of extinct mammals as his only guide in the work.

The special investigations of Marsh and Leidy reveal an almost unbroken line from our present horse with its simple toe, and two rudimentary metatarsals in the shape of the splint-bones, to a creature in which metatarsals support rudimentary toes, and still other forms in which these rudimentary toes are working-toes, and below that again another form in which a fourth toe is seen as a rudiment, till forms are reached in which all the toes rest on the ground. It is still more striking to study attentively those earlier generalized horses with four toes, and follow the successive reduction in the number of toes as the later formations are reached, till in the latest deposits and at present we have the modern specialized horse with but a single toe, the lost toes represented by two slender bones hidden beneath the flesh. And now comes crowning proof that our modern horse has been derived from some three-toed progenitor, for in certain instances horses have come into existence with splint-bones developed into sturdy bones sustaining at the extremities phalangeal bones, and outside accessory hoofs! Such freaks of Nature demand an explanation. They receive a rational one through the theories of Darwin. Without the law of reversion, we are left in blind bewilderment.

While all these facts, in overwhelming array, testify to the extreme mutability of forms, induced oftentimes by apparently the most trivial of causes, and set at rest the question as to the fixedness of species, they show at the same time the richness of that store from which, by natural selection, forms may be selected.

Realizing the uniformity of Nature's laws, the human mind bravely asks, "Do these wonderful interpretations throw any light upon the origin of man?"

Rigidly adhering to the inductive method, science is prepared to show that man did not appear suddenly and free from those animal proclivities and passions which make him a sinful creature, but that he has risen from a lowly origin, and his passions and desires, but feebly repressed, may be as surely traced to ancestral traits, as the aberrant muscles in his structure may be recognized in some degraded progenitor. And in proof of this there is established a series of facts of precisely the same nature as is seen in those discoveries which link the horse in an almost unbroken line to earlier and more generalized animals.

It is instructive to read the discussions in relation to man's position in Nature as represented by Agassiz, Morton, and others.

The position that these eminent men were justified in taking shocked the Church, and received from her the same vigorous denunciations that Darwin was forced to bear at a later day.

The systematist, in formulating the separate species and genera of the apes and monkeys, was early led to see that man also in various parts of the world presented differences quite as striking, and if it were assumed, as indeed it was, that the peculiarities among men were only varietal, then it could be claimed with equal emphasis that the differences among apes were only varietal. Agassiz, in his keen grasp of things, readily saw this, and, since the races of men revealed differences just as specific in their characters as the animals immediately below them, he was forced to admit the plurality of origin of the human race. He says:

"Unless we recognize the differences among men, and we recognize the identity of these differences with the differences which exist among animals, we are not true to our subject, and, whatever be the origin of these differences, they are of some account; and if it ever is proved that all men have a common origin, then it will be at the same time proved that all monkeys have a common origin, and it will by the same evidence be proved that man and monkeys cannot have a different origin."

He confesses that he "saw the time coming when the position of the origin of man would be mixed up with the question of the origin of animals, and a community of origin might be affirmed for them all." With these convictions it is not surprising that he should have been led to express the opinions regarding the diversity of the human race that we find recorded.

Agassiz, in the meetings of the American Academy, repeatedly and in various ways illustrated the diversity of the human race. In one place he alludes to the difficulty in defining the species of man, and says the same difficulties occur in defining the species of anthropoid apes. We quote from the records:

"The languages of different races of men were neither more different nor more similar than the sounds characteristic of animals of the same genus; and their analogy can no more be fully accounted for on any hypothesis of transmission or tradition than in the case of birds of the same genus uttering similar notes in Europe and America."—("Proceedings of the American Academy," vol. iii, p. 6.)

Again, in a later volume, he expresses a general disbelief in the supposed derivation of later languages from earlier ones. He regarded each language and each race as substantially primordial, and ascribed their resemblances to a similarity in the mental organization of the races.

This extract illustrates the extremity to which one is logically driven if he accepts the hypothesis of special creation, and these words are quoted, not with the belief that at the present time they would have been uttered, but as illustrating the necessary admissions with the theory of plurality of origin. In precisely the same manner that Whitney, Müller, and other eminent philologists, have shown the outgrowth of present existing languages from primitive forms of language, so science is prepared to show the outgrowth of present men from primitive forms of animals. Agassiz was bitterly assailed by the Church for the bold attitude he assumed regarding the plurality of origin of the human race, though now that science will show that after all man has originated from a common centre, it seems no better satisfied.

The facts bearing on man's lowly origin have been fully contributed by American students, and, as all intelligent men understand the bearing of these facts on the question, it is only necessary to allude to them here. If man has really been derived from an ancestor in common with the ape, we must expect to show:—1. That in his earlier stages he recalls certain persistent characters in the apes; 2. That the more ancient man will reveal more ape-like features than the present existing man; and, 3. That certain characteristics pertaining to early men still persist in the inferior races of men.

Prof. Wyman[37] points out certain resemblances between the limbs of the human embryo and the permanent condition of the limbs of lower animals. In some human embryos about an inch in length he found that the great toe was shorter than the others, and, instead of being parallel to them, projected at an angle from the side of the foot, thus corresponding with the permanent condition of this part in the Quadrumana.

In some observations made on the skeleton of a Hottentot, Prof. Wyman[38] calls attention to the complete ossification of the nasal bones, no trace of a suture remaining. This was more noticeable

[37] "Proceedings of the Boston Society of Natural History," vol. x, p. 185.
[38] Ibid., vol. ix, p. 352.

as the individual was young, and the other bones were immature, and had an interest "in connection with the fact that the nasal bones are coössified at an early period in the monkeys and before the completion of the first dentition in gorillas and chimpanzees." Careful measurements of the pelvis also revealed quadrumanous features, though "the resemblance is trifling in comparison with the differences."

In a study of the crania, Wyman[39] found differences in the relative position of the *foramen magnum*. In the North American Indian this opening was farther back than in the negro, while some crania from Kauai presented this opening still farther back than in the Indian; and more than half the lot from Kauai had the peculiarity in the nostrils first pointed out in the negro by Dr. John Neil, of Philadelphia, namely, the deficiency of the sharp ridge which forms the lower border of the opening. In its place is a rounded border, or an inclined plane.

This feature occurs very frequently in different races, but more rarely in Europeans. It is, however, never absent in the apes. Prof. Wyman, in studying the characters of certain ancient crania from a burial-place near Shell Mound, Florida, observed the *foramen magnum* quite far back, and remarks on the massive character of the bones composing the skull, the parietal being nearly twice the thickness of ordinary parietals, while the general roughness of the surfaces for muscular attachments on the hinder part of the head is very striking.[40]

In certain measurements of synostotic crania, Prof. Wyman found that the length of the parietals was twenty-four millimetres above the average, the parietals being lengthened from before backward, the frontal and occipital being but slightly augmented. Now, in the much-discussed Neanderthal skull, wherein it is urged by Dr. Davis that it is a synostotic skull, though denied by Huxley, Wyman shows that the parietals measure nine millimetres *below* the average, which is certainly against the view that the Neanderthal skull is synostotic.[41]

In an essay entitled "Observations on Crania and Other Parts of the Skeleton," Prof. Wyman shows that the relative capacity of the skull "is to be considered merely as an anatomical and not as

[39] "Proceedings of the Boston Society of Natural History," vol. xi, p. 447.
[40] "Fourth Annual Report of the Peabody Museum of Arch. and Ethn.," Cambridge.
[41] "Proceedings of the Boston Society of Natural History," vol. xi, p. 455.

a physiological characteristic,"[42] a most important distinction certainly in considering the large capacity of certain ancient skulls, since we must know the quality as well as the quantity in order to assume the intellectual position of the races. In this essay are also quoted the results of a large series of measurements made by Dr. B. A. Gould, in which it is shown that the arms of the blacks are relatively longer as compared with the whites, in this respect approaching the higher animals, a confirmation of the observations made by Broca, Pruner Bey, Lawrence, and others.

The perforation of the humerus, which occurs in the apes quite generally, was found to occur rarely in the white race. Of fifty humeri, Wyman found but two perforated, while of Indian humeri he found thirty-one per cent. perforated. In some of the remains of ancient men there has been found a remarkable lateral flattening of the tibia, unlike anything found at present, but always characteristic of the earliest races. These tibiæ have received the name of platycnemic tibiæ.

Wyman[43] quotes Broca as saying that the measurements of these tibiæ resemble the ape, and, what is more striking, in a small number of instances "the bone is bent and is strongly convex forward, and its angles so rounded as to present the nearly oval section seen in the apes." The occurrence of these platycnemic tibiæ has been noticed by several investigators. They have been obtained from the mounds of Kentucky by Mr. Carr, Mr. Lyon, and Mr. Putnam. Prof. Wyman found them in Florida mounds. To Mr. Henry Gillman, of Detroit, science is indebted for the discovery of the flattest tibiæ ever recorded, exceeding even those discovered in Europe. Mr. Gillman has opened a number of mounds along the Detroit and Rouge Rivers in Michigan, and assiduously studied the characters of these remains, which indicate a very ancient race of men. Many of these tibiæ he has sent to the Peabody Archæological Museum at Cambridge. Associated with these remarkable tibiæ he found large numbers of perforated humeri.

At the Detroit meeting of the Association, Prof. W. S. Barnard showed that the muscles which move the fingers and toes have been developed from one common muscle, and, in studying the various degrees of specialization of the muscles which move the hand and

[42] "Fourth Annual Report of the Peabody Museum of Arch. and Ethn."
[43] Ibid.

foot in the gorilla and lower apes, he finds that in the foot "man remains a creature of the past not modified by that which makes him a man, the brain. The hand has been modified and perfected by its services to the brain." Prof. Barnard also contributed another essay, entitled "Comparative Myology of Man and Apes." From very careful studies he is led to believe that the relative position of the origin of the muscles is more constant than that of their insertions. In this examination he brings to light a muscle which Traill dissected in the higher apes, and which he called the *scansorius*, and this was supposed to have no representative in man.

Traill was followed by Wyman, Owen, Wilder, and Bischoff, who, in a controversy with Huxley, argued from this muscle against the simian origin of man. Mr. Barnard now shows that Traill was mistaken, and that other naturalists were misled by the weight of his authority. What Traill interpreted as the *gluteus minimus* is the *pyriformis*, and what he figured as a new muscle separating the apes from man, the *scansorious* is the *homologue* of our *gluteus minimus*.

From gradually accumulating data, in regard to microcephalic skulls, it would seem as if Carl Vogt was right in judging them to be cases of reversion. Prof. Wyman says, in regard to a microcephalic skull from Mauritius, that, "taking together the high temporal ridges, the union of the temporals with the frontals, the projection of the jaws, the narrow and retreating forehead, the small capacity, and the form and proportions of the nasal openings, the general resemblance to that of an ape is most striking, and seems to justify Vogt's expression of a man-ape, it being understood that the skull we are describing is not a natural, but an anomalous formation."[44]

It would be difficult to imagine, indeed, that mere reduction in the size of the brain, through arrest of development, should produce a series of characters so closely resembling the apes as is found to be the case in so many widely-separated examples. Thus, in the Mauritius microcephalic skull the capacity is only twenty-five cubic inches. The jaws are extremely prognathous, the zygomatic arches stand out wide and free, and the temporal ridges approach within one and a quarter inch. If such examples should prove to be veritable cases of reversion, then we have a parallel in the startling appearance of the long-lost rudimentary toes of

44 "Seventh Annual Report of the Peabody Museum of Arch. and Ethn."

the horse, traces of which are only seen in the hidden splint-bones. In the "Seventh Annual Report of the Peabody Museum," Prof. Wyman describes a microcephalic skull from the ancient *huacas* of Peru. Its capacity is only thirty-three cubic inches; "the frontal bone is much slanted backward, has a decided ridge corresponding to the frontal suture, and is slightly concave on each side of it."

Wyman states that the bones of the head are well formed, though, from the diminutive size of the brain, idiocy must have existed.

Associated with the remarkable collection of platycnemic tibiæ and perforated humeri discovered by Henry Gillman, we should have expected some anomalous forms of crania, and in this expectation we are not disappointed.

In company with two skulls which appear to be normal, Mr. Gillman discovered one of most remarkable proportions. Wyman considered it a case of extreme individual variation, and not the result of artificial deformity. The skull in question has only a capacity of fifty-six cubic inches. The average capacity of Indian crania, according to Morton's measurements, being eighty-four cubic inches, and the minimum capacity being sixty-nine cubic inches. This skull of Gillman's is therefore thirteen cubic inches less than the smallest Indian skull heretofore described. But more extraordinary still is the approximation of the temporal ridges. While in ordinary crania the separation of these ridges is usually from three to four inches, and never less than two inches, in this unique skull from the Detroit River mound the ridges in question approach within three-quarters of an inch; in this respect, as Wyman says, presenting the same condition as that of the chimpanzee. A rounded median crest can be distinctly seen and felt between these ridges, and the skull is markedly depressed on each side for the passage of the powerful mastoid muscles.

Is this, too, a case of partial reversion? Such extraordinary forms as the Neanderthal and Engis skulls, and the one above cited, with the La Naulette and other lower jaws, could not have been uncommon in those early days, since the chances against finding them would be simply enormous, unless, indeed, they were of common occurrence. Regarding these remains as we do those of the remains of other mammals, we must admit either that these low characters represent retention of ancestral peculiarities, or that they are cases of reversion. In considering the Neanderthal skull, with its retreating frontal, its enormous frontal crest, and other anthropoid characters, Huxley is led to say that at most there is "demonstrated the existence of a man whose skull may be said to revert somewhat toward the pithecoid type."

To a mind unbiased by preconceived opinions, and frankly willing to interpret the facts as they stand revealed by the study of these ancient remains the world over, the evidences of man's lowly origin seems, indeed, overwhelming.

Looking at the whole question impartially, we find that among recent men there are high types as well as low types, with a variation so great as to have induced Agassiz, Morton, and others, to consider them specific. And while, as Wyman asserts, no one race possesses all the low characters, yet with the relatively long arms, the tendency of the pelvis to depart from the normal proportion, and numerous other facts of like significance, there are yet retained among some of them more resemblance to the higher apes than can be found among others.

Prof. Cope, not content with tracing man back to some ape-like progenitor, has, in a suggestive way, considered man's relations to the Tertiary mammalia. In a communication to the Association at Detroit, on this subject, he prefaced his paper by saying that in the doctrine of evolution two propositions must be established: 1. That a relation of orderly succession of structure exists, which corresponds with a succession in time; 2. That the terms (species, genera, etc.) of this succession actually display transitions or connections by intermediate forms, whether observed to arise in descent, or to be of such varietal character as to admit of no other explanation of their origin." He shows that the primary forms of mammalia are strongly indicated in the structure of the feet, and also in the character of the teeth. In recent land-mammals there are several types of foot to be recognized, the many-toed plantigrade, the carnivorous, the ox, and the horse types. Among the earlier types of the Eocene, he finds the most generalized type in the Coryphodon of Owen (Bathmodon of Cope). This creature was plantigrade, with a short calcaneum, and an imperfect hinge for the foot. From this generalized form he traces a line of succession of intermediate forms to the horse on the one hand, and the ox on the other.

The Coryphodon was one of the earliest known mammals, while the horse and the ox preceded man by a single geological period.

Without entering into a technical description of the successive forms presented by Prof. Cope, we may quote his words wherein he shows that "the mammals of the Lower Eocene exhibit a greater percentage of types that walk on the soles of their feet, while the successive periods exhibit an increasing number of those that walk on the toes; while the hoofed animals and Carnivora of recent times nearly all have the heel high in the air, the principal exceptions being the elephant and bear families." After presenting the gradual osteological changes of the foot, from the earlier types to the later ones, through several lines of descent, and considering also the teeth as well, he says: "The relation of man to this history is highly interesting. Thus, in all generalized points, his limbs are those of the primitive type, so common in the Eocene. He is plantigrade, has five toes, separated tarsals and carpals, short heel, rather flat astragalus, and neither hoofs nor claws, but something between the two; the bones of the forearm and leg are not so unequal as in the higher types, and remain entirely distinct from each other, and the ankle-joint is not so perfect as in many of them. In his teeth his character is thoroughly primitive. . . .

"His structural superiority consists solely in the complexity and size of his brain. A very important lesson is derived from these and kindred facts. The monkeys were anticipated in the greater fields of the world's activity by more powerful rivals. The ancestors of the ungulates held the fields and the swamps, and the Carnivora, driven by hunger, learned the arts and cruelties of the chase. The weaker ancestors of the Quadrumana possessed neither speed nor weapons of offense and defense, and nothing but an arboreal life was left them, where they developed the prehensile powers of the feet. Their digestive system unspecialized, their food various, their life the price of ceaseless vigilance, no wonder that their inquisitiveness and wakefulness were stimulated and developed, which is the condition of progressive intelligence"—adding that "the race has not been to the swift, nor the battle to the strong." Prof. Cope shows in this case that "the survival of the fittest has been the survival of the most intelligent, and natural selection proves to be, in its highest animal phase, intelligent selection."

Prof. Fiske has, in a clearer way, shown that when variations in intelligence became more important than variations in physical structure, then they were seized upon, to the relative exclusion of the latter.

It is intelligent strength, other things being equal, that conquers the savage, and the gradual selection of the best and biggest brains is not seen alone in man.

In one of the most significant discoveries of Prof. Marsh, the mammalia are found to show an increase in the size of the brain coincident with their succession in the rocks.

One of the most extraordinary mammals from the Tertiary beds of the West is the Dinoceras, with its rhinoceros and elephant characters, its skull ornamented with prominent tubercles, its unique dentition, embracing large cutting tusks, and altogether forming a beast like the fabled monsters of old.

A study of its cranial cavity, made by Prof. Marsh, shows that its brain was proportionally smaller than that of any other known mammal. Indeed, it was almost reptilian, and of such diminutive size that it could have been drawn through the neural canal of all the presacral vertebræ. Prof. Marsh has followed up this discovery with the most important results, and is now prepared to state the following conclusions:

1. That all the Tertiary mammals had small brains.
2. There is an increase in the size of the brain during this period.
3. This increase was mainly confined to the cerebral hemispheres or higher portion of the brain.
4. In some groups the convolutions of the brain have gradually become more complicated.
5. In some the cerebellum and olfactory lobes have even diminished in size.

He also finds some evidence that the same general law holds good for birds and reptiles from the Cretaceous to the present time.[45]

Thus we have in other groups, as well as man, convincing proof that, with successive survival of forms, there is a corresponding survival of larger brains.

Prof. Shaler[46] has offered some suggestive thoughts in showing the intense selective action which must have taken place in the shape and character of the pelvis in man, on his assumption of the erect position — the caudal vertebræ turning inward; the lower portion of the pelvis drawing together to hold the viscera, which had before rested on the elastic abdominal walls; the attending

[45] "American Journal of Science," vol. xii, July, 1876.
[46] "Proceedings of the Boston Society of Natural History," vol. xv, p. 188.

difficulties of parturition, and other troubles in those parts—all pointing to the change which has taken place.

In this connection Prof. Shaler remarks that the question of labor in woman must not be overlooked from this standpoint.

In a memoir on the shell-heaps of Florida, by Prof. Wyman, wherein he describes a number of low characters in man already alluded to, he gives the following conclusions: "The steady progress of discovery justifies the inference that man in the earlier periods of his existence, of which we have any knowledge, was at most a savage, enjoying the advantage of a few rude inventions. According to the theory of evolution, which has the merit of being based upon, and not inconsistent with, the observed analogies and processes of Nature, he must have gone through a period, when he was passing out of the animal into the human state, when he was not yet provided with tools of any sort, and when he lived the life of a brute."[47]

These words have no obscure utterance, and when we regard the character of the one who wrote them, his cautious methods of research, and the long deliberation he was wont to give to all such questions, then they become doubly significant.

Recognizing clearly the existence of these lower and earlier stages in man, it has been one of the most difficult problems to solve the first steps toward his society and family relations. Prof. John Fiske, in his "Outlines of Cosmic Philosophy," has given for the first time a rational explanation of the origin and persistence of family relations, and thence communal relations, and, finally, society.

Never before has there been presented so clear an idea of man's physical changes, and the effects of natural selection in seizing upon attendant or correlated nervous changes, as in the work of this author.

Prof. Fiske says: "Civilization originated when in the highest mammals variations in intelligence became so much more important than variations in physical structure that they began to be seized upon by natural selection, to the relative exclusion of the latter."[48]

Starting from the researches of Sir Henry Maine, Lubbock, and others, he finds social evolution must have originated after fami-

[47] "Memoirs of the Peabody Academy of Science," vol. i, part iv.
[48] Fiske's "Cosmic Philosophy," vol. ii, p. 310.

lies temporarily organized among the higher mammals had become permanently organized. But how this step was effected has been an insoluble problem. Bagehot, in his remarkable work on "Physics and Politics," says: "It is almost beyond imagination how man, as we know man, could by any sort of process have gained this step in civilization." Darwin supposes that men were originally weak and inoffensive creatures, like the chimpanzee, and were compelled to band together to make up in combined strength what they lacked as individuals.

That man, for his age, is a weak animal physically, there can be no doubt. Fiske shows that "increase of intelligence in complexity and specialty involves a lengthening of the period during which the nervous connections involved in ordinary adjustments are becoming organized." From these conditions arose the phenomena of infancy, and he shows that with increase of intelligence infancy becomes longer. In the human race it is longer than in any other mammal, and much longer in civilized man than in the savage.

In the orang-outang the infant does not begin to walk till it is a month old, and in performing this act it holds to various objects for support, as in the human infant. Previous to that time it reposes on its back, and becomes absorbed in gazing at its hands and feet. Now, still lower down among the monkeys, at the age of one month the young are fully matured so far as walking and prehension are concerned. It is shown, furthermore, that where infancy is very short, parental feeling may be intense for a while, but soon dies out, and the offspring of one becomes of no greater interest than those of a stranger, "and in general the duration of the feelings which insure the protection of the offspring is determined by the duration of the infancy. . . . .

"Hence if long infancies could have suddenly come into existence among a primitive race of ape-like men, the race would have quickly perished from inadequate persistence of parental affection." Prof. Fiske, in a most reasonable way, shows that "the prolonged helplessness of the offspring must keep the parents together for longer and longer periods in successive epochs; and when at last the association is so long kept up that the older children are growing mature while the younger ones still need protection, the family relations begin to become permanent. The parents have lived so long in company that to seek new companionships involves some disturbance of ingrained habits, and meanwhile the older sons are more likely to continue their original association with each other than to establish associations with strangers, since they have common objects to achieve, and common enmities bequeathed, inherited or acquired with neighboring families."

In his chapter on the moral genesis of man Fiske maintains that "the prolongation of human infancy accompanying the development of intelligence, and the correlative extension of parental feeling, are facts established by observation wherever observation is possible; and to maintain that the correlation of these phenomena was kept up during an epoch which is hidden from observation, and can only be known by inference, is to make a genuine induction, involving no other assumption than that the operations of Nature are uniform. To him who is still capable of believing that the human raee was created by miracle in a single day, with all its attributes, physical and psychical, compounded and proportioned, just as they now are, the present inquiry is of course devoid of significance. But for the evolutionist there would seem to be no alternative but to accept, when once propounded, the present series of inferences."

Recalling now the various evidences educed by Wyman, Gillman, and others, regarding the anomalous characters of the remains of primitive man, it seems impossible that a mind unbiased by preconceived opinion should be able to resist the conviction as to man's lowly origin.

If we take into account the rapidly-accumulating data of European naturalists concerning primitive man, with the mass of evidence presented in these notes, we find an array of facts which irresistibly point to a common origin with animals directly below us, and these evidences are found in the massive skulls with coarse ridges for muscular attachments, the rounding of the base of the nostrils, the early ossification of the nasal bones, the small cranial capacity in certain forms, the prominence of the frontal crest, the posterior position of the *foramen magnum*, the approximation of the temporal ridges, the lateral flattening of the tibia, the perforation of the humerus, the tendency of the pelvis to depart from its usual proportions, and, associated with all these, a rudeness of culture and the evidence of the manifestation of the coarsest instincts. He must be blind indeed who cannot recognize the bearing of such grave and suggestive modifications. Bu what application are we to make of such revelations if we vividly receive them as such? We are no longer to rest with the blind fatalism of the Turks, or listless resignation of the masses, but are to make a living use of them. We are to trace evil and corrupt passions to their source. The dreadful outrages which shock us from time to time in the public prints are not instigated by an evil spirit, but are outbursts of the same savage nature which found more frequent expression years ago, and which are still present with the lower races of to-day. When the study of heredity reveals the fact that even the nature of vagabondage is perpetuated; when the surprising revelations of Margaret, mother of criminals, from whose loins nearly a thousand criminals have thus far been traced, are considered, common-sense will ultimately recognize that the imprisonment of a criminal for ten or twenty years is not simply to punish him or relieve the public of his lawless acts, but to restrain him from perpetuating his kind. No sudden revulsion of feelings and amended ways is to purify the criminal taint, but he is to be quarantined in just the same way that a case of the plague might be, that his kind may not increase. With these plain facts thoroughly understood, men high in authority must find some other excuse for the exercise of their pardoning power, and other reasons be given for allowing so large a proportion of criminals to go free. With the monstrous blot of Mormonism and free-love in our country, the statute-books are to be again revised from the standpoint of science, with its rigid moral and physical laws, and not from the basis of established usage or long-continued recognition.

[Hon. Lewis H. Morgan in dealing with Australian kinships, and showing the status of family relations, shows how common communalism is among them, and even before that the intermarriage of brothers and sisters, and thus significantly refers to some of the excrescences of modern civilization, such as free-love and Mormonism, as reversions to ancestral modes. "The nations of the Aryan family assume not only to be civilized, but to be far advanced in civilization; whereas that is strictly true of a small minority only. Barbarism and savagism still lurk in all cities, and in all corners of civilized lands, repressed by law and restrained by intelligence. We have the same identical brain perpetuated through reproduction which worked in the skulls of the savages and barbarians of by-gone ages; and it has come down to us laden and saturated with the thoughts, aspirations and passions, with which it was busied through the intermediate periods. It is the same brain grown older and larger in the experience of ages. These outcrops of barbarism are so many revelations of anterior proclivities, a kind of mental atavism."—Proc. Am. Acad., Vol. VIII, p. 412.]

PAPERS READ.

On a Provisional Hypothesis of Pangenesis. By W. K. Brooks, of Baltimore, Md.

[ABSTRACT.]

This paper proposes a modification of Darwin's hypothesis of the same name; removing most of its difficulties, but retaining all that is valuable. According to the hypothesis, in its modified form, characteristics which are constitutional, and already hereditary, are transmitted by the female organism by means of the ovum; while new variations are transmitted by gemmules, which are thrown off by the varying physiological units of the body, gathered up by the testicle, and transmitted to the next generation by impregnation.

The ovum then is the germ of an entire organism, and may develop without impregnation; while the gemmules or germs of single units, can develop only after they are united with an ovum. The ovum is essential; the gemmules only important. The ovum is the conservation element, and the female organism the conservative organism; while the male fluid is the progressive element, and the male organism the progressive organism.

Among the many proofs of the truth of this view, we may instance what are known as reciprocal crosses; that is cases in which two allied species are crossed using, first, the male of species A, and the female of species B; and then reversing the process and crossing the male of B with the female of A. If our hypothesis is true the offspring in the first instance should be nearer to A, and in the second case nearer to B.

The mule has an ass for its father and a mare for its mother, while the hinny is the offspring of the stallion and the female ass; and the result agrees with our hypothesis, for the mule has the voice, ears, head, tail and general form and color of the ass, while the hinny has a long tail like that of a horse, its head, ears and hair, are like those of a horse, and its voice is a neigh.

Another proof adduced is the well-known fact, that, throughout the animal and vegetable kingdoms, almost without exception, wherever the sexes differ the male departs more widely than the female from the ancestral form, as this is shown by the young and by allied forms.

On the Plastidule Hypothesis. By Louis Elsberg, of New York.

After having accepted as true the general doctrine of Evolution and the effectiveness of natural selection within its sphere of action,—I felt the need of a clear and logical conception of the mode of propagation and differentiation of organisms; and was gradually led to adopt, and in 1868 or 1869 to announce, a modification of the current development theory. As stated by Mr. Darwin as an inference from analogy (Origin of Species, chap. xiv, conclusion) "probably all the organic beings which have ever lived upon this earth have descended from some one primordial form." So far as I know, neither Mr. Darwin nor any other advocate of evolution had in this connection made any distinction between "primordial form" and "individual primordial being;" whereas I had gradually come to believe that all living beings other than the simple protoplasson have, indeed, descended from one primordial form (viz., protoplasson), but from different individuals of such primordial form which have originated at different times of the earth's existence, and have possibly also more or less differed in chemical constitution; that the individual protoplasson which started upon its evolution first in time is, other circumstances[1] being equal, most highly developed, which means that it may be stated as generally true that the most highly developed organisms are so, simply because they are the oldest in organic existence, those in their constitution nearest to protoplasson being the youngest; that the difference of rank among existing forms is mainly due to the difference of time during which, or the number of times which regeneration has occurred, and that the difference of direction in which development has taken place is in part due to the modifying or adapting influences with which the organism has been surrounded.

Under the name of hypothesis of regeneration these ideas have been published in various places during the past seven or eight years, among others in "London Monthly Microscopical Journal," 1872, p. 182, and in the "Proceedings" of this Association at Hartford, August, 1874.

[1] *i. e.* possible chemical difference in different specks of primitive protoplasson, and different external conditions acting upon them either to retard or accelerate evolution, or change its direction.

Whatever is transmitted by a man and woman to their child must be contained in the two generative elements: *i. e.* in either the female ovum or the male semen. Now, since not only the peculiarities of the immediate parents, but also those of anterior ancestors, may be transmitted, it is certain that the two constituents of the germ, the ovum and spermatic particles, contain molecules to which are attached all these peculiarities. I assume, and this assumption constitutes the foundation of the hypothesis, that *the germ of every derivative living being contains plastidules of its whole ancestry.* Plastidules I explained as being plastid molecules. A plastid is living matter or bioplasson in its ascertained most elementary form, as it is found on ultimate physical or microscopical analysis to compose all organic bodies, plants as well as animals, the highest as well as the lowest. Formerly, the so-called "cell" was considered the form element of organic bodies, and it was hence termed "elementary organism," or "life unit:" now, the ultimate "life stuff" or, to speak with Huxley "physical basis of life" is known to be the substance called "protoplasm" or (to use a designation etymologically more nearly meaning living, forming matter) "bioplasson."

Plastidules or plastid molecules are the smallest particles in which the qualities of a plastid reside: the really "elementary factors of plastid life." I took good care to state ("Proceedings" of this Association, 1874, p. 90), that, "I regard a plastidule quite as much a centre or bundle of force as of matter." Again, "Proceedings," p. 92), I explained: "the germ of a child contains molecules or plastidules or bundles of force derived through its parents from grandparents, great-grandparents, and in fact from a long line of ancestors." Again, when I spoke of the hypothesis ("Proceedings," p. 93), as "the hypothesis of the preservation of organic molecules, because it assumes that certain plastidules are, though not forever, yet for a long time, preserved and transmitted from generation to generation," "I added, "or I might call it the hypothesis of the conservation of organic forces, which with the explanation already made [*i. e.*, that a plastidule is a centre of force quite as much as a centre of matter, and that force or molecular motion is preserved and transmitted quite as much as molecular substance], would express the same thing."

That I have been, nevertheless, misunderstood on this point, I have been made aware by verbal communications from members of this Association and others, and in print by Prof. Hæckel (*Die Perigenesis der Plastidule oder die Wellenzeugung der Lebenstheilchen.* Ein Versuch zur mechanischen Erklärung der elementaren Entwickelungs-Vorgänge. Von Ernst Hæckel. Berlin, 1876), and subsequently also by Mr. E. Ray Lankester (in "Nature," July 13, 1866).

Several personal discussions convinced me that I had not sufficiently explicated the modes of transmission of force which meet the argument of the plastidule hypothesis; it was suggested to me to bring the subject again before this Association for the purpose of laying greater stress upon this aspect of the case; and before I could do so, Prof. Hæckel's Pamphlet on the Perigenesis (*i. e.*, as he explains, the generation of force-waves) of plastidules reached me.

After quoting the very passage from the "Proceedings," p. 93, which I have cited, he says: (Pamphlet cit, p. 74) "As it clearly appears from this and the further details of Elsberg, he agrees in the most essential feature with the Pangenesis hypothesis of Darwin, in as much as both here and there the material transmission of actual molecules throughout the whole series of blood-related genertions, and therefore the material composition of each germ of corporeal particles of its whole ancestry, is asserted. But just this fundamental thought our perigenesis hypothesis opposes. For we assume an immediate transmission of corporeal molecules only from the procreating individual to the procreated, but not also from the older series of ancestors. From these there is transmitted or "inherited" only the particular form of *periodic motion;* and it is only this continuing "wave-motion of the plastidules" which, by virtue of their memory, causes the qualities of the older ancestors to reappear in the later progeny."

To Darwin's Pangenesis hypothesis I shall allude presently. Of the obvious immediate transmission I said ("Proceedings," p. 93) "It must be of course also borne in mind that the transmission of ancestral plastidules takes place in each generation through material derived from the immediate parents only." Concerning the transmission of "*only the particular form of periodic motion,*" I must say that the fact of the inseparable union and interdependence of function and structure,—the fact that whenever we deal in science exclusively with either matter or force, we have to do with but one of two aspects of one and the same thing,—is so firmly rooted in my mind that to me the hereditary transmission of these force-waves involves material transmission, not *necessarily* of plastidules actually preserved from older ancestors, but certainly of plastidules which have in some way become *equivalent* to these; or, as I have elsewhere expressed it, "to which are attached not only the peculiarities [particular molecular motions] of the immediate parents but also those of anterior ancestors reappearing in the child." I should not have expected so close and philosophical a reasoner as Hæckel to exclude some such material transmission,—the less so because from study of his works and personal intercourse, I feel confident that his views of matter and force (resp. modes of motion) are like those I have expressed. I distinctly stated ("Proceedings," p. 90) "Nor is the specific transmission of the identical plastidules the point necessarily insisted upon, since the transmission of particles rendered identical by assimilation and growth meets the purposes of the argument;" and again ("Proceedings," p. 91) "nutritive assimilation or growth impresses new molecules with all the qualities of the old and thus preserves the identity." I admit that I should have entered more fully into the discussion of possible modes of transmission and conservation of molecular force-waves; but I must declare that what Hæckel calls perigenesis of plastidules, so far as I can understand it, is contained in my statement of the regeneration hypothesis, though he gives in his essay greater prominence than I did in mine, to this one of the two correlative ideas of the hypothesis, and elaborates it more ably than I could possibly have done.

As to the Pangenesis hypothesis, I have always taken it to consist *most essentially* in the assumption "that cells, before their conversion into completely passive or 'form material,' throw off minute granules or atoms, which circulate freely throughout the system, and when supplied with proper nutriment multiply by self-division, subsequently becoming developed into cells like those from which they were derived. These granules for the sake of distinction may be called cell-gemmules, or, as the cellular theory is not fully established, simply gemmules." (Darwin's "Variation of Animals and Plants under Domestication," vol. II, chapter xxvii, second part.)

This essential feature of pangenesis I have not adopted in my hypothesis of regeneration. No one at all acquainted with the latter can for a moment believe that its "plastidules" are the same as Mr. Darwin's "gemmules" under another name. In almost all the qualities ascribed by Mr. Darwin to gemmules they differ from plastidules; to wit., in their free circulation throughout the body, their peculiar affinity for each other, their multiplication, etc.; indeed the two views are entirely distinct. Mr. Darwin deals with supposed emanations, *i. e.*, particles proceeding, or being given off, universally and continuously from "cells;" I with the ultimate particles, or molecules, of which the living matter is made up, reaching further back than all so-called "cells."

If any one were to attempt to combine my conception with that of Mr. Darwin, he would, perhaps, have to regard the gemmules "thrown off by every cell or unit of the body, not only during the adult state but during all the stages of development" as combining to form plastidules which would be incompatible with my definition of plastidules, or else that gemmules are made up of plastidules which seems to be as incongruous.

I will add here—although it is in answer rather to Mr. Lankester than to Prof. Hæckel—that Mr. Darwin's view of "the retention of free and undeveloped gemmules in the same body from early youth to old age" and "the long continued transmission of undeveloped gemmules" approaches his hypothesis to mine. Mr. Darwin says of his gemmules: "They are supposed to be transmitted from the parents to the offspring, are generally developed in the generation which immediately succeeds, but are often transmitted in a dormant state during many generations and are then developed. Their development is supposed to depend on their union with other partially developed cells or gemmules which precede them in the regular course of growth." Again he says: "According to my view, the germs or gemmules of each separate part were not originally pre-formed, but are continually produced at all ages during each generation, with some handed down from preceding generations." Again; "The aggregation of gemmules derived from every part of the body, through their mutual affinity, would form buds, and their aggregation in some special manner, apparently in small quantity, together probably with the presence of gemmules of certain primordial cells, would constitute the sexual elements." Again: "The transmission of dormant gemmules during many successive generations is hardly in itself more improbable, as previously remarked, than that of the retention during many ages of rudimentary organs, or even only of a tendency to the production of a rudiment; but there is no reason to suppose that all dormant gemmules would be transmitted and propagated for ever." And again: "All organic beings, moreover, include many dormant gemmules derived from their grand parents and more remote progenitors, but not from all their progenitors." Finally: "Reversion depends on the transmission from the forefather to his descendants of dormant gemmules, which occasionally become developed under certain known or unknown conditions." In so far as these expressions mean that the germ of every derivative living being contains particles derived from not only its immediate progenitors but also remoter ancestors, I am with them in entire accord, and do not claim either priority or originality; yet, as my regeneration hypothesis involves a modification of the theory of development held by Mr. Darwin, I certainly can not take the liberty of ascribing the hypothesis to him.

Prof. Hæckel criticizes my advocacy of Heitzmann's histological views. In the exception he takes, he is, however, unfortunate; for the very statement he makes in supposed opposition—viz., that the network arrangement found in living matter is a secondary phenomenon—is one published by Heitzmann in his original communication to the Vienna Academy *Ueber die Lebens-phasen des Protoplasmas* (On the life-phases of protoplasma) June 26, 1873, with which I entirely agree. The real and fundamental point of divergence Hæckel does not mention. This is, that while Hæckel clings to Virchow's politico-physiological comparison, that every higher organism is like an organized social community or state, in which the individual citizens are represented by the "cells," each having a certain morphological and physiological autonomy, although on the other hand interdependent and subject to the laws of the whole;—Heitzmann's views necessitate the comparison of the body to a machine, such as, for instance, a steam-engine, in which though there are single parts, no part is at all autonomous but all combine to make up one individual. According to Hæckel the body is composed of colonies of amœbæ; according to Heitzmann the body is one complex amœba.

The application of the term "memory," as first introduced by Prof. Hering, to the persistence or reproduction of the qualities of plastidules is a very happy one; and, altogether, Hæckel's pamphlet is a valuable contribution to the mechanical explanation of the elementary processes of development.

Mr. E. Ray Lankester's article in "Nature" July 13, 1876, was written for the avowed purpose of showing "that Prof. Hæckel's theory is essentially that with which both English and German students of Mr. Herbert Spencer's works have long been familiar; and that it does not furnish a clearer explanation than does Mr. Darwin's Pangenesis, of the special facts of Heredity which Mr. Darwin had in view." In the course of this article he is rather severe on my essay in the "Proceedings" of this Association, Hartford, 1874, with which, though, he is "only acquainted through Prof. Haeckel's citations." To show the laxity with which he permits himself to write, I shall quote this sentence: "I am unable to say whether Mr. Darwin was acquainted with or had considered Mr. Herbert Spencer's Hypothesis of physiological units, when in 1868 he published his own provisional hypothesis of Pangenesis." Turning to this publication[2] we read there: "Nearly similar views have been propounded, as I find, by other authors, more especially by Mr. Herbert Spencer;" then in a long foot-note several authors are named as well as Mr. Herbert Spencer, and Mr. Darwin makes reference to the very same volume, chapter and page as Mr. E. Ray Lankester.

As to my hypothesis of regeneration, — not only has Mr. Lankester not read the essay in the "Proceedings," but he either has not read or else has forgotten my article in the "London Monthly Microscopical Journal;" nevertheless he unhesitatingly passes judgment, thus: "Elsberg does not appear to have helped on the discussion of the subject to a great extent, since he proceeds no further than is implied in adopting Mr. Darwin's theory of Pangenesis, whilst substituting the "plastidules" for Mr. Darwin's "gemmules." How much or how little I have adopted "Mr. Darwin's theory of Pangenesis, whilst substituting the plastidules for Mr. Darwin's gemmules," I have already shown.

According to Mr. Lankester, what I have done is to combine Mr. Herbert Spencer's with Mr. Darwin's hypothesis; but he himself suggested doing this, he tells us, in an essay published six years ago. I willingly accord to Mr. Lankester the priority of making this suggestion. As to the facts, I have always, with Mr.

[2] The Variation of Animals and Plants Under Domestication. By Charles Darwin, M. A., F. R. S., etc. Authorized edition, New York, 1868, vol. ii, p. 449.

Darwin himself, looked upon his views as being very similar to those of Mr. Herbert Spencer; there is, however, this difference that the latter author inclines more to the "force" aspect and the former to the "matter" aspect of the case. Now, as my hypothesis includes both aspects, it certainly does *in so far* combine these two hypotheses; but as it differs from each of them not only in the particulars in which they differ, but also on points on which they agree, it is not a combination of them but something distinct. *In the first place*, plastidules are different from Mr. Herbert Spencer's Physiological Units, because: 1. Physiological units like gemmules are believed to be "thrown off" from each separate part of the whole body. Whether they are like gemmules supposed to be thrown off during all the stages of development is not quite clear to me, but interests me simply as a question of agreement or disagreement between Messrs. Spencer and Darwin; 2. Physiological units like gemmules are supposed to be diffused through the body, and to be merely aggregated in the sexual organs, these being "essentially nothing more than vehicles in which are contained small groups of physiological units in a fit state for obeying their proclivity towards the structural arrangement of the species they belong to;" 3. Physiological units like gemmules are supposed to grow and multiply.

Not only, therefore, is the conception of plastidules different from Mr. Herbert Spencer's conception of physiological units, but, *in the second place*, the assumption: 1. Of the origination of living matter at different times of the earth's existence; 2. Of the more or less approximate parallelism of development of different organisms thus started at different times; and 3. Of the dependence of rank among existing forms in greatest part upon this difference of duration in organic existence, has not been made, so far as I am aware, by Mr. Herbert Spencer any more than by Mr. Darwin.

So much for the fact of the combination. As to the manner in which, in Mr. Lankester's opinion, I have acquitted myself in making the alleged combination, he says: "It appears to me that Elsberg, in his combination of the Spencerian and Darwinian hypotheses, has omitted the sound element in the latter and retained the more questionable." Having thus again passed judgment on me, he vouchsafes to tell how I *should* have made the combination, viz.: "He [referring to me] should have conjoined Mr. Herbert Spencer's conception of plastidules possessing special polarities or force affections which they are capable of propagating as changes of state (*i. e.* force-waves) to associated plastidules and so to offspring, with Mr. Darwin's conception of a universal and continuous emission of such changes from all the cells of an organism, and the frequent occurrence of a persistently latent condition of those changes."

Mr. E. Ray Lankester has thus abundantly proved how little he knows of my views; but not yet content, he adds: "This is, in fact, the position which Prof. Hæckel takes up, though independently of what Mr. Herbert Spencer has written on the subject, excepting so far as the influence of the latter is to be traced in Elsberg's essay."

Before Mr. Darwin, Prof. Hæckel, I, or anybody else that I know of, approached the solution of the problem, Mr. Herbert Spencer had, for the purpose of explaining heredity, variation and other vital phenomena—so far as an explanation could be looked for—elaborated the hypothesis of physiological units, "which possess the property of arranging themselves into the special structures of the organisms to which they belong." He assumed that "each organism is built up of certain of these highly plastic units peculiar to its species—units which slowly work towards an equilibrium of their complex polarities, in producing an aggregate of the specific structure, and which are at the same time slowly modifiable by the reactions of this aggregate."

This hypothesis, although vague and indefinite compared with the conception I attained a number of years later, may be looked upon as the seed from which all subsequent speculations sprang. Darwin's "modified and amplified" hypothesis of pangenesis was a step in the direction of greater clearness of conception and statement; but together with this greater clearness it made the explanation, necessarily perhaps, more complex. The advance in our notions of molecular physics simplified the solution of the problem for me by enabling me to refer vital phenomena more definitely to the molecules of living matter. Hæckel brought into prominence the propagation of force-waves. But to Herbert Spencer belongs the honor of having first conceived a consistent hypothesis for interpreting the phenomena of derivative life.

On a Variation in the Colors of Animals. By S. W. Garman, of Cambridge, Mass.

It is not necessary, in order to produce variety of effects in nature, that there be great diversity of causes. Given diversity of objects acted upon, or difference of circumstances attending the action and a single cause is capable of producing many and varied effects. The greatest complexity, in a natural phenomenon, is often due to extreme difficulty in separating the modifications produced by peculiarities of individual, or locality, from the effect of a very simple general cause. An effect confined to an individual or species of a particular locality, and not appearing in others, would rightly be supposed to be due to some peculiarity of that individual, or its circumstances, which might or might not be under the control of the will of the creature. But in the case of an effect felt in the most distant countries, by animals of the most widely different conditions and necessities, in climates quite as diverse as the animals themselves, affecting not alone those to which it proves beneficial but the entire fauna of their neighborhood, and not only in localities upon the land favoring its action but also in similar ones in the waters, in the case of an effect so general the conclusion is inevitable that it is due to a cause universal and constant in its action and entirely independent of the wills of the animals affected. To such a cause is ascribed the effect, the variation in the colors of animals known as the pallor or decrease in intensity or amount of darkness exhibited by the faunæ of plains, deserts, mountains, certain islands, or snowy regions when compared with those of dark soiled, grass, or forest-covered countries. A cause of this character is the only one we can conceive to be likely to accumulate by imperceptible accessions through long periods of time, a variation which, however valuable it may have ultimately become as a means of protection, must have been in its inception, and for a considerable duration, an exceedingly small, unappreciable and useless quantity. The proposition that this variation is caused by the bleaching action of reflected light, is supported by the fact that it exists only in the presence of such light, that in the colors of animals exposed to light the blanching is directly proportioned to the amount of reflection in their surroundings and varies directly as this amount in the different seasons of the year, and by the absence of any other adequate means of accounting for it. It is but a step from the acceptance of the belief in the possession by reflected light of a bleaching power exerted in case of particular mammals and birds to the generalization, yet the necessity of taking it is not forced upon us until our attention is directed to the faunæ of the waters, where the same whitening process goes on but under circumstances so radically different as to cut down the number of probable causes to a minimum. Though the fact of the bleaching has been repeatedly asserted, in different species, by eminent scientists, of whom Prof. J. A. Allen has gone so far as to enumerate "Sunlight intensified by reflection" as one of several causes whose combined action produce the effect, no one has yet felt disposed to credit it with its full extent; no one has asserted its existence in the waters, and no one has attempted by its means to account for difference of summer and winter colors in birds and mammals or of colors of dorsal and ventral surfaces.

From the nature of the agent with which we have to deal, the slowness of its action, and the difficulty of direct experiment, it becomes necessary to cite numerous instances in order to give even an approximate idea of the extent of its influence.

At the outset it is to be distinctly understood that the variation treated of is simply a whitening or blanching of color, such as is noticed in tracing a group from its representatives inhabiting localities where vegetation is most abundant to those of barren or desert places. In amount it varies from the unappreciable to an utter obliteration of all markings in a uniform white. It is only in its effect upon the more stationary pigment of the skin and its outgrowths that it is considered.

The changes of color due to excitement, irritation, acceleration or retardation of the flow of blood from various causes or to food, as will be seen at once, are beyond our limits. Upon the consideration of the appropriation of different tints or plans of marking by one species and another I do not purpose entering at present. When the laws of color are better understood we shall have foundation for theories of spots and stripes. The popular ideas of voluntary color change, on the part of certain reptiles and batrachians, are in the main exaggerations and misconceptions. The changes of the most able among them are no more numerous than those to which a man is subject under the influence of fear, shame, excitement, irritation, etc., and are not yet proved to be any more under the control of will in the one instance than in the other. Those in which these sudden changes are very marked are few and of the more naked; in other animals the changes are hidden by the coverings of the skin, and do not become liable to be confounded with the primal or permanent colors. Even though certain changes might, by producing similarity of appearance with soil or surroundings, be beneficial on some occasions, the fact is that the reptiles most able to vary, when startled or frightened go through the changes regardless of the color on which they may happen to rest.

Notwithstanding they have been unable to retain the same appearance for any length of time the chamæleons kept for study by different naturalists have signally failed to take on the colors of chairs, carpets or other articles on which from time to time they found themselves. As for the very brilliant colors of some reptiles it is difficult to imagine surroundings with which they will coincide, and we are obliged to suppose such as the bright scarlet of species of Calotes for some other purpose,—possibly we shall yet be favored with the suggestion that this color has been selected because, like those of the flowers, it is more attractive to the insects on which its wearer feeds. A fruitful source of error in respect to changes of color lies in the great differences of appearance that may exist where there has been no change whatever in the object itself. Translucent animals passing over light and dark objects successively can show the colors passed without the slightest increase or decrease in their own. Thus young fishes moving from bright sand to black mud appear to change instantaneously, but viewed horizontally no difference is visible in the entire course. Like the animals, pieces of wood or rocks that are dark in the shade apparently lose much of their darkness when suddenly placed in the bright sunshine.

A horse that is light brown at midday presents a darker appearance on the approach of night, and at a later hour is not to be distinguished from the black at his side. If all the light falling on an object is absorbed the aspect is black; increase in the amount of light seems to make no change in the index of absorption, but whatever is in excess is thrown off with the effect of lightening the appearance; further increase produces only greater excess and lightness. A large majority of the supposed changes are explained in this way.

Most animals exposed to light possess color. The fact of the existence of certain species in the Mammoth Cave possessing it in complete darkness is an argument in favor of their derivation from progenitors not so deprived. Although such species as have never been exposed may be colorless it is not to be said that some now colorless never were colored, since a long continuance under adverse conditions may have caused the loss of the evidence. Naturally such colors as are most permanent will be most influenced by the variation and the transitory accessions from their very nature will present little opportunity for the action of an agent which makes its approaches so gradually. The brilliant tints of crimson, orange, purple, etc., which are to be directly correlated with the amount of heat, do not escape modification. Lustre being simply a mechanical or structural effect is not to be treated as color. The bleaching power is exerted by the confusing blending prismatic reflections of sands and snows rather than the simple refraction of mirror-like surfaces. The term reflection is used here only in connection with the former. In terms of effect and cause the law may be stated as follows: *Among animals possessed of darker colors the tendency to vary toward white is directly as the amount of reflection in their surroundings.* The instances cited in support and for illustration are few of the many that offer and are principally taken from the vertebrata because they are better known.

We are accustomed to think only of the great in connection with this matter, yet there is no reason why the most satisfactory experiments in respect to coloration and influence of surroundings may not be made with an animal whose entire world is darkened by the shadow of a wandering bird.

Migratory animals strive to place themselves in the midst of circumstances similar to those they leave, which is not conducive to variation; on this account as well as the difficulty in following them from place to place they are less available for observations than the more stationary, which are usually subjected to diverse influences in the different seasons of the year and can be traced with much greater ease. Evidence of the bleaching effect of reflected light exists in the prevalence of the paler colors in localities favoring it and the darker in the others, as is shown by the whites on the snows, the grays on the sandy plains and deserts, and the browns and blacks in the forests.

It may be seen by comparing the faunæ of different countries;

or by comparison of the families of an order, the genera of a family, the species of a genus, or the individuals of a species when subject to different degrees of exposure.

The body of the same specimen in the different seasons, or if the abdominal portions, generally more exposed to its action, are compared with flanks and back, often shows most emphatically the difference in effect between reflected and unreflected light. In the cases of animals living where absorption is most complete and reflection least, or in those of exclusively nocturnal habits, the dark colors prevail on belly and back, as may be seen on the inhabitants of dense forests or soils of dark colors. Proceeding from such a locality regardless of direction in latitude or altitude the colors are found to be constantly growing lighter in proportion to the increase in amount of reflection until are reached the light colors of the plains of North America or Central Asia, the lighter ones of Northern Africa or the white of the snows of the mountains or the poles. And it is particularly noticeable that in most instances the bleaching is least on the back, more on the sides, and most on the abdomen, provided the latter is not applied to the ground so as to prevent the effect of reflection. Many animals which accommodate themselves to changed circumstances in winter do not bring about the change of color by complete changes of plumage or pelage, but as the season advances the blanching effect, possibly aided by the cold, slowly makes its appearance and the whitened surface is gradually extended up the flanks until finally the back is included. If the whiteness of the belly in so many animals is due to bleaching then certain others which because of structure, habits or locality are not exposed to it should retain the dark color below as well as on the back; that this is so is shown by many reptiles, by the moles in the earth, the bison in the grasses, and the moose and bear in the thickets and forests.

INSECTS.

For hints that have been of material aid to me in the accumulation of a mass of facts among the insects in support of the idea here advanced as to the cause of the variation I am indebted to Mr. Geo. Dimmock, a close and accurate observer. The evidence is quite as conclusive and more abundant in this class than among the vertebrates. Wherever the latter suffer a diminution of the amount of darkness in coloration a corresponding decrease is to be noticed in that of the insects which is undoubtedly due to the same cause. An entomologist in looking over his cabinet is able to say "from the intensity or fadedness of the colors" about what sort of a locality furnished such and such specimens; in other words, he can with considerable accuracy indicate the character of the habitat from the condition of the markings of the specimen. The insects of the sandy plains, tracts of least amount of vegetation and great reflection, are more pale than representatives of the same species and genera from the neighboring woodlands.

The Cicindelidæ being constantly in the same horizontal position and frequenting the same localities from day to day show very distinctly the bleaching effect. A comparison of specimens enables one in a short time to point out with some degree of certainty by the ashiness or paleness alone the species of the sands as distinguished from those of the bushy or grass-grown loams. On a more critical examination individuals living upon light colored banks can be separated from others of the same species inhabiting the black soils.

The ashy light colored inhabitants of the light colored soils are further distinguished from those on the dark, in the grasses and bushes, by a whitening of the color around the margin on the under surface that extends a greater or less distance from the edges of the elytra toward the centre of the abdomen, which from being applied to the ground much of the time is less bleached. The Lepidoptera of the plains, as is well known, are of lighter colors than those of the woods and meadows. A decrease in the amount of dark color in the species of the mountains from the bases toward the summits has led to a proportioning of intensity of colors according to altitudes, an arrangement which must often be reversed when proceeding from wooded mountain sides or plateaus to the plains below them.

AQUATIC ANIMALS.

In the waters the variations are similar to those on the land in some respects and quite different in others. In clear waters, in shallow unshaded streams and on bright sandy beds the fauna is lighter colored than in dark waters, in deeply shaded streams and over muddy bottoms. Here the variation is the same as obtains on the land in situations similar as regards light and reflection.

That this is so would seem to indicate a similar cause, and also that that cause cannot be difference in amount of moisture or dryness as has been suggested with good show of reason in case of land animals. Many of the lower marine forms while confined to the rock or mud of the bottom are more or less dark colored, but during their free swimming stages when exposed to the influence of reflection from a broken surface or the bottom they become much lighter. A few of the least erratic of the American Selachia will serve to illustrate some of the peculiarities of variation in the depths. *Raja lævis*, *R. ocellata*, *R. erinacea*, and *R. radiata*, var. *americana*, occurring from Cape Cod to Grand Menan and northward are very dark, uniform or indistinctly spotted, and agree well in depth of color. Specimens of *Raja ocellata* and *R. erinacea* from south of the Cape are much lighter with spots more distinct, and the color gradually becomes lighter and the spots decrease in size to the southern limits of the ranges of the species. *Raja eglanteria* with a range from Buzzard's Bay to the Tortugas and probably southward is dark olivaceous with spots of considerable size more or less confluent into bands at the north and changes southward until of a light brownish yellow or reddish with small dots of darker. *Uraptera Agassizii* from southern Brazil and the Rajæ from Peru agree in depth of color with those from south of Cape Cod. *Uraptera binoculata* from San Francisco and the genera Sympterygia and Psammobatis from southern South America agree with the Rajæ from Massachusetts Bay and northward. The species of Torpedo from Massachusetts Bay and that from San Francisco agree in the possession of a dark chocolate brown color.

Narcine braziliensis from Rio Janeiro with a dark plumbeous or olive ground and dark brown bands and spots is represented in the Antilles by a variety with a yellowish to reddish ground and bands identical in shape and position but from which the centres have faded until nearly as light as the ground, yet the young from the two localities are so nearly alike that it is with difficulty they are distinguished. *Urolophus torpedinus* from the Caribbean Sea differs from the closely allied species *U. Halleri* from San Diego, Cal., in having a lighter ground and being covered with larger spots of white so that the aspect is grayish; *Halleri* has an olive ground, very small spots and a dark appearance. *Urolophus mundus* from Panama is much lighter colored than *U. chilensis*. *Rhinobatus Horkelii* from northeastern South America is lighter than the same species farther south; *R. undulatus* from Rio Grande do Sul to Rio Janeiro is quite dark, with bands of black, and grows lighter to the northward until hardly to be separated from the former. The species from Panama, *R. leucorhynchus* is light, that from Peru dark and that from California dark and banded as the southern species on the Atlantic side of the continent.

The ornamental tints of the fishes increase in number and brilliancy toward the warmer waters of the tropics, as the same class of colors does among the birds from the north or south toward the equator; but on passing to the northern or southern waters the less brilliant more modest and darker colors are found to prevail. On the land the amount of reflection increases toward the poles; in the waters the direction of increase is toward the equator. Over the equatorial waters the rays of light are most direct, they penetrate the water with more readiness and to greater depth, thus increasing the possibility of reflection from the bottom; to the northward or southward the rays are more oblique, owing to the roundness of the earth and the position of the sun, the likelihood that they will be refracted off into space has increased, the probability of penetration to the bottom lessened, and in consequence there is a decrease in the amount of reflection; in these directions there is an increase in the darkness of coloration. The majority of fishes are lighter colored on the surface most exposed to the action of reflected rays.

BATRACHIANS.

While there is a gradual lightening of colors in this group as we approach the plains from the wooded tracts, as may be seen in the frogs and toads, there is also a tendency toward increase of dark color from the tropics like that observed in the fishes.

Scaphiopus, Rana and Bufo present marked illustrations of this. *Scaphiopus solitarius* from the northern limits of its range in New England is almost uniform brown, the white lines so distinct in specimens from South Carolina being indistinct or absent. A variety of this species (*S. albus*) discovered on the island Key West, Fla., by Count Pourtalès, would be characterized as white with irregular bands of brown. The South Carolina specimens are intermediate between the extremes from the north and south.

Rana silvatica from the south is light; at the north it becomes so dark that the brown patches on the sides of the head are indistinct. *Rana clamata* from being light colored with a reddish tint at the south loses the red and becomes quite dark as we go north. *Rana pipiens* Gmelin has an increase in number and size of the black spots, and *Bufo lentiginosus* from gray becomes brown along the same route. The European frogs and toads serve quite as well for examples as the North American. Salamandra and Amblystoma may be cited as showing the greater prevalence of light markings at the southward in the lower Batrachia. Having naked skins the transitory color-changes of this group are more liable to be confused with the permanent; this, with the great differences of the markings in individuals, has been the cause of an unlimited amount of exaggeration as to ability to change at will.

REPTILES.

The land reptiles accord better with the mammals. Those from the sandy plains and plateaus are lighter colored than those living in the midst of vegetation. From southern South America there is a marked increase in the amount of darkness toward the forests of the Amazon. From the plains of California, Nevada, Utah and Colorado there is an increase in darkness in the forest regions of the Pacific coast at the northward and also southward, in Mexico, Texas and the southeastern states, as is well shown by the species of the genera Heloderma, Phrynosoma, Crotalus, Heterodon, Tropidonotus, etc. From Algeria there is a very decided increase northward in Europe. In these cases and the numerous others equally important that may be cited the increase in the pallor or lightness is always in the direction of greater reflection. In such serpents as are exposed to reflected light we perceive not only a lightening of the color of the whole body as compared with that of allied individuals or species not so exposed, but there is a marked difference in the colors of the same body subjected to different degrees of exposure. That portion of the belly habitually pressed against the ground is darker in many species than the anterior portion which is raised and becomes bleached. Take for instance the form of Heterodon from the plains, described as *H. nasicus*, which is quite light colored, and by choosing forms to the southeastward a regular gradation of color may be built up to the very dark form to which the name *H. niger* was given. In *H. nasicus* the abdomen is darker than the back, but under the portion carried off the earth it is quite light and gradually shades into the brown farther back.

The common black snake or racer (*Coryphodon constrictor*) of the Eastern States is also a good illustration. On the prairies and plains of the West its representatives are much lighter colored. Under the throat, and for some distance back on the abdomen, the colors are light, slowly darkening to the portion kept on the ground. This effect may be seen in Bothrops, Crotalus, Tropidonotus, Ischnognathus, Oxyrhopus, and is quite general among the Colubrine and Coronelline forms.

BIRDS.

By different authors attention has been repeatedly called to the extent of the bleaching effect in this group. The majority of birds are lighter on the lower side. Of the less migratory the lighter colored are found on the sandy soils of plains and deserts, in the regions of most reflection, and many are accommodated to changed surroundings in winter by a change of color.

The change of such as turn white in winter does not appear to be accomplished by change of plumage. The moult begins early in the summer and continues a greater or less length of time according to the species or age of the bird. The new feathers are brown or gray as the case may be, and in the fall when the frosts and snows come they bleach until sooner or later in such birds as the Ptarmigan they become white. In manner the change is the same as that of certain mammals. The change is probably aided in both cases by the effect of the cold on the circulation and vitality in the extremities of the feathers or hair. Erratic birds subject themselves to diverse conditions in such rapid succession that it is only on a very intimate acquaintance with their habits that one can determine the influences to which modifications may be due. A bird like the crow is not to be considered an exception because in some localities he frequents the beach or is seen far out on the open grounds. An inhabitant of the depths of the forest, his retreats are in the most secluded places. His flight is usually at a distance from the earth, and in reality he is one of the least likely to feel the effect of reflected rays.

Records of the amount of variation in particular species are to be found in the works of Mr. Allen, Prof. Baird, Dr. Coues, Mr. Ridgway and others.

MAMMALS.

Very few of the more brilliant colors are exhibited in this group. Departures from black or white or their mixtures are comparatively few and when occurring are most often in the shape of a tinge of red or yellow on the brown or gray. The instances given below illustrate to some extent what is peculiar to certain localities and the directions of increase or decrease in the darkness of coloration.

Southward from the home of the white polar bear, in America there is a brown bear on the barren grounds, a cinnamon on the Rocky Mountains, a grizzly on the Sierras of California, and a black in the forests of the eastern and southern United States.

In the old world, south from the polar regions, there is a brown bear (*Ursus arctos*) and in the forests of India another black (*U. torquatos*). More or less nocturnal in habits the bears experience little of a bleaching effect on the ventral surface, and hibernating in winter they reappear in spring as dark or darker than in the fall.

According to Sir John Richardson, "Wolves totally white are not uncommon in the most northern parts of America, particularly in districts nearly destitute of wood." In the foot-note following the sentence quoted he says: "Muller informs us that white wolves are found on the Jenisei, and Regnard says that the Lapland wolves are almost all of a whitish-gray color; there are some of them white."

Speaking of "The Black American wolf, *Lupus ater*," he says, quoting from Griffith, Warden and Desmarest, "Black Wolves are "more frequent in the southern parts of Europe than in the north- "ern; and to the south of the Pyrenees they are said to be more "common than the ordinary species or variety. In like manner "the American Wolf is more common on the Missouri than farther "north; and it is reported to be plentiful in Florida, where, "according to Bartram, the females are distinguished by a white "spot on the breast."

The Coyote (*Canis latrans*) of the plains, has an average of gray similar to that of the Jackal (*Vulpes aureus*) from Algeria.

The Arctic Fox (*Vulpes lagopus*) is brown in summer and bleaches white in winter; the young are brown or plumbeous; early in winter they become grayish, later pure white. The hairs are not whitened to the base, which remains of a brownish. In the spring and summer the white fur is replaced by a new coat of brown. To the southward the foxes are darker and there is quite as much confusion amongst them as the wolves, black, red and gray appearing in the same locality. The wolves and foxes are usually lighter colored on the belly, but, being more or less nocturnal and burrowing, in many cases the difference is slight.

The Felidæ follow the general rule; in the mountains and snowy regions are found the light colored species and in the forests the darker. Animals like the badgers (Taxidea and Meles), the Porcupines (Erythizon and Hystrix), the Woodchucks (Arctomys), or the Moles (Scalops and Condylura), as their build and habits would suggest retain much of the dark color on the abdomen.

Such as the Beaver and Muskrat (Castor and Fiber) are also nearly unicolor, and withdrawing from the influences of winter are as dark or darker in that season. The Otter, Fisher and Sable remind one of some ophidians in the light color extending from the chin down the breast to the horizontal portion of the body. The Weasels are reddish brown in summer; in winter the white of the belly is extended up the flanks until finally the brown of the back has disappeared. The Rats, Squirrels and Field Mice are usually lighter beneath and in some species lighter winter colors obtain. The average color of the north African mammals is lighter than that of those of the plains of North America. Those specimens of Ctenodactylus I have seen are of a tarnished white all over the body, but on blowing the hairs apart a darker color appears, which indicates that it has become bleached from the dark of another season or of its young stages, an effect which is produced on the arctic rabbits and foxes under climatic conditions as nearly opposite as can be imagined.

Species of Gerbillus, Mus, Fennecus, Dipus, Herpestes, Vulpes, Gazella, Giraffa and Camelus fairly represent the character of the coloration of the animals of this country. On comparison with a like number of the common European animals it will be seen that it is comparatively light and generally presents a faded or bleached appearance.

A contrast is presented in the darker colors of the Stag and Roebuck, or even the lighter to grayish of the Chamois and Ibex, which respectively represent regions of less and greater amounts of reflection in Europe. We may look to the Weasels and Rabbits for examples of the more changeable, and to the tawny brown of the Wild Boar in the dense thickets for one of the more permanent.

On the plains of Siberia the colors have the pallor of the corresponding American territory, and are much whiter than those of India and Europe. Here, as elsewhere, the variation affects the entire fauna. Though the conditions as respects reflection resemble to some extent those of Algeria the climates have little in common.

According to Sir George Simpson a most remarkable instance of variation is presented in the inhabitants of one of the Steppes. On page 199, of the second part of "An Overland Journey Round the World," published in 1847, in describing his route from Irkutsk to Tobolsk this author makes the statement given below. "On the ensuing day we entered Baralinsky steppe, a "flat and fertile prairie of vast extent. Among the many agri- "cultural settlements, that studded this boundless plain, the one "which most particularly attracted my attention, was a colony of "Jews absolutely turned farmers—a phenomenon the more extra- "ordinary in a country where every one else was agog in pursuit "of gold and silver. But the alteration of complexion was, per- "haps, more remarkable than that of disposition. Though these "tillers of the ground still retained their hereditary features, yet, "in spite of the usual influence of rural labor, they had exchanged "the swarthy countenance and dark locks of their race for fair "skins and light hair, etc." People of this race living in north eastern Europe are of lighter complexion generally. Instances of reversion from absence of the cause occur in Malabar and Cochin China where the recently arrived are designated as the white and those of long residence in the country as the black Jews. The direct effect is exemplified by the light colored races of the elevated regions of India, Abyssinia, or the interior of Africa which present most marked contrasts with the dark ones by which they are surrounded.

In South America the Aucheniæ (Vicuña and Guanaco), the Agoutis, Viscachas, and smaller rodents of the plateaus of the Andes have average coloration similar to that of the plains of North America, and show the whitening effect on the abdomen.

Of the North American mammals the Moose, Bison, Musk Ox and Peccaries, inhabiting the forests, thickets and unreflecting dark-soiled grass-producing valleys, wear the darker colors and do not become whiter on the belly. The principal character by which the Mountain Bison, inhabiting the valleys and groves among the foothills of the ranges is distinguished from the Bison of the plains is said to be a somewhat darker color. The Caribous (Rangifer) of the woods are darker than those of the mountains or the far north; the latter are more white beneath and have whiter colors for winter.

Cervus macrotis of Colorado and Wyoming is hardly as dark as *C. virginianus* or *C. mexicanus;* they are light below and vary to a lighter gray in winter. The Rocky Mountain Sheep and the Prong-horn Antelope of the plains are very like in colors; both are light brownish gray, lighter below, and are not so dark in winter.

The Rocky Mountain Goat (*Aplocerus montanus*) from the summits is uniform whitish. The Polar Hare is white in winter; adults in summer and the young are brown or plumbeous; the fur bleaches to the roots. Southward the rabbits become a more or less perfect white in winter. Still farther south on the prairies of Illinois, Wisconsin, Minnesota, and Iowa they become light gray, and, as Pennant expresses it, "From New England southward they retain their brown color the whole year."

Sometimes it is the case that at the time of losing the hair various mammals appear darker below from the appearance of the new and colored hair on that portion of the body before the loss of the old coat, by which it is hidden, on the flanks and back.

These instances will be sufficient to show the extent of the variation. Its tendency is to bring the colors of the animals to agree with those of its surroundings; for this reason it has been classed as protective coloration, notwithstanding the fact of its occurrence on all the species of a locality whether in need of protection or not. Since the effect is not produced suddenly it is natural that exceptions should exist.

The very disagreement of color, with that required by the locality, in a species which has migrated at a recent date may often render important service in tracing its wanderings. While in case of the removal of a species of Lepus the change of color might be brought about in a short time, for a species of another genus less susceptible a long period would be necessary, and at any time previous to agreement with the demands of the locality it might be recognized as a species out of place. An excess of darkness or of lightness in the coloration would immediately suggest a region of less or of greater reflection as the former home. This law of conformability is accepted in our practice. There is no question as to the forests of the tropics being the home of the negro; on the plains, deserts or in the polar regions he would at once be considered an exile. Whatever may have been the original color of mankind, though we have little or no reason for supposing it was other than black, no one thinks of looking elsewhere than to the mountains and plateaus, regions of abundance of reflected light for the origin of the white race; and the colors of men are not usually thought of as having been selected by them for purposes of protection.

The effects of reflected light have not been unnoticed by naturalists. In 1747 Drage, the Clerk of the California, noted it in the northern Hare. Sir John Richardson remarks of the same animal in 1829: "After a careful examination, however, of many speci-"mens in different states, I agree with the Clerk of the California "in thinking that the change to the winter dress takes place by a "lengthening and blanching of the summer fur; whilst the change "in the beginning of summer consists in the winter coat falling "off during the growth of the new and colored fur."

Previous to 1870 Professor Baird had several times called attention to the paler colors of the birds of the arid regions of the plains and lower California.

In 1872 Mr. Allen, in a paper read before the Boston Society of Natural History, entitled "Geographical Variation in North American Birds," speaks of it more directly. He says, to use his own words, "Humidity alone, or in conjunction with greater "intensity of light, seems equally well to account for the increase "of color to the southward, yet, from the well known bleaching "effect of sunlight, intensified by reflection, upon the colors of "animals living upon sandy islands, and sea beaches, and desert "interior regions, it seems doubtful whether the large share of "modification in intensity of color in birds may not be due to "humidity alone, or to humidity and a high temperature together, "rather than to intensity of light." Again, in 1874, at a February meeting of the same Society, in a paper "On Geographical Variation in Color among North American Squirrels," he speaks of what he terms "the laws of (1) the enlargement of peripheral parts to "the southward; (2) of the increase in intensity and extent of "dark colors at the southward; and (3) increase of color with "increase of humidity or the correlation of intensity of color with "the mean annual rainfall." And farther along in the same paper he says, "In respect to the correlation of intensity of color with "the degree of humidity, it would perhaps be more in accordance "with cause and effect to express this law of correlation as a "*decrease* of intensity of color with a *decrease* of *humidity*, the "paleness evidently resulting from exposure and the blanching "effect of intense sunlight, and a dry, often intensely heated "atmosphere." It is scarcely necessary to remark that these laws as far as they have to do with the variation in color are not of general application.

Although appearing to account admirably for its presence on the land, in the waters degrees of humidity and dryness are out of the question, yet there as on the land in localities similarly conditioned as regards reflection we see the same lack of darkness in coloration.

In northern Africa the blanching effect is accompanied by intense heat, but in the United States on the plains the season in which the variation is greatest is the coldest, and in the water as on the land the bleaching goes on regardless of temperature. There is sufficient evidence that humidity and dryness and heat are not causes in that the variation is similar on the hot dry sands and the cold damp snows. Moisture seems to be only an indirect agent in that it promotes the growth of vegetation which lessens the possibility of reflection.

Professor Cope, also, adds the weight of his testimony in respect to the existence of this variation among the reptiles, and as he credits it to a different cause I may be pardoned for quoting his words. On page ninety-four of the first Bulletin of the U. S. National Museum occurs the following: "Another character of the "reptilian life of arid regions is to be seen in a peculiarity of color-"ation. This, which has been already observed by the ornitholo-"gists, consists of a pallor, or arenaceous hue of the body, nearly "corresponding with the tints of dry or sandy earth. This prevails "throughout the Batrachia and Reptilia of the Sonoran region, "although it is often relieved by markings of brilliant color, of "which red is much the most usual. This peculiarity doubtless "results immediately from the power of metachrosis, or color "change, possessed by all cold blooded vertebrata by means of "which they readily assume the color of the body on which they "rest. That a prevalent color of such bodies should lead to a "habit of preference for that color is necessary, and as such habits "become automatic, the permanence of the color is naturally es-"tablished." If the Reptiles and Batrachians possessed this power, which we cannot admit to have been established in the case of a single species, it would still be to account for the same variation among fishes, birds, mammals, insects and the balance of the animal kingdom. There is no appearance of urgent necessity demanding that the lower animals should be less at the mercy of their surroundings than the higher, and there is no reason for crediting them with power to achieve for themselves results which in the higher are accomplished by force of circumstances. It is obvious that the power of color change is not possessed by the bird, yet it experiences the same variation in the same locality, has equal necessity and is considered to be descended from reptilian ancestors. The same result obtained in the same locality, under the same conditions by animals having so much in common would lead us to ascribe it to the same cause in each instance.

The effect is a general one and not to be accounted for by different special causes or adaptations in particular groups. It consists in a blanching of the coloration of the fauna of one locality as compared with that of another; also a blanching of the individuals of some species at a certain season of the year, and of one part of the body when contrasted with another on the same individual.

Land animals or those of the water in localities where there is no reflected light are not affected by the variation; the amount of the variation increases directly as the amount of reflection; it is greatest in the locality of greatest reflection; in any locality is greater in that part of the year in which there is most reflection, and on the body is greatest on the part most exposed to the influence of reflected rays. The phenomena to be accounted for are a decrease in depth of color from the forests and dark soils to the sandy plains and snows, a general decrease of darkness of color from the equator to the poles, and lighter colors in winter in regions affected by snow, lighter colors over the lighter colored bottoms and a general decrease in darkness of color toward the equator in the waters, and a decrease in depth of coloration on the ventral surfaces in the majority of species on land and in water. They are accounted for by the blanching effect of reflected light.

In this hasty manner I have endeavored to suggest what appears to me as the action of a physical agent, the operation of one of the laws of circumstances or environment which go so far toward adapting the animal to its habitat and which are ever selecting, augmenting and perpetuating from the many variations so frequently occurring the few which for some reason or other are fitted for survival. Though a law of this character, or that of the correlation of the density of pelage and plumage to the amount of heat radiation from the body, would direct the course and limit the amount of selection yet it could be in no way inimical to orthodox evolution. Those naturalists who are averse to centralization of power in the hands of a being of whose will all of nature's laws are merely the expressions, find opposition to law one of the necessities to their existence as such which, it can hardly be doubted, will be lost to them as science acquires a better acquaintance with nature's self. As long as we must admit that the forces which at a particular time and place called into existence and built up "*a branching series*" were equally competent and likely under favoring circumstances to start and develop a number of series in various places at the same or different times, and that all which has been thus far adduced in favor of evolution supports a belief in various centers quite as well as that of a common point of divergence, a positive assertion that all classes of animals are descendants of a common ancestor is unwarranted. But, whether the different animals now in existence have descended from a common ancestor, having left sooner or later as the case may have been the trunk or some branch of the genealogical tree, or each kind has retained its individuality through all the stages of its existence and development from its very beginning, it is certainly the case that we are able to account for the phenomena of variation in animated nature more satisfactorily and in greater number by means of the action singly or combined of the physical laws which govern the universe than by Selection, itself subject to their control.

On the History of the Crystalline Stratified Rocks. By T. Sterry Hunt, of Boston, Mass.

ABSTRACT.

The speaker referred to the views as to the origin of the crystalline stratified rocks of North America which he had advanced in his address to the Association in 1871, and then proceeded to refer to the history of opinion on this subject in Europe and America. Those whose memory goes back a quarter of a century will remember that the crystalline formations were included under the two categories of erupted masses and crystalline schists. To the former were referred a great part of the gneisses, the hyperite or norite rocks, the serpentines, and all the feldspathic greenstones, including diorites, gabbros and diabases, which occur in association with these; while the various micaceous, chloritic and talcose schists were regarded as the result of the action of these eruptive rocks upon ordinary uncrystalline sedimentary strata; the reaction of which upon the igneous rocks had, however, at the same time profoundly altered the latter. By this double hypothesis of a metamorphism alike of the eruptive and the sedimentary rocks, it was attempted to explain the great diversities offered by the crystalline formations. This notion required the presence of great quantities of eruptive rocks, and accordingly all the more massive of the crystalline formations were included in this category, while the schistose crystalline rocks were regarded as contact-formations; the appearance of stratification among these massive rocks was conceived to be due to other causes than aqueous deposition. Such views are not yet obsolete, but are, on the contrary, held to a greater or less extent on both sides of the Atlantic, and still color the geological descriptions of the most eminent authorities; although an intelligent study of the relations of the truly eruptive rocks, which are so often found included in uncrystalline sedimentary deposits, affords sufficient evidence to refute this ancient theory.

He then proceeded to show how the Canadian geological survey, as early as 1847, distinguished two stratified, but discordant divisions in the crystalline formations of Lake Superior, Lake Huron, and the Ottawa, and maintained the aqueous origin and stratified character of the so-called granites, syenites, and serpentines of these two series, to which, in 1854 and 1855, were given the now universally recognized names of Laurentian and Huronian. That the

rocks of the second series occur in the Appalachians was early apparent, and was noted soon after by Rogers, in his Geology of Pennsylvania. Inasmuch, however, as the study of the strata in northern New York showed nothing, at the out-crop, between the basal beds of the uncrystalline formations and the old granitic gneisses, there was a general disposition on the part of American geologists to maintain that the whole of the crystalline formations east of the Hudson, which were clearly distinct from these gneisses, could be no other than the members of the New York paleozoic system under peculiar conditions, due in part to original deposition and in part to subsequent alterations. This view was subsequently adopted by Logan, who maintained that the crystalline rocks in eastern Canada are altered paleozoic. The fossiliferous strata, supposed to be the equivalents of these crystalline rocks, however, include in the vicinity of Quebec, fragments of the latter, which, according to the speaker, are of Huronian age.

Later, the geological survey of Canada studied the so-called hyperite, norite or labradorite rocks, which by Emmons, in northern New York, had been regarded as of igneous origin. They were, however, shown to constitute a distinct stratified series, resting unconformably upon the Laurentian, and provisionally named Upper Laurentian. It is not as yet certain what relation they sustain to the Huronian; that is, whether they are older or younger than it, and they have since been designated the Norian series. The rocks of this series are found over considerable areas in Canada and New York.

Still another series, very distinct both geologically and geographically from those preceding, is that of the gneisses and mica-schists of the White Mountains, of Manhattan Island, of Philadelphia and of Washington, to which the name of Montalban has been given by the speaker, and which, in the present state of our knowledge, appear to be of less antiquity than the Huronian. Besides these must be mentioned the granular quartzites, the slates and the limestones which, together, make up the chief part of the original Taconic of Emmons, and constitute still another well-defined division of the crystalline rocks. It is not at all certain that these five divisions include the whole of the crystalline stratified rocks, but in the speaker's opinion all of these may be provisionally included in them. These crystalline series form the region of the Laurentides and the Adirondacks, and also, for the greater part, the Atlantic belt. This, however, includes in New Jersey and in southwest Virginia, certain areas of uncrystalline lower paleozoic strata separated from the Allegheny belt by the great Appalachian valley, and holding in their conglomerates pebbles derived from the adjacent crystalline formations.

These great divisions are not confined to our continent, but are finding recognition abroad, where, as I have long since pointed out, the Laurentian, Huronian, Montalban and Norian, all appear with their characteristics. Gastaldi, in a paper published during the present year, declares that the basal rock-formations, both of the Alps and the Apennines, consist of Laurentian gneisses, overlaid by a vast series which, in opposition to the views of most European geologists who had regarded them as altered strata of more recent date, he declares to be pre-paleozoic, and refers to the Huronian. The mineralogical and lithological characters of the crystalline rocks have, according to him, a constant and definite value, as marking geological horizons. These conclusions of the eminent Italian geologist are identical with those which the speaker ventured to publish in 1871 as the result of his own studies in Alpine geology. The Montalban series, though found in many parts of the Alps, is not recognized in the section recently published by Gastaldi, who finds overlying the Huronian a vast series of quartzites, with more or less crystalline schists, and magnesian limestones including gypsums. These are not without many resemblances to the Taconic schists and limestones, which occupy a similar position along the Appalachian valley, and probably correspond to the Hastings limestone series of Ontario, there unconformably overlaid by the Trenton formation; and perhaps to a similar limestone series near St. John, New Brunswick, which is, according to the local geologists, older than the Lower Cambrian Menevian beds of that region.

The question may be asked whether all these various crystalline series are to be called Eozoic. In the speaker's opinion it will be found as difficult to draw the line between eozoic and paleozoic as it is to define that between mesozoic and the paleozoic on the one hand and the cenozoic on the other. There are no hard and fast lines in nature; breaks are local, and there is nowhere an apparent hiatus in the geological succession which is not somewhere filled. The deposition of the materials of the crystalline rocks supposes conditions unlike those of our own time, but not incompatible with the existence of organic life, as is shown by the presence of Eozoon alike in the old Laurentian serpentinic limestones, in the newer crystalline schists of Bavaria, and in the Hastings limestone of Ontario, and also by the forms somewhat resembling Stromatopora lately discovered by Hawes in the Huronian greenstones of New Hampshire. Brachiopoda are now found in Europe in strata which are very far below the paleozoic base of our New York series, and Torell, the eminent Swedish geologist, who is with us on this occasion, has found beneath the Fucoidal sandstone, which in Sweden underlies the Paradoxides zone, a still lower division, the Eophyton sandstone, in which, besides a species of *Lingula*, are casts, apparently of a radiate animal, to which he has given the name of *Monocraterion*. Prof. Prime has found in the Auroral limestone of Pennsylvania, casts which, in the opinion of Dr. Torell, who has lately examined them, must be referred to an animal generically identical with *Monocraterion*. These same limestones in Pennsylvania, which belong to the lower Taconic series of Emmons, have also afforded an undescribed species of *Lingula*. We look for further revelations as to the life of the remote past, but meanwhile, it does not appear improbable that this seemingly imperishable type of *Brachiopods* may serve like the *Rhizopods*, represented by *Eozoön*, as a connecting link between eozoic and paleozoic time.

Geology of Eastern Pennsylvania. By T. Sterry Hunt, of Boston, Mass.

Prof. H. D. Rogers, in his geological survey of the state of Pennsylvania, pointed out the great difference between the gneisses of Philadelphia and those of what is known as the South Mountain. This chain of gneissic hills, which forms the Highlands of the Hudson and of New Jersey, crosses the Delaware near Easton, and extends as far as Reading on the Schuylkill. Along the north-west side of this chain stretches the Appalachian or Kittatinny valley, occupied in great part by the Primal and Auroral rocks of Rogers which, through the break in the South Mountain range between the Schuylkill and the Susquehanna, pass beneath the great belt of Mesozoic sandstone and gain the subordinate valleys on the Atlantic side of the South Mountain chain. The rocks of this last, as far as the Schuylkill, are clearly Laurentian, as are also those of the Welsh Mountain, which stretches between the Schuylkill and the Susquehanna along the southern border of the Mesozoic belt.

From the southern side of the Mesozoic sandstone at Norristown, to Philadelphia, the Schuylkill River, crossing nearly at right angles the stratification, affords on either bank a most instructive section, which has been described by Rogers as presenting in its northwestern portion the Primal slates and Auroral limestones, generally with a southeast dip; followed by a series of gneisses, including micaceous and talcose rocks, having a prevalent northwest dip. In studying this section last year I was so fortunate as to recognize near Conshohocken a belt of Laurentian gneiss identical with that of the South and Welsh Mountains, which, I think, serves to guide us to a better interpretation of the geology of the region. This Laurentian belt, which is not more than a mile in width on the river, separates the Auroral limestones of the northwest from the Philadelphia gneisses and mica-schists; which are typical rocks of the Montalban series. The Laurentian gneiss, much contorted, and in parts nearly vertical, is succeeded on the southeast side, after a little interval in which the rocks are concealed, by a belt having all the characters of the Huronian series, in which are included the large steatite quarries on both sides of the Schuylkill. These are associated with dark colored serpentines, chloritic schists, unctuous mica-schists holding garnets, feldspathic and hornblendic rocks, and argillites; the section presenting most of the typical rocks of the Huronian, in very highly inclined strata, which, from the data given by Rogers, seem to have a strike somewhat different from that of the Laurentian gneisses; an observation confirmed by my friend, Mr. Charles E. Hall, of the Pennsylvania geological survey, who has since examined the section. I have not been able to follow the succession to the southeast so as to find the limit between these Huronian strata and the Montalban rocks of Manayunk and Philadelphia; nor have I as yet been able to determine a point which would have much interest, namely, whether Huronian rocks appear to the northwest of the Laurentian belt, between this and the closely contiguous Auroral limestones. This section along the Schuylkill has the advantage of offering within a short distance, and in a very accessible place, characteristic exposures of three great series of our crystalline formations.

The intermediate position of the Huronian in this section seems to show that its stratigraphical position is below the Montalban, and it is to be noted that the dips of the great breadth of the Montalban strata on the one side, and of the Auroral limestone on the other, are towards what we may call the Laurentian axis. Similar dips are to be seen in the Auroral limestones along the northwest side of the South Mountain (also Laurentian), and it is worth inquiring how often such phenomena as these have perplexed geologists, and led them to assign to ancient crystalline rocks a less antiquity than belongs to them. It would seem as if the Laurentian gneiss had formed a barrier, by the resistance of which the unconformably overlying formations have been thrown into folds, and in many cases faulted, so that they are made to dip on either side towards the axis.

The Primal slates and sandstone of Rogers, which underlie the Auroral magnesian limestone, present many points of great interest. In some places, as at Chickis on the Susquehanna, and below Reading on the Schuylkill (where they are seen to rest on the Laurentian), they are apparently several thousand feet in thickness; while elsewhere, along the South Mountain, their only representative is a few feet of detrital sandstone interposed between the Auroral limestone and the gneiss. This overlapping shows that the two formations, if not actually unconformable, were deposited under very different physical conditions, and that a great subsidence of an ancient shore-line took place in the interval between the two.

The crystalline character often observed by these so-called Primal strata was noticed by Rogers, who ascribed it to their subsequent alteration by intrusive rocks. A careful study of this series has, however, convinced me that its detrital beds include, in many parts, deposits of chemical origin, such as beds of crystalline magnesian limestone, often holding serpentine, chloritic, steatitic and micaceous schists, and especially great beds of magnetic and more rarely specular or red hematite iron ores, of which the Cornwall, Boyertown and Dillsburg deposits are examples. The aspeet of these ores, and their associated rocks, is unlike that of the other crystalline series already mentioned, and I believe that these Primal slates constitute a newer and distinct formation of crystalline strata. The schistose strata intercalated among the Auroral limestones, partake of the mineral characters of the Primal slates, which, from their soft unctuous nature, were formerly called magnesian, though this quality is due, in great part, to the presence of a hydrous mica. These strata include deposits of carbonate of iron, and others of pyrites, from the alteration of the one and the other of which, in the deeply decayed portions of these strata (now converted into clays) have been formed the great quantities of hydrous iron ores which characterize, throughout the whole extent of their out-crops, the Primal and Auroral strata. These are the Lower Taconic rocks of Emmons.

To the south of the Susquehanna, the South Mountain, almost effaced between this river and the Schuylkill, reappears, and thence stretches southward to the Potomac, beyond which it takes the name of the Blue Ridge. In this portion, so far as I am aware, it shows no Laurentian north of the Potomac, but consists of Montalban and Huronian, the latter constituting the strata at Harper's Ferry, and for some miles to the eastward along the Baltimore and Ohio R. R. In the southern part of Pennsylvania, to the west of Gettysburg, this mountainous belt, rising between the Mesozoic on the east and the great limestone valley on the west, presents an immense development of a peculiar type of crystalline rocks which I detected there last year, and which has a considerable geological importance. It is a bedded petrosilex, grayish, reddish or purplish in color, sometimes granular, but more often jasper-like in texture, and frequently porphyritic from the presence of small crystals of orthoclase-feldspar or of glassy quartz. There is here found a great breadth of this rock distinctly bedded, presenting different varieties, and alternating with dioritic, or diabasic, epidotic and chloritic rocks, with argillites, in which are sometimes included thin beds of the petrosilex, — the strata generally dipping at high angles to the southeast. These petrosilex beds, with their accompanying rocks, are identical with those which I have described as occurring on the eastern coast of New England, and farther northward along the Bay of Fundy in New Brunswick. They are well seen at Lynn, Saugus and Marblehead in Massachusetts, and also on the shores of Passamaquoddy Bay, where they are interstratified, as in the South Mountain, with rocks having the characters of the Huronian series; to which great division I have provisionally referred these bedded petrosilex rocks, with the suggestion that they probably occupy a position near the base of the series.

This petrosilex is identical in its lithological character with the *hälleflinta*, or stratified flint-rock of the Swedish geologists, which is by them assigned to a similar position, that is to say, just above the most ancient gneisses. It is remarkable as being, in that country, the rock which encloses the most considerable deposits of crystalline iron ores; those found in the underlying gneisses in Sweden, being less important than those of the hälleflinta. This relation has the more interest because, as I have elsewhere pointed out, it is the same petrosilex (sometimes associated, as at Pilot Knob, with argillites) which contains the rich specular and magnetic iron ores of southeastern Missouri, and of which the lithology has been so well described by Pumpelly and Schmidt. Similar iron ores, but as yet in insignificant quantities, occur in the petrosilex rocks on the coast of Maine and in eastern Massachusetts. I may also note that I have observed bedded petrosilex rocks like those just noticed, to the north of Lake Superior, both in an island south of St. Ignace and on the adjacent main land. The conglomerate or breccia which, in the rocks of the Keweenaw series on the south shore of this lake, includes the native copper of the Calumet and Hecla and the Boston and Albany mines, is also made up of the ruins of a precisely similar petrosilex-porphyry.

These peculiar rocks, which make such a conspicuous figure in the South Mountain of Pennsylvania, south of the Susquehanna, are of interest economically, from the fact that they are, in other regions, the repositories of rich iron ores, and also because they afford ornamental porphyries of rare beauty, similar to those wrought at Elfdalen in Sweden. Polished porphyries, from this series of rocks and porphyry-conglomerates, are now exhibited from Massachusetts in the United States government building of the International Exhibition in Philadelphia.

I owe the opportunity to make these observations in Pennsylvania to the Commissioners of the Second Geological Survey of the State, and in my forthcoming report to Prof. Lesley, the director of that survey, I propose to discuss in detail the facts which I thus concisely bring to the attention of the Association.

On the Origin of Mineral Veins. By Charles Whittlesey, of Cleveland, Ohio.

Although many able treatises have been written upon the processes of mineral segregations, the subject does not appear to be exhausted. A correct understanding of these processes is not of scientific interest alone.

It has also a practical side which is worthy of attention.

Dr. T. S. Hunt has recently treated the question of chemical solutions in a masterly manner; but beyond this, there lies the question of the transportation of atoms; resulting in a concentration of mineral in lodes, fissures, and other centres, where it becomes available to man. It is not difficult to explain the origin of fissures, faults, floors, and other places where mineral is lodged. They may, in general, be traced to mechanical disturbance. A fracture once commenced may be increased by the process of its own deposition, which includes crystallization, a powerful expansive force, analogous to the freezing of water.

Fractures may be deepened and extended in this way. The wall rock of a vein is frequently decomposed, forming flucan, which gives greater width to the crack, while the process of filling is going on. What is the agent or force by which these spaces are filled? One law must govern all such deposits, or the result could not be so uniform.

Chemical affinity plays an important part but does not explain all phenomena, and this mode of action, is itself, probably due to electricity. My object will be to show that electricity in some of its manifestations, may be regarded as the principal agent, or force, which accomplishes the filling of veins. I shall use this term in its general sense, to cover galvanism, electro-magnetism and all other modes of electrical action. It may not be a vital force, but is a necessary, all-pervading, and vital agent.

It exists, not alone in the mineral kingdom, but in its gentler forms is present in the vegetable and the animal kingdoms.

The tissues of plants and animals are so constructed as to form galvanical circuits, which appear to play incessantly in the germination, growth, and perfection of organic substances.

The term fissure, or fissure-vein will also be used generically; embracing balls, vugs, stock-werks, floors, and bunches; as well as true veins. The universal presence of electricity being admitted or accounted for, its effects on mineral solutions are well known.

In the use of the telegraph battery, the copper of the solution is separated, and forms on the jars in a crystalline form.

Experiments upon muddy mixtures by introducing wires attached to galvanic batteries, have produced artificial veins. Undoubtedly atoms of simple substances, are in different electrical conditions as a permanent law of matter. Combinations of primordial atoms known as alloys, oxides, acids, and sulphides, must assume new conditions in this respect.

The contact of atoms or of their combinations, must alike change their electrical, and consequently their chemical conditions. In masses of matter there is another source of excitement and disturbance, which is everywhere present on a greater or less scale. I refer to the *unequally heated condition* of the solid, liquid and gaseous portions of the earth. This cause produces mechanical force every day, in the form of lightning.

In the phase of magnetic action, motion and momentum, are imparted to metallic particles and to machinery. Galvanic batteries applied to a solution, or chemical combination, not only change its conditions, but produce motion and concentration.

On the side of magnetism we may consider the earth as an incessant exciter, through the unequalled heated state of its central parts, compared with the surface. Heat which is an indispensable factor is everywhere present.

Wherever there are vacancies in rocks, great or small, subjected to changes of temperature, these spaces are filled with transported matter.

The conglomerates of Lake Superior, frequently have the interstices between the pebbles filled with native copper, epidote, calc spar and iron pyrites. This outside matter must in this instance be concentrated by the same agent, as the contents of fissure-veins. All that is necessary to such a process is an open space to attract the deposit, while the galvanic forces are circulating it. On the earth's surface the equatorial zones are at a higher temperature than the temperate; and the temperate, than the arctic.

Between summer and winter there is a change both on land and water, extending to moderate depths. In the ocean the wide-spread circulation of its waters produces variety of temperature, as it has from the era of the first sedimentary deposits.

The slow but steady effects of a changeable temperature, in waters containing mineral solutions, impinging upon rocks differently heated, must also be taken into consideration. Many of the metals found in sea water go to form part of the sedimentary strata, which, while being deposited were under galvanic influences. Sediments laid down upon igneous rocks have different conducting qualities from them, and produce mineral deposits along the line of contact.

Dykes and overflows of lava produce metamorphisms, not only by contact heat, which involves electrical action, but at a distance, in modes which cannot be accounted for by direct action. The difficulties which present themselves to many minds in finding a sufficient supply of galvanic force, may be overcome by considering the wide-spread effects of changes of temperature. Throughout the globe these changes are incessant.

Where winds blow against mountains which are generally cooler than the atmosphere, thunder-storms are produced for which the explanation is evidently, a disturbance of the electrical equilibrium. I have witnessed on Lake Superior and at Washington City, in mid-winter with deep snow on the ground, a warm south wind give rise immediately to lightning and heavy thunder. In warm climates during the summer, light thunder-storms occur nearly every day, about 2 o'clock, P. M., when the sun is at its greatest force, producing local, and unequally heated currents of air. As the sun moves in declination north or south, it is followed by a movable line of storms parallel to the equator.

These may be traced to the same cause as local storms; that is, to unequally heated portions of the atmosphere.

I have observed the magnetic needle on bare rocky surfaces seriously disturbed about mid-day, evidently due to local electrical disturbance. The diurnal motion of the needle is probably due to the changeable condition of the earth, and the atmosphere during the day.

Volcanoes, when ashes and lava are ejected in large quantities, give rise to local thunder-storms around their summits, when the adjacent region is clear and quiet.

Electricity made manifest by friction in the common case of friction with amalgam and glass, woollen and silk, cloth or the fur of animals, may be explained by the unequally heated condition of those articles. When fires are started in stoves a crackling sound is frequently heard in the pipe, which resembles that of the electrical spark produced artificially. These familiar instances show that slight changes of temperature disturb the electrical equilibrium of many substances.

When such disturbances, are diffused through extended portions of the earth with its varied strata, and variable conducting capacities, the currents must not only be powerful, but must settle into permanent courses or channels.

If electricity is a sufficient agent, and unequally heated bodies are its exciters, there is a perpetual force in action, which only requires time, to produce the effects which we observe in mineral deposits.

On the Origin of Kames or Eskers in New Hampshire. By Warren Upham, of Nashua, N. H.

The river-lands of New Hampshire, composed of modified drift and alluvium, have been carefully studied and mapped as one portion of the work of the State Geological Survey. These include bottom-lands, here called *intervals*, and terraces, rising in steps, the highest of which often forms extensive plains. All these are of fine gravel, sand, clay, or silt, levelly stratified. Ridges are also frequently found, showing an arched, irregular stratification, and composed of layers varying from sand to very coarse water-worn gravel, with pebbles two or three feet in diameter. Wherever the ordinary fine alluvium has been deposited, it overlies or in part covers the gravel ridges, which are therefore the oldest of our modified drift deposits. Similar ridges of gravel have been often described by European geologists, under the various names of *Kames* in Scotland, *Eskers* in Ireland, and *asar* in Norway and Sweden.

As the work of exploration progressed, a large portion of the facts noted, so far as pertained to the plains, terraces, and bottom-lands, including all the levelly stratified alluvium, seemed to be readily explained. These deposits are so intimately associated with the Kames or Eskers, that a short description of them is needed to render the mode of occurrence of the latter more clear. Along the Connecticut River, which separates New Hampshire and Vermont, for a distance of 120 miles south from Fifteen-miles Falls, and thence extending south into Massachusetts, and on the Pemigewasset River, which is the name applied to the upper portion of the Merrimack, the terraces are very numerous, frequently four or five on a side, and reaching a height of 100 to 200 feet above the stream. In the upper Connecticut valley and on the Merrimack River, the alluvium principally consists of the bottom-land and the high terrace or plain, which averages about 100 feet above the river. Both the terraces and intervals have a slight descent with the valley, that of the highest terrace showing frequently a very regular slope. On the Merrimack and Pemigewasset rivers for ninety-six miles, this slope is 5 to 15 feet to a mile, rising from 160 to 750 feet above the sea, being steepest in the upper portion of its course. On the Connecticut River for 120 miles north from Massachusetts line, the slope of the highest terrace is less steep and less regular, rising from 350 to 650 feet above the sea. The alluvial deposits are then interrupted by the Fifteen-miles Falls, where the river seems to have cut a channel for itself through glacial drift, or *till*. Above these rapids the slope of the upper terrace rises with the valley in forty-five miles from 850 to 1100 feet above the sea, averaging 5½ feet to a mile. It is noticeable that the slope of the highest terrace is more regular than the present descent of the river, in which falls or rapids alternate with long distances that are nearly level.

The date of deposition of these alluvial deposits appears to have been at the melting of the great northern ice-sheet, which had accumulated deep enough to cover every mountain summit in New Hampshire. That it overtopped Mt. Washington has been recently discovered by Prof. C. H. Hitchcock, the State Geologist, who has found transported rocks, and shown that glacial drift or till underlies the angular blocks at the summit. The return of a warmer climate gradually melted this great ice covering, the last of which, like the snow in spring, must have rapidly disappeared. The finer materials carried by rivers flowing from the melting glacier, and brought down in the greatest abundance by every stream as soon as the ice had gone, rapidly filled the valleys with great depths of fine gravel, sand, and clay. There seems to be no need of supposing these strata to have been formed in arms of the sea, or in lakes produced by any barriers, as of ice or of drift-materials, which might have obstructed drainage. None of the facts observed require any deep submergence or any different conditions in this respect from those which now exist; and it is probable that these thick deposits accumulated in the same way that additions are made to bottom-lands by high floods at the present day. The melting of the great ice-sheet supplied both the vast amount of material and of water for sweeping it into the valleys, filling them to the level of the highest terraces or plains. Since that time the rivers have been at work excavating deep and wide channels in this alluvium, and the terraces mark heights at which the river during this process has left portions of its successive flood-plains. These conclusions are the same with those which had been announced by Prof. J. D. Dana, based on an examination of the alluvial deposits of Connecticut. The plains, terraces, and bottom-lands, which are thus clearly explained, constitute the larger portion of the modified drift in New Hampshire.

There remained still uninterpreted the remarkable ridges, similar to the Kames or Eskers of Europe, composed of arched, irregular layers of sand or gravel, fine or often very coarse, and in some places containing occasional angular boulders; which are found in New Hampshire, sometimes a single one extending continuously for many miles along the lowest portion of a valley, or elsewhere short and several parallel to each other, or in very irregular mounds and ridges with hollows enclosing small ponds. These ridges, or remnants of them, exist on almost every river in the State. Their position is generally along the middle or lowest part of the valleys, which are bordered by high ranges of hills; but in the southeast part of the State, in some parts of Maine and in eastern Massachusetts, where there are only scattered hills with the valleys not much below the general level of the country, these ridges, of smaller size than in the great valleys, are found extending usually north and south without special regard to the present water-courses.

Similar ridges have been described by geologists in many portions of the northern United States, and it is probable that, if more attention were given to surface geology, they would be found common throughout the whole area that was covered by the continental ice-sheet. In the valleys of our two largest rivers, the Connecticut and Merrimack, they extend for long distances, but had never before been noticed by geologists, owing to the large amount of levelly stratified alluvium, forming the conspicuous terraces and plains, by which these underlying gravel ridges, or Kames, are often nearly concealed. If we imagine this later alluvium not yet deposited in the valleys, which then contained only the Kame extending for miles in a high continuous ridge with steep sides, we shall have the problem that was to be solved.

The explanation commonly offered by geologists in this country and in Europe, but which has plainly seemed to them in many respects unsatisfactory, is that these Kames or Eskers have been heaped up and formed into ridges many miles long, or elsewhere into the most irregular mounds and ridges with hollows, through the agency of marine currents during a submergence of the land. This theory seems wholly unequal to the task of accounting for such Kames as exist in the valleys of the Connecticut and Merrimack rivers, which, being bordered on both sides by high hills, must have formed long estuaries, open to the sea only at their mouths, and therefore not affected by oceanic currents. The explanation of these ridges or Kames, which has seemed to satisfy all the facts observed in New Hampshire, and which it is hoped may add to the correct understanding of this subject, refers their origin to *glacial rivers.* Through the whole glacial period, rivers probably existed to some extent beneath the ice, but their volume and strength of current became greatly increased during the final melting of the great ice-sheet. In many instances these probably filled deep channels along lines of depression upon the surface of the glacier, which at the last part of the melting would coincide nearly with our present valleys, or oftener probably they formed for themselves great tunnels beneath the ice, seeking of course the lowest land for their route. By these glacial rivers, which flowed beneath or on the surface of the ice, discharging the water supplied by its melting, there were deposited from the low water of winter layers of sand, from the strong currents of summer layers of gravel often very coarse, which would be very irregularly bedded, here sand and there gravel accumulating, and without much order interstratified with each other. At the melting of the ice-walls on each side of these glacial rivers, the materials which had collected to a great depth in their channels, were left in long ridges, a section of which would show an irregular anticlinal stratification. Where mounds or irregular short ridges occur, showing by their composition and stratification that they are of the same class with the longer Kames, they have probably resulted from the rapid deposition of these rivers as the last of the glacier was melting, ridges and irregular masses of ice having existed where there are now hollows or ponds. These irregular accumulations of sand and gravel were many years ago attributed to similar causes by Dr. Edward Hitchcock, who accordingly named them *Moraine terraces.* The occurrence of occasional angular boulders enclosed or on the surface of the Kames, is readily explained, as the course of these rivers was probably in most cases beneath the glacier, and they would be dropped from the melting of the ice overhead. Afterward, when the ice had wholly disappeared, many of the valleys were gradually filled, and the Kames covered, as already shown, with levelly stratified alluvium.

The course of the glacial river of Connecticut valley for a distance of twenty-four miles is marked by a single continuous Kame, frequently nearly covered by the alluvium of the highest terraces, extending from Lyme, N. H., to Windsor, Vt. Its height is 150 to 250 feet above the river, by which it has been frequently cut through as well as by tributary streams. This ridge occupies nearly the middle of the valley, and as the river has cut its channel through the alluvium, this has been often a barrier rising steeply upon one side and protecting the plains behind it. In two or three places it has been swept away by the river for a distance of one half mile to one mile, and below these places the terraces show by their coarseness that the Kame has supplied a portion of their material. Probably this ridge, twenty-four miles long, is but a portion of what formerly existed. Short remnants of similar form and material occur northward at Wells River and Colebrook, the last at an altitude of 1050 feet above the sea; and southward at Charlestown, Bellows Falls, Dummerston, and Brattleborough. The material of the Kame in Connecticut valley is principally gravel, always water-worn, the largest pebbles being one to two feet in diameter, with occasional layers one or two feet in thickness of coarse sharp sand. Large angular boulders are very rarely found embedded in the Kame or upon its top. A section invariably shows an anticlinal stratification.

In the Merrimack valley a series of Kames, always in ridges, sometimes a single one, but more often with irregular branches or several parallel to each other, extends from Loudon along Soucook River and the west side of Merrimack River to Manchester, a distance of twenty miles. Their height varies from 60 to 125 feet above the river, and they are often covered, or nearly so, by the alluvium. These ridges are coarser than the Kames of Connecticut valley, consisting almost wholly of very coarse water-worn gravel, with the largest pebbles three to four feet in diameter, and containing fewer and only thin layers of sand. The stratification is sometimes indistinct; but it is anticlinal, wherever it is shown. On the Soucook River and south from the Pinnacle in Hooksett, these ridges of coarse water-worn gravel are found changing for several hundred feet into large angular blocks and earth with scarcely any water-worn pebbles, succeeded again by coarse rounded gravel. These portions resemble ordinary till, except that the blocks do not show glaciation, and they are plainly part of the same continuous Kame, elsewhere composed of water-worn materials. Here the course of the glacial rivers which formed these ridges seemed to have been determined by medial moraines, which have thus become a portion of the Kame.

Further light is thrown upon this page of geological history by another series of Kames, terminal moraines, and extensive plains, extending south from the Saco River at Conway through a narrow low valley to the basin of Ossipee Lake, and thence by a low valley extending south and southeast past Pine River Pond and Balch Pond into Maine. The principal outlet during the melting of the ice-sheet from the southeast side of the White Mountains appears to have been along this line, crossing two low water-shed divisions. The height of the Saco River at Conway is 440 feet above sea; of the water-shed between this and Ossipee Lake, 516; of the Lake, 408; of the water-shed between Pine River Pond and Balch Pond, about 550. After the glacier had retreated from the coast it seems for a long time to have still covered the Ossipee Lake basin and the valley of Pine River and Balch ponds to the southeast. During this time a large glacial river, shown by a Kame extending several miles along Pine River, brought down great quantities of gravel and sand which form extensive low plains stretching southeastward from Balch Pond. This Kame of Pine River varies from 75 to 125 feet in height, and is always composed of water-worn materials, the largest pebbles being two to three feet in diameter, with occasional angular boulders enclosed. Nearly all the modified drift along this valley, extending to the average width of a mile, consists of gravel with pebbles six inches to one foot in diameter, and it is all more or less ridged, with irregular hollows, indicating that masses of ice still remained at the time of its deposition.

After this, the glacier disappeared from the broad low basin of Ossipee Lake, and again for a long time had its terminal front at the border of the low area from which it had retreated. Its moraines fill the west side of the narrow valley between Madison and Conway. These consist of ridges and heaps of large and small, angular or glaciated boulders and earth, disposed with all possible irregularity and confusion. These have been derived from the precipitous hills on the west side of this valley, and from the neighboring mountains at the northwest. Along the middle and east side of this valley, beside the railroad, these ridges are of water-worn coarse gravel with marks of stratification. The largest pebbles in the south portion are two to three feet in diameter, but they are less coarse farther to the north. Occasional boulders are found embedded in these Kames. Thus at the west and higher side of the valley, the most irregular mounds and ridges of morainic material are found, which gradually change as we come to the centre of the valley to ordinary water-worn Kames. The distance that these extend is about six miles. At the south end of the valley, which was the outlet, the Kames are very coarse, and are short and irregular. Here they are in part moraines modified and water-worn by the outflowing glacial river, the course of which in the north half of the valley is marked by a single conspicuous Kame, 75 feet high, extending on the east side of the railroad for three miles, its north end being at Pequawket Pond. This appears for a long time to have been the outlet for the melting of the glacier that covered the Saco valley, and the material brought down was spread out to form the extensive sand and gravel plains about Ossipee Lake and Six-mile Pond. From the comparatively small amount of levelly stratified drift associated with these Kames, it seems probable that before the ice here had wholly disappeared, the present outlet by Saco River was opened and conveyed the later alluvium in that direction.

Having now described the most important Kames of New Hampshire, it will be well to sum up briefly the reasons which we have for considering them the deposits of glacial rivers. From explorations in the Alps and in Greenland, we know that streams in summer are found flowing on the surface of glaciers, and that falling through crevasses these gather to form considerable rivers beneath the ice. At the final melting of the great continental ice-sheet, we must suppose that such rivers existed both beneath and on the surface of the ice. From the abundance of abraded materials which we know to have filled the ice-mass, especially in its lower portion, the melting must have set free great quantities of various degrees of fineness, to be swept down by the glacial streams. The swollen current, we must suppose, carried forward the clay, sand, and fine gravel, to be deposited after it had escaped from its glacial channel. This channel, however, would probably contain strata of gravel with coarse pebbles or boulders, rounded by the water, and occasionally mixed with layers of sand, deposited by the diminished currents of winter. The melting of the ice on each side would leave these materials in long ridges, which by the falling down of the slopes would assume an anticlinal stratification. Irregular ridges or mounds would be produced where the materials were deposited in hollows among irregular masses of ice. When the rivers flowed beneath the glacier, as was more commonly the case, angular boulders would be dropped from the melting ice and be found embedded or on the surface of the Kame. These coarse deposits would be likely to resist subsequent eroding agencies, and might be expected to be found in the principal valleys, or, where these are not prominent features, extending across the country without regard to the present courses of streams.

Now these deposits, which after consideration we should expect as the product of the glacial rivers, are exactly what we find in the Kames or Eskers of New Hampshire. Their frequency in this State, and their well-marked and interesting character, point them out as an important portion of our records of glacial time, but they did not appear to accord with any explanation which supposed them to have been formed by marine currents. In Europe this theory has been held, as set forth, but not in full confidence, by Mr. Geikie in his recent important work on the "Great Ice Age." A satisfactory explanation of the facts observed in New Hampshire and their full significance, seem to be found in referring the origin of these ridges to glacial rivers, and it appears that the same explanation will be found to apply in general to these deposits wherever they occur.

Another subject of much interest in this connection has been also presented by these explorations in New Hampshire. Banks of sand are found at heights varying from the level of the highest terrace to two hundred feet above it, apparently isolated on the hills, along the east side of Connecticut and Merrimack valleys and southeast of Ossipee Lake. These patches of sand are very conspicuous because they are often destitute of vegetation and blown in drifts by the wind. They vary in size, the largest sometimes covering an acre or more and being ten to fifteen feet in depth. Their irregularly sloping surface, not level-topped like the terraces, and their great elevation, in many cases 100 to 200 feet or even more above the highest of the regular river deposits, made it necessary to refer their origin to some cause different from that which filled the valleys with the ordinary alluvium of the terraces and plains. This cause appears to have been the prevailing northwest winds, which were probably the same at the end of the glacial period. The valleys being then filled from side to side with their original alluvial plains and being not yet covered by forest, these winds could have acted with more effect than at the present day. That this is a true explanation of these high banks of sand appears to be proved by the fineness of their material which contains only particles such as could be carried by the wind; by their frequent occurrence on the east side of the valleys where they would be formed by the prevailing strong northwest winds, while they have never been found on the sheltered hills of the opposite side; and by the train of sand-drifts, usually grassed over, which may be always found leading down in a northwest direction from the banks of wind-blown sand to the ordinary alluvium of the valley. Since the clearing of the country the upper portion of these trains of sand has often been carried several hundred feet onward and thirty to fifty feet higher. The excavation of the old drifts has been sometimes six or seven feet in depth, as shown by great stumps beneath which the sand has been swept away by the wind. These *dunes* are ridged, channelled, and heaped up, in the same manner as the more extensive dunes of a sea-coast. Similar irregular accumulations of sand, observed elsewhere, have been often included in the same class with the Kames. The inland dunes which we have described lead to the question whether these mounds or ridges of sand may not have sometimes been produced by winds. In any case, under the explanations here suggested, it would appear that deposits, consisting wholly of fine sand, must be regarded as of different origin from the ridges and mounds, composed mainly of gravel, often very coarse, and stratified in arched, irregular layers, to which the names of Kames, Eskers and Asar have been applied.

A description of these remarkable deposits in Scotland, Ireland, and Scandinavia, in the last of which they sometimes extend 150 to 200 miles with tributary branches, and a full statement of the theories under which they have been explained, may be found in the very valuable work of Mr. Geikie, already cited. (See "Great Ice Age," American edition, pp. 209–237, p. 346, and pp. 357–367.) The account there given of these ridges, as to their mode of occurrence, their materials and stratification, and other particulars, is very full and complete; and all the facts stated in regard to them in these countries seem to be simply and clearly explained by referring these deposits, like those described in New Hampshire, to the action of glacial rivers.

Glacial or Ice Deposits in Boone County, Kentucky, of two distinct and widely distant periods. By George Sutton, of Aurora, Indiana.

Upon the most elevated portion of the table-land in Boone County, Kentucky, at an elevation of between 450 and 500 feet above high-water mark of the Ohio River, or about 1,000 feet above the ocean, may be seen extensive accumulations of drift. This drift, in some places, is cemented into firm conglomerate, and caps the highest hills on the north side of Middle Creek, and also on each side of Rattling Run—a small stream entering Middle Creek—presenting perpendicular cliffs, varying from thirty to forty feet in height, making a marked contrast to the general appearance of this section of country, and affording a fine subject for investigation and speculation to the geologist.

Near the mouth of Wolper Creek, between five and six miles northwest of this formation, may be seen another deposit of drift, also cemented into conglomerate, which overhangs the creek with perpendicular bluffs. This formation is more than 100 feet in thickness above high-water mark, but between 300 and 400 feet below the deposit of conglomerate on the highlands above Middle Creek. The two deposits have received but little attention from geologists. No mention is made of them in the geological reports of Kentucky, and the only notice that I have seen published of the deposits on the highlands above Middle Creek, is a brief description by Prof. John Lock in the "Cincinnati Gazette," in the year 1845. Mr. Robert B. Warder, in the Geological Report of Indiana for 1872, merely directs attention to Split-Rock opposite the mouth of Laughery Creek as possibly being the terminal moraine of an ancient glacier. Prof. Lock regarded this conglomerate as the evidence of the destruction of a great arch of rocks which united the coal-fields of Ohio with those of Indiana and Kentucky. He says: "The question arises, did this mountain-arch ever exist in fact at the place of our city, or in other words, were the bent layers which are cut off both to the east and to the west, ever complete and continuous? I am of opinion that these layers were continuous, and that causes difficult to be ascertained have swept the upraised layers away, leaving a level country, the surface of which cuts the layers of rocks obliquely and in reverse order both east and west of us." * * * * "What has become of the ruins of these removed layers? I am decidedly of opinion that the conglomerate of Split Rock exhibits a portion of them, for there we have the layers of blue limestone with its millions of characteristic fossils, forced up, piled chaotically together, and re-cemented in the fantastic heaps in which it was piled by the whirlpools."

We direct attention to these two drift formations, presenting evidence not of the destruction of a great arch of rocks which possibly at one time united the coal-fields of Ohio with those of Indiana, but of the transporting power of ice either by glacial or river action at what appears to me to be two distinct and widely distant periods; both, however, since the formation of the great drainage lines across the continent. The deposits of the one period must have been made prior to the formation of the present Ohio Valley: the other after the river had cut down its channel to nearly its present depth.

It is well-known that the surface of the country over all this portion of the *lower silurian* formation, was once nearly level, or only slightly undulating, and that the Ohio River and small streams have cut their channels through this table-land to the depth of from 400 to 500 feet.

The altitude above the ocean at the Ohio and Mississippi R.R. depot at Aurora is 492 feet. The depot is about ten feet above the high-water mark of the Ohio River, and a few miles to the north of the conglomerate in Kentucky.

The table-land at Milan, about ten miles to the west of Aurora, is 506 feet, 10 inches higher, making the highest portion of the country about 1,000 feet above the ocean, and showing the depth of the valleys to be between 400 and 500 feet. The table-land in Kentucky, above Middle Creek, is probably the most elevated of any throughout this section of country.

The conglomerate upon the most elevated portion of the table-land may be seen in a retired part of the country about two miles and a half from the mouth of Wolper Creek; from this point it may be traced across the country in a south-easterly direction, following the trend of the river hills to Middle Creek, where it attains its greatest thickness, varying from 30 to near 100 feet, and at an elevation of about 500 feet above high-water mark in the Ohio River.

On the south side of Middle Creek we again find this formation upon the highest portions of the country. It may be traced capping the hills in a southeasterly direction, giving a reddish appearance to the soil, but presenting here more the appearance of uncemented drift, than on the north side of Middle Creek. It caps the hills, apparently, along the line of ancient drainage, evidently having been deposited before the river and small streams had worn out their present valleys. But little of this drift is found in the valley of Middle Creek; occasionally, on the hill-sides, a piece of conglomerate may be seen that has rolled down from its more elevated position.

The composition of this conglomerate and drift presents a great variety of formations, the silurian, however, predominating. The angles of the fragments are rounded, and every fragment bears the most conclusive evidence of being water-worn. No evidence of stratification can be seen in the perpendicular cliffs; small pebbles and large angular boulders may be seen mingled in confusion and so firmly cemented together that it is difficult, in some places, to break fragments from the main mass.

The conglomerate at the mouth of Wolper Creek is about five miles north-west of that seen on the highlands above Middle Creek; it presents perpendicular cliffs, and is more than 100 feet in thickness. The perpendicular height in one place that I measured was 73 feet. Above this cliff there was a rise of 20 feet to the highest point, and it probably extended many feet below the soil and rubbish at the bottom, making the deposit at this place at least 100 feet in thickness. Above the mouth of Wolper Creek a large mass of this conglomerate has been undermined by the river, and slid off from the main body, making a narrow chasm of several hundred feet in length, and from five to six feet in breadth; this point is known as *Split Rock*. On one side of this chasm we measured a perpendicular height of 72 feet, and above this conglomerate there is at least 20 feet more of drift and soil. How much below the rubbish at the bottom of this chasm the conglomerate extends, we do not know, but it is below high-water mark.

This conglomerate, like that on the highlands, is composed of a great variety of formations; fragments varying from the finest sand, to several hundred pounds in weight, intermingled in every state of confusion. Large boulders of granite may be seen imbedded and cemented along the perpendicular wall 60 feet above high-water mark—showing that there must have been a transporting power much greater than water alone to have piled up these masses of stone to such an elevation. They show, also, that when this conglomerate was deposited, the Ohio River must have had a much greater volume of water than it has been known to have during our highest freshets within the last 80 years, or since the country has been settled.

This accumulation of conglomerate is between one and two miles in length, and in some places nearly a mile in breadth. It blends with a stratified and also an undulating terrace formation, which is six or seven miles in length along the river and in some places more than a mile in breadth.

When we stand upon the most elevated portion of the cliff overhanging the mouth of Wolper Creek, and look over the undulating cultivated fields back to the river-hills which rise 300 feet above the terrace formation, and bear in mind that on the top of these hills is the conglomerate to which we first directed your attention, the conclusion seems to me to be irresistible that these two deposits of conglomerate were made at widely distant periods. The one dating back to a period *prior* to the formation of our valleys, the *other after* the river had cut down its channel 450 feet below this table-land. The antiquity of the one is seen most clearly where it caps the hills on each side of Middle Creek, and, also, on each side of Rattling Run, showing that the deposit of the one *must* have preceded that of the other by the length of time which it took to form the Ohio Valley and the valleys of its tributaries.

• Such, briefly, are some of the facts which may be seen by a visit to this section of country.

The question arises, how came this accumulation of drift at these points? When we look at the map of this part of the country, we see that the Great Miami, and the White Water rivers running from the north, empty into the Ohio Valley near this point.

Here, also, we find that the river makes a sudden bend to the south-east, forming the western angle of the great *North Bend* in the Ohio River. A line drawn from the White Water and Miami Valleys across this acute angle, intersects the conglomerate at Wolper Creek. Along this line behind the hills to the east of Petersburgh, Kentucky, may be seen *an ancient valley* which the Ohio River has long since abandoned. This valley is between three and four miles in length, and from one-fourth to one-half a mile in breadth. It was evidently cut down through the strata of rocks, and shows that at one time, here was an extensive island, not formed by the accumulation of drift or sediment, as most of the islands in the Ohio River now are, but an island made by the river eroding out two channels through the strata of the silurian formation. It is now above the highest floods of the Ohio River—is extremely fertile, and is cultivated throughout the whole length. Wells sunk in it pass through loam, sand and gravel similar to what we find in our low bottom-lands.

Along the White Water and Miami valleys, we see accumulations of drift. We are told in the Geological Reports of Ohio, that the drift formation is traced across the state of Ohio from Ashtabula to Dayton. It is seen in large quantities near the mouth of White Water, and its outlet to the south and south-west was the mouth of the Miami Valley. Now, if we imagine a time when there was a vast accumulation of ice at the junction of these streams—this ice principally brought down from the north through the Miami and White Water valleys—the ancient drainage lines probably valley glaciers—this ice meeting the ice in the Ohio, and forming an enormous ice gorge at this point, grinding along the eastern bank of the Ohio River, and piling up in confusion sand, gravel, and boulders—it seems to me we have an explanation for the accumulation of the drift found near the mouth of Wolper Creek.

The sudden bend in the Ohio River and the narrow valley just below the bend probably produced ice obstructions in an ancient day just as we see ice obstructions produced at this portion of the river at the present time. It is also probable that at some remote period the ice obstructions turned a part of the volume of water across this acute angle in the river and formed the cut-off or ancient valley seen beyond the hills east of Petersburgh.

On the Indiana side of the river, we again find the drift near the river hills between two and three miles above Rising Sun, or nearly opposite Laughery Island, and at about the same height as the river terrace formation in Kentucky.

We also find near the mouth of Nogan Creek, and extending back from the river along the valley of this creek to the town of Cochran, another terrace-formation which is nearly of the same height as the terrace on the opposite side of the river in Kentucky. From an excavation made for a turnpike, and also for excavations made for the O. & M. R. R., we see that this terrace, unlike the terrace in Kentucky, is almost entirely composed of laminated clay and loam; scarcely any gravel can be seen. It is away from the current of the Ohio River and was evidently formed by the deposits of sediment in the back water during ancient floods or freshets of the Ohio River, just as deposits are left after the freshets at the present time on the lowlands back from the river. But it is difficult to account for the thickness of this terrace rising as it does from a level of the bed of the river to nearly 50 feet above high water mark, unless we adopt the theory that there were at one time floods in the Ohio River far greater than any known since the country has been settled, a view which would be in accordance with the theory of the melting of a great continental glacier.

The accumulation of drift on the highlands above Middle Creek were probably produced by causes similar to those that made the deposits in the valley, but at a far more ancient period. We can imagine how ice borne down from the north along the ancient drainage lines of the White Water and Miami Rivers, and meeting the ice in the Ohio at this bend, would crowd it on to the south side of the then shallow valley and leave deposits; the same which we now see, and which have so effectually resisted the decomposing effects of time.

In attempting to account for the formation of these deposits it is not necessary to allude to the changes that may possibly have taken place at different periods in the elevation or depression of portions of the continent. The same great drainage lines now seen in this part of our country must have continued from the time the river first began to erode out its present valley, and in this long series of years changes of climate have taken place, and the conglomerate to which we now direct your attention is evidence we believe, to sustain the theory that ice has brought down from the North boulders and drift at two distinct and widely distant periods.

New facts relating to Eozoon Canadense. By J. W. Dawson, of Montreal, Canada.

At the last meeting of this Association, I had the pleasure of exhibiting some specimens of *Eozoon Canadense*, and of giving some oral explanations as to its nature and mode of occurrence. I now ask permission to mention a few additional facts which have been made known since the meeting at Detroit, and which still further contribute to our knowledge of the most ancient known fossil.

(1.) I would first beg leave to direct attention to the very interesting series of specimens now on exhibition in Philadelphia, in the collection of the Canadian Geological Survey; and which give a rare opportunity to study the various aspects of the fossil. In connection with Eozoon, I would also mention the remarkable mass of Graphite from Buckingham on the Ottawa, exhibited by the Dominion Plumbago Company of Canada. This mass is from one of the great beds of that mineral occurring in the Lower Laurentian, on a horizon not remote from that of Eozoon, and which in my judgment are really Laurentian coals, representing the vegetation of that period, as yet altogether unknown to us in its forms and structures.

(2.) A very interesting specimen, found last autumn by Messrs. Richardson and Weston, at Petite Nation, has enabled me to delineate, in a recent paper, the inverted conical form of a perfect small specimen of Eozoon, and also to show that the acervuline chambers on its upper surface are precisely similar to those small aggregations of spherical chambers resembling *Globigerinæ*, and to which I have given the name *Archæospherinæ;* so that these may not improbably be loose chambers or germs of Eozoon.

(3.) Mr. W. J. Morris of Perth, Ontario, has in the past summer found abundant specimens *in situ* of Eozoon mineralized with Loganite, in the original locality at Burgess. These specimens show that the Burgess variety is on the whole thicker and more continuous in its sarcode chambers, and less developed as to the separating walls than the Grenville and Petite Nation specimens. These new specimens from Burgess have also enabled me for the first time to detect in their dolomitised walls traces of the canal system, into which, however, the Loganite does not penetrate. In some in which the dolomite is mixed with calcite, there is also an extremely minute granular structure, which I believe to indicate an originally porous character of the cell-wall, of which only obscure indications exist in other specimens.

(4.) Mr. G. F. Matthew has sent to me from the Laurentian of Lily Lake, near St. John, New Brunswick, specimens of a dolomitic limestone containing fragments of the skeleton of Eozoon, showing the canal system. This is the first recognition of this fossil in the Laurentian of New Brunswick. A notice of the fact has appeared or will shortly appear in "Silliman's Journal."

(5.) Recent explorations by Mr. Vennor of the Geological Survey have thrown further light on the precise geological horizon of Eozoon in the great Laurentian system. In Sir William Logan's original sections on the East side of the Ottawa, the lowest rock represented is a great thickness of orthoclase gneiss, corresponding probably to the fundamental or Bogian gneiss of the Scandinavian and Bavarian geologists. Above this is a very thick limestone, that of Trembling Lake, which has afforded no fossils. Next is another vast thickness of gneissic beds. Then comes a second limestone, also non-fossiliferous as yet, that of Green Lake. Then another gneissic series and a third limestone, that of Grenville, which is the special resting place of Eozoon, and is also associated with beds rich in graphite and in calcic phosphate. Still higher is a fourth limestone, and then the Upper Laurentian. Mr. Vennor's observations relate to a region about eighty miles distant, on the west side of the Ottawa and remarkable for its rich deposits of apatite and graphite, though affording Eozoon only in a few places, and in these not precisely in the same state of mineralization as at Petite Nation and Grenville. In this region Mr. Vennor has worked out a series corresponding in its main features with that ascertained by Logan, and it now appears that in both series Eozoon is apparently confined to one horizon, and that in this it is associated with the more important deposits of graphite and apatite. It is true that in the districts explored by Mr. Vennor there are some groups of strata of uncertain age, and which may be upper Laurentian or even Huronian; but the main accordance above stated seems to be certain. It would thus appear that Eozoon and those deposits of graphite and apatite which are probably of organic origin, are characteristic of one great zone of the Lower Laurentian.

(6.) The abundant phosphates occurring in the Lower Laurentian, and as already stated in irregularly stratified beds, and associated with graphite and Eozoon, naturally raise the question whether they are of organic accumulation. The apatite of the Lower Laurentian has indeed as yet afforded no organic structure. Some light may however be thrown on its origin by the analogy of later deposits of similar character; and I have endeavored, in a paper recently read before the Geological Society of London, to show that the calcic phosphate contained in the Cambrian and Silurian rocks of Canada presents in its mode of occurrence points of similarity to that of the Laurentian; while the prevalence of low forms of life, as *Lingulæ*, *Trilobites* and *Hyolithes*, having much calcic phosphate in their skeletons, in the Primordial seas, and the consequent accumulation of beds rich in phosphatic concretions and coprolites, points to the possibility of similar conditions in the earlier Laurentian. I may also here refer, as corroborative of this view, to the recently published researches of Hicks and others on the Silurian Phosphates of Wales.

(7.) The objections to the animal nature of Eozoon recently promulgated by Otto Hahn, and which have been answered in detail by Dr. Carpenter and myself, have directed attention anew to the geological relations of serpentine; and though I must protest against the idea prevailing in some quarters, that there is any necessary connection between this mineral and Eozoon, yet as serpentine exists in connection with many specimens of this fossil, it is time that geologists were warned against the extravagant ideas of pseudomorphism which have been promulgated in connection with it. I have, therefore, been engaged in the present summer in reëxamining large series of specimens of serpentines associated with organic remains, and have visited some of the Canadian localities of such serpentines, and have studied their geological relations. I hope to show, when these researches are complete, that microscopical and palæontological evidence completely vindicates the theory of aqueous deposition of serpentine as maintained by Dr. T. Sterry Hunt, and shows that this mineral, like glauconite and similar silicates, may fill the pores and cavities of fossils, without in any way destroying their forms or structures. I have examples of Silurian corals and other fossils mineralized with true serpentine, precisely like Eozoon in the Laurentian. Further it can be shown that the Lower Silurian serpentines of Canada, alike in their interstratification with fossiliferous limestones, and in their passage into limestone, dolomite and even red slates, conform in a striking manner to the known laws of deposition of hydrous silicates in the modern oceans. Whatever opinions may be held as to the metamorphic origin of certain serpentines, or as to the mode of formation of serpentine veins, the facts I already possess are amply sufficient to show that such theories have no application to the ordinary serpentines found in beds associated with fossiliferous rocks.

(8.) I may add that I hold Gümbel's elaborate exposition of the foraminiferal nature of *Receptaculites*, in the Transactions of the Royal Bavarian Academy, and the announcement by Prof. Karl Moebius of a recent sessile Foraminifer from the Mauritius, not very remote from Eozoon in its general mode of growth, to be important contributions towards the history of this oldest fossil; whose investigation, as will be seen from the above notes, is by no means fully worked out.

On some Unexplained Phenomena in the Geyser Basins of the Yellowstone National Park. By Theo. B. Comstock, of Ithaca, N. Y.

When it is considered that all our knowledge of the remarkable workings of nature in the Yellowstone Park has been gathered by a few observers wholly during the summer months, it need not seem strange that many questions have arisen which can only receive answer by future investigation. Every new observer has to report changes in the force and character of eruptions, as well as differences in the nature and amount of the deposits of the various hot springs and geysers—changes which give evidence of anything but regularity in the subterranean movements. The *Giantess* geyser, for instance, has been only twice seen in action; the *Giant* as reported by Hayden, Peale, Doane, Barlow and Heap, and myself, is known to have changed very much in character between the summers of 1870 and 1873, and important differences were noticed in the action of the Hot Springs on Gardiner's River by the same observers at different times during the above interval. These are but a few of the many cases of a similar kind which might be cited, but they are sufficient to show that any attempt to solve the problems connected with the geysers must be the result of constant and long continued observation at all seasons of the year.

It is the object of this paper to direct attention to the importance of improving all opportunities for research in this direction, as well as to bring to the notice of the Association several interesting observations made by myself while traversing the region under consideration.

According to known facts and the theories by means of which their interpretation has been attempted, the occurrence of what may be called "sympathy" in the eruptions of adjacent or widely separated geysers does not seem impossible, but they furnish no adequate explanation of the striking cases of this nature which have been observed in the geyser basins of Fire Hole River. A few prominent examples will serve to illustrate this point. In the Lower Geyser Basin, it is quite the rule for the geysers to play in groups, as in the case of the *Fountain*, which is almost always associated in eruption with several of its neighbors upon the same mound or terrace, including the *Clepsydra*, the *Fitful* and some irregular springs. The *White Dome*, about one mile distant from these, rarely or never is seen in action without being immediately preceded or followed by the eruption of from one to six or eight others within a radius of a quarter of a mile. In the Upper Fire Hole Basin, the eruptions of the *Turban* and the *Saw-Mill* are frequently, if not always nearly coincident, while the overflow from the *Turban* often excites the powerful *Grand Geyser* to excessive activity. Similarly, but with less apparent reason, the three chimneys, which I have named the *Trinity Geysers* from their resemblances in form and phenomena, were seen in 1873 in almost simultaneous action. Not only this, but at the time of our visit to this region, nine of the geysers of this Upper Basin were observed in action, as follows: — First the *Grotto* was agitated, an eruption of the *Riverside* beginning just before the former became quiet, the *Fan* becoming active about the middle of the eruption of the *Riverside*. These three geysers are rather widely separated, although they occupy together a somewhat restricted portion of the basin. Above these, and issuing from the floor of the basin all within the space of a few yards, are the *Turban*, the *Saw-Mill* and the *Grand Geysers*. At the time mentioned, these were seen in action very shortly after the cessation of activity in the lower group, and, during a portion of the eruption of each, all were playing together. These eruptions had ceased but a few minutes, when the *Trinity Geysers* farther up the Fire Hole River and much more elevated, were almost simultaneously agitated. Numerous illustrations of apparent sympathy might also be drawn from the various groups of mere hot springs, which have been published in my own report. My present object being only to indicate some problems meriting closer study, I will offer no theory, but merely remark that in many cases in which this apparent sympathy is seen between more or less widely separated springs, others much nearer are wholly independent in their manifestations, if they do not actually exhibit sympathy with springs in other portions of the basin. So far as I have been able to learn from previous reports and from personal observation, there is nothing to suggest any approach toward "sympathy" with other geysers in the case of the *Castle*, of the Upper Basin. This chimney is somewhat isolated, but it has nearer neighbors than some which appear quite closely connected in action. The *Giant*, the *Giantess*, *Old Faithful* and others, are superficially scattered and no indications of correlated activity have been observed. There may be some significance in the singular fact that, with the exception of the *Giantess*, *Beehive* and some minor bowls near the extreme upper end of the basin, on the right bank of the Fire Hole River, and the *Grotto*, at the lower end, upon the opposite bank, all the geysers on the former side of the stream seem more or less sympathetic, while those upon the left bank are all apparently solitary in their movements.

Another equally interesting feature was observed in both of the main Fire Hole basins, viz:—when a number of geysers or geyser groups became successively active, the course of the eruptive wave was usually up stream. It would be hazardous to attempt an explanation of this phenomenon without an abundance of evidence, but it may be stated that the ordinary theory of the geyser does not seem alone sufficient to account for this. Bunsen's admirable investigations in Iceland resulted in an explanation of the cause of eruptions which is generally accepted. No doubts of the correctness of this view are suggested by observations in the Yellowstone Park, except as regards what may be called minor phenomena, though some of these may prove to be of greater importance than is now thought. There is abundant reason for believing that the surface features of the various springs have a marked effect upon the character of their pulsations, and, as the writer has already remarked,[1] Bunsen's theory must probably be supplemented by others to account for all the phenomena which have been observed. The retardation of the eruptions of the more elevated and, presumably, more elongated geyser tubes does not seem remarkable, except in so far as it points to some very regular source of water supply under ground. Differences in the heights of the several columns of water would probably cause irregularity in the duration of their respective dormant intervals, but this does not seem to be the case with these geysers.

Besides the foregoing hints of discoveries which await the investigator in this region, others are furnished by the peculiar vascillations in temperature of the water at different points in the streams, the causes of which are often very difficult to explain from surface indications alone. The presence of carbonic acid in some instances will account for the agitation of the contents of

[1] Report upon the Reconnaissance of N. W. Wyoming, etc., Jones, 1873.—Edition from Engineer Office, 1875, p. 257.

bowls at a temperature considerably below even the reduced boiling point at the great altitude of the district; but we must look for underground agencies to interpret phenomena of the kind above indicated.

The building up of geyser cones has been described more than once by those who have visited the basins of the Fire Hole, and yet those who have the best knowledge of their structure, and its numerous modifications, are most free to acknowledge that our ignorance upon this subject alone is very great. Even so simple a matter as the formation of polished pebbles in the pools adjacent to the geysers, was not clearly understood until investigated by the writer in 1873, and many questions have arisen which will only be solved after months or years of patient observation.

Again not a few geysers are quite irregular in their eruptions, though their near neighbors may spout almost as regularly as the clock strikes. The *Giant*, the *White Dome* and the *Castle* have been differently reported by observers at different times, while *Old Faithful* has not been known to miss its average of one eruption each hour since its discovery. Another fact of interest is the difference in the length of the dormant intervals in adjacent geysers. *Old Faithful* and the *Castle* are within sight of each other, and not very dissimilar in external aspect, and yet the former ejects its liquid contents twenty-four times for every eruption of the latter, *i. e.*, it did so in 1873.

In the Lower Geyser Basin there is evidence unmistakable of one or more floods of enormous extent. In a low, level portion between the *Fountain* and the *White Dome*, the trees in 1873 were covered at the base to a height of two or three feet with a coating of silica similar to the ordinary geyser deposits of the region, and upon the flat beyond the *Fountain* a thick, sandy deposit of the same material is spread over the surface. The many signs of freshness visible over this area lead to the belief that the amount of overflow from the geysers has quite recently been excessive. Upon the well founded supposition that the water supply of the subterranean chambers is largely furnished from the neighboring country, it seems probable that the manifestations during the winter or spring months will richly reward the observer with new and valuable facts, which will have an important bearing upon questions relating to thermo-dynamics. There are certainly few fields which promise as great a return for a small amount of investigation. The attention of Congress should certainly be called to the pressing need of more careful espionage and government of the Park, for its treasures are fragile in the extreme, its manifestations waning and evanescent, and profane hands have already ruthlessly torn and shattered monuments of great scientific value, which can never be replaced.

In closing, I would venture to repeat what seems to me merely the fulfilment of a duty, by again requesting the thoughtful attention of scientific men in all departments of physical research, to the "great importance of *prompt*, *constant*, *extended and connected observation* of the rare and rapidly waning phenomena, which form the most striking and characteristic features of the district under consideration."[2] The last report was made three years ago, and many changes have doubtless occurred since that date, while the waste arising from spoliation is enormous and constantly increasing with absolutely no means of protection.[3] Numbers stand ready to sacrifice time and energy to the work, and a bare allowance for sustenance is all that is needed to make the National Park a source of glory rather than of discredit to the nation, as it now is.

The "Two-Ocean Water."—the Union of the Atlantic and Pacific Oceans in the Rocky Mountains. By Theo. B. Comstock, of Ithaca, N. Y.

On the map of Captain (now Major) W. F. Raynolds, embracing that portion of the Missouri River country traversed by himself in 1859 and 1860, there was first recorded the peculiar hydro-

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[2] "American Naturalist," Vol. VIII, March, 1874, p. 158. Pamphlet extract, "Scientific Value of Yellowstone Park," p. 21.

[3] Since the reading of this paper, a report has been published of the "Reconnaissance from Carroll, Montana, to the Yellowstone National Park, in the summer of 1875," conducted by Capt. Wm. Ludlow of the Engineer Corps, U. S. Army, with a valuable geological report by Edward S. Dana and George Bird Grinnell. The statements of these gentlemen indicate great natural changes since 1873, and their representations of the ruthless destruction now in progress afford the strongest confirmation of the truth of the views here expressed.

graphical feature known as the "Two-Ocean Water." Its position is there indicated roughly by means of dotted lines, according to the account given by Bridger, the guide of the party. In the report of the expedition Raynolds remarks that "having seen this phenomenon on a small scale in the highlands of Maine, where a rivulet discharges a portion of its waters into the Atlantic and the remainder into the St. Lawrence, I am prepared to concede that Bridger's 'Two-Ocean River' *may* be a verity."

Later explorers were unable to pass judgment upon this statement until the summer of 1872, when some of the members of Dr. Hayden's party made important discoveries in the district within which the peculiar water-shed must have been discovered, if not wholly mythical, as many supposed. Dr. Hayden, after a careful reconnaissance of the region, reported that such a phenomenon was at least doubtful, at the same time suggesting that the "low ridge in the great water divide of the continent has doubtless given rise to the story of the Two-Ocean River, and such a stream has found its way to most of our printed maps."[1] He had previously announced the same opinion in the following words: "Between Flat Mountain and the Yellow-Stone Range the divide is very low. The sources of some of the branches of Snake River extend up within two miles of the lake, and the elevation is not more than 400 feet above the lake level. This is what has been hitherto understood as 'Two Ocean Pass.'"[2]

The expedition of Capt. W. A. Jones, in 1873, was more fortunate in this respect. Having ascended the valley of the Upper Yellowstone for twenty-five or thirty miles, the trail of our party left the marshy bottom-lands to traverse the drier portion about fifty feet above the stream upon the right bank. The river at this point was then (early in September) rather sluggish, the slope being somewhat gradual. Presently we crossed a small, but rapid rivulet coursing down the mountain side and falling abruptly into the valley just beneath us. Beyond us, the view was unobstructed, but the stream appeared to ascend the slope towards us until we observed that the rivulet had divided in the plain below, one portion gliding silently into the river behind us, to find its way at last into the Gulf of Mexico, while the other branch had descended in front to join the westward flowing waters of the Columbia, via

[1] Rep't U. S. Geol. Surv. 1872, p. 4. [2] Rep't U. S. Geol. Surv., 1871, p. 132.

Snake River, finally reaching the Pacific Ocean. The true position of this remarkable feature of physical geography is clearly shown on the map which illustrates the report of Capt. Jones.[3] The mountain stream now bears the name proposed by Raynolds — the "Two Ocean Creek"— and its two branches are named respectively, Atlantic and Pacific creeks. Thus is verified another of the stories of that faithful guide and hunter — James Bridger — one of the most worthy of Rocky Mountain pioneers.

Like other peculiarities in the topography of the region, the primary cause of this union of two great river-systems of North America is the ancient volcanic outflow which has deeply buried thousands of square miles of pre-tertiary rocks. Upon either side of the little grassy plain which forms the "divide," hundreds of feet of basaltic beds are piled, and the separation of the streams is effected by very simple means. The present streams traverse valleys of ancient origin, but there are indications of more modern influences which have induced the flow in opposite directions.

The "Two-Ocean Water" is undoubtedly the most remarkable phenomenon of the kind yet observed upon the American continent, but there are other peculiarities in the same region, which are quite similar in origin. The Sweetwater River rises upon the western slope of the Wind River Mountains, and joins the North Platte after cutting across the range at the South Pass, and several minor instances of very interesting water-sheds were observed by the writer in the Uintah Mountains and in the Owl Creek Range and the Sierra Shoshone.

As a rule, these cases afford striking evidences of the local nature of the post-tertiary glacial action throughout a large portion of the Rocky Mountain region. The damming up of streams, the widening of valleys and the accumulation of detritus in the very heart of the mountains, all furnish proof that the ice sheet, though extensive, was mainly confined to the more elevated tracts, where it was productive of very uncommon results in many instances.

[3] Report upon the Reconnaissance of N. W. Wyoming made in the summer of 1873, by Wm. A. Jones, Captain of Engineers. Washington: Government Printing Office, 1874.

On the Post-glacial History of Sequoia gigantea. By John Muir, of San Francisco, Cal.

During the past summer I explored the Sequoia belt of the Sierra Nevada, tracing its boundaries and learning what I could of the post-glacial history of the species, and of its future prospects. Perhaps the most important of the questions put to the forests are as follows: —

What area does Sequoia now occupy as the principal tree? Was the species ever more extensively distributed on the Sierra during post-glacial times? Is the species verging to extinction? And if so, then to what causes will its extinction be due? What have been its relations to climate, to soil, and to other coniferous trees with which it is associated? What are those relations now? What are they likely to be in the future?

Some of the answers obtained to these questions, seem plain and full of significance, and cannot I think, fail to interest every student of natural history. I shall endeavor, therefore, to present them in as clear and compact a shape as possible.

By reference to the map exhibited it will be seen that the Sequoia belt extends from the well known Calaveras groves on the north, to the head of Deer Creek on the south, a distance of about 200 miles. The northern limit being a little above the 38th parallel, the southern a little below the 36th, and the elevation above sea-level varies from about 5000, to 8000 feet.

From the Calaveras to the south fork of King's River, the species occurs only in small isolated groves and patches, so sparsely distributed along the belt, that two gaps occur nearly forty miles in width, one between the Calaveras and Tuolumne groves, the other between those of the Fresno and King's River. Hence southward the trees are nowhere gathered in small sequestered groups, but stretch majestically across the broad and rugged basins of the Kaweah and Tule in noble forests, a distance of nearly seventy miles, and with a width of from three to ten miles; the continuity of the belt being broken here only by deep sheer-walled cañons.

The Fresno group, the largest Sequoia congregation of the north, occupies an area of three or four square miles. From the so-called King's River Grove in the neighborhood of Thomas' Mill, I pushed off in a northeasterly direction, along the bevelled rim of the cañon of the south fork of King's River. Here I discovered a majestic forest of Sequoia, nearly six miles long, by two wide; not hemmed in by pines and firs, but growing as the predominant species, in brave and comfortable independence, over hill, and dale, and rocky ridge-top. This is the northernmost assemblage of Sequoias that may fairly be called a forest.

Descending the precipitous divide between King's River and the Kaweah, we enter the colossal forests of the main continuous portion of the Sequoia belt. As we advance southward, the trees become more and more irrepressibly exuberant, tossing their massive crowns against the sky from every ridge-top, and waving onward in graceful compliance to the complicated topography of the basins of the Kaweah and Tule.

The finest of the Kaweah portion of the belt is located on the broad, lofty ridge separating the waters of Marble Creek from the Middle fork, and extends from the granite headlands overlooking the hot plains, back within a few miles of the cool glacial fountains.

The extreme upper limit of the belt is reached between the Middle and South forks at an elevation of 8,400 above the sea.

The most compact and majestic portion of the Tule forests, lies on the North fork, forming, I think, the finest block of Sequoia in the entire belt.

Southward from here I thought I could detect a slight decrease in the general thrift of the forests; this being the only indication of approach to the southern limit. But shortly after crossing the divide between the basins of the Tule and Deer Creek, the belt is abruptly contracted, and terminated. I made a careful survey of the southern boundary, and of the woods beyond, without discovering a single Sequoia, or any trace of their former existence.

I was greatly interested, however, to find that the species had crossed over from the head of Deer Creek into the valley of the Upper Kern, and planted colonies northward along the eastern slopes of the western summit or Greenhorn Range. The western summit puts out from the main backbone of the range at the head of King's River, trending southward, and enclosing the Upper Kern valley on the west, and it is just where this lofty spur begins to break down, on its approach to its southern termination, that the Sequoia has been able to cross it.

Though the area occupied by the species increases from north to south, there is no corresponding increase in the size of the trees; a diameter of twenty feet, and height of 275, is, perhaps, about the average for full-grown trees; specimens twenty-five feet in diameter are not rare, and a good many approach 300 feet in height. Occasionally one meets a specimen thirty feet in diameter, and rarely one that is larger. The largest I have yet seen and measured is a majestic stump on the south side of King's River. It measures thirty-five feet eight inches in diameter inside the bark four feet from the ground, and a plank thus wide of solid wood could be obtained from it without a decaying fibre.

The main continuous portion of the Sequoia belt, stretching across the basins of the Kaweah and Tule, forms by far the greater portion of the entire coniferous forest, and is plainly visible from the San Joaquin Valley, the light fringe of pines in front not being dense enough to hide it, while it extends so far up the range, there is but little space left for pines or firs above it.

It appears then from this general survey of the Sequoia forests, that notwithstanding the colossal dimensions of the trees, and their peculiarly interesting character, more than ninety per cent. of the whole number of individuals have hitherto remained unknown.

Was the species ever more extensively distributed on the Sierra in post-glacial times?

I have been led to the conclusion that it never was. Because careful search along the margins of the groves, and in the gaps between, fails to discover a single trace of its previous existence beyond its present bounds.

Notwithstanding I feel confident, that if every Sequoia in the range were to die to-day, numerous monuments of their existence would remain, of so imperishable a nature as to be available for the student more than ten thousand years hence.

In the first place we might notice that no species of coniferous tree in the range keeps its individuals so well together as Sequoia; a mile is perhaps the greatest distance of any straggler from the main body, and all of those stragglers that have come under my observation are *young*, instead of old monumental trees, relics of a more extended growth.

Again, we might notice in this connection, the well-known longevity of Sequoia. A tree was felled last summer in the old King's River Grove, whose annual rings, counted by three different persons, numbered from 2125, to 2137; and this specimen was by no means a very aged looking tree, and measured only twenty-three feet in diameter inside the bark, while the giant of the New King's River forest, to which I have already called attention, is nearly twice as large, and probably about twice as old; for it is standing on dry gravelly ground where the growth has been slow, the annual rings measuring only about the one-thirtieth of an inch throughout a considerable portion of the diameter.

Again, Sequoia trunks frequently endure for centuries after they fall. I have a specimen block, cut from a fallen trunk, which is hardly distinguishable from specimens cut from living trees, although the old trunk fragment from which it was derived has lain exposed in the damp forest more than 380 years, probably thrice as long. The time measure, in the case, is simply this. When the ponderous trunk, to which the old vestige belonged, fell, it sunk itself into the ground, thus making a long straight ditch, and in the middle of this ditch, a silver fir is growing, that is now four feet in diameter, and 380 years old, as determined by cutting it half through and counting the rings, thus demonstrating that the remnant of the trunk that made the ditch, has lain on the ground *more* than 380 years. For it is evident that to find the whole time, we must add to the 380 years, the time that the vanished portion of the trunk lay in the ditch before being burned out of the way, plus the time that passed ere the seed from which the monumental fir sprang fell into the prepared soil and took root. Now, because Sequoia trunks are never wholly consumed in one forest fire, and those fires recur only at considerable intervals, and because Sequoia ditches after being cleared are often left unplanted for centuries, it becomes evident that the trunk remnant in question may probably have lain a thousand years or more. And this instance is by no means a rare one.

But admitting that upon those areas supposed to have been once covered with Sequoia every tree may have fallen, and every trunk burned or buried, leaving not a remnant, many of the ditches made by the fall of the ponderous trunks, and the bowls made by their upturning roots, would remain patent for thousands of years after the last vestige of the trunks that made them had vanished. Much of this ditch-writing would no doubt be quickly effaced by the flood-action of overflowing streams, and rain-washing; but no inconsiderable portion would remain enduringly engraved on ridge-tops beyond all such destructive action; for, where all the conditions are favorable, it is almost absolutely imperishable. *Now these historic ditches, and root bowls occur in all the present Sequoia groves and forests, but not the faintest vestige of one presents itself outside of them.*

We therefore conclude that the area covered by Sequoia has not been diminished during the last eight or ten thousand years, and probably not at all in post-glacial times.

Is the species verging to extinction? What are its relations to climate, soil, and associated trees?

All the phenomena bearing on these questions, also throw light, as we shall endeavor to show, upon the peculiar distribution of the species, and sustain the conclusion already arrived at on the question of extension.

In the northern groups there are few young trees or saplings growing up around the failing old ones to perpetuate the race, and inasmuch as those aged Sequoias, so nearly childless, are the only ones commonly known, the species seems doomed to speedy extinction, as being nothing more than an expiring remnant, vanquished in the so-called struggle for life, by pines and firs, that have driven it into its last strongholds, in moist glens where climate is exceptionally favorable. But the language of the majestic continuous forests of the South, creates a very different impression. No tree of all the forest is more enduringly established in concordance with climate and soil. It grows heartily everywhere, on moraines, rocky ledges, along water-courses, and in the deep moist alluvium of meadows; with a multitude of seedlings and saplings crowding up around the aged, seemingly abundantly able to maintain the forest in prime vigor. For every old storm-stricken tree, there is one or more in all the glory of prime; and for each of these, many young trees, and crowds of exuberant saplings. So that if all the trees of any section of the main Sequoia forest were ranged together according to age, a very promising curve would be presented, all the way up from last year's seedlings to giants, and with the young and middle-aged portion of the curve, many times longer than the old portion. Even as far north as the Fresno, I counted 536 saplings and seedlings growing promisingly upon a piece of rough avalanche soil not exceeding two acres in area. This soil bed is about seven years old, and had been seeded almost simultaneously by pines, firs, Libocedrus, and Sequoia; presenting a remarkably simple and instructive illustration of the struggle for life among the rival species; and it was interesting to note that the conditions thus far affecting them have enabled the young Sequoias to gain a marked advantage over all the others.

In every instance like the above, I have observed that the seedling Sequoia is capable of growing on both drier and wetter soil than its rivals, but requires more sunshine than they; the latter fact being clearly shown wherever a sugar pine or fir is growing in close contact with a Sequoia of about equal age and size, and equally exposed to the sun, the branches of the latter are always less leafy. Towards the south, however, where the Sequoia becomes *more* exuberant and numerous, the rival trees become *less* so; and where they mix with Sequoias, they mostly grow up beneath them like slender grasses among stalks of Indian corn. Upon a bed of sandy flood-soil I counted ninety-four Sequoias, from one to twelve feet high, on a patch of ground once occupied by four large sugar pines which lay crumbling beneath them; an instance of conditions which have enabled Sequoia to crowd out the pines.

I also noted eighty-six vigorous saplings upon a piece of fresh ground prepared for their reception by fire. Thus fire, the great destroyer of Sequoia, also furnishes bare virgin ground one of the conditions essential for its growth from the seed. Fresh ground is however furnished in sufficient quantities for the constant renewal of the forests without fire, viz., by the fall of old trees. The soil is thus upturned and mellowed, and many trees are planted for every one that falls. Landslips and floods also give rise to bare virgin ground; and a tree now and then owes its existence to a burrowing wolf or squirrel, but the main supply of fresh soil is furnished by the fall of aged trees.

The climatic changes in progress in the Sierra, bearing on the tenure of tree life, are entirely misapprehended, especially as to the *time*, and the means, employed by nature in effecting them. It is constantly asserted in a vague way, that the Sierra was vastly wetter than now, and that the increasing drouth will of itself extinguish Sequoia, leaving its ground to other trees supposed capable of flourishing in a drier climate. But that Sequoia can and does grow on as dry ground as any of its present rivals, is manifest in a thousand places. "Why then," it will be asked, "are Sequoias always found in *greatest abundance* in well watered places where streams are exceptionally abundant?" Simply because a growth of Sequoias always *creates* those streams. The thirsty mountaineer knows well that in every Sequoia grove he will find running water, but it is a very complete mistake to suppose that the water is the *cause* of the grove being there; for on the contrary, the grove is the entire cause of the *water* being there; drain off the water if possible, and the trees will remain, but cut off the trees, and the streams will vanish. Never was cause more completely mistaken for effect than in the case of these related phenomena of Sequoia woods and perennial streams, and I confess that at first I shared in the blunder.

When attention is called to the method of Sequoia stream-making, it will be apprehended at once. The roots of this immense tree cover the ground, forming a thick continuous, capacious sponge, that absorbs, and holds back the rains and melting snows, only allowing them to ooze and flow gently. Indeed every fallen leaf, and rootlet, as well as long clasping root, and prostrate trunk, may be regarded as dams, hoarding the bounty of storm-clouds, and dispensing it as blessings all through the summer, instead of allowing it to go headlong in short-lived floods. Evaporation is also checked by the dense Sequoia foliage to a greater extent than by any other Sierra tree and the air is entangled in masses and broad sheets, that are quickly saturated; while thirsty winds are not allowed to go sponging and licking along the ground.

So great is the retention of water in many places in the main belt that bogs and meadows are created by the killing of the trees; a single trunk falling across a stream in the woods often forms a dam 200 feet long, and from ten to thirty feet high, giving rise to a pond, which kills the trees within its reach. These dead trees fall in turn, thus making a clearing, while sediments gradually accumulate changing the pond into a bog, or meadow, for a growth of carices and sphagnum. In some instances a chain of small bogs or meadows rise above one another on a hillside, which are gradually merged into one another, forming sloping bogs or meadows which form very striking features of Sequoia woods, and since all the trees that have fallen into them have been preserved, they contain records of the generations that have passed since they began to form.

Since then it is a fact that thousands of Sequoias are growing thriftily on what is termed dry ground, and even clinging like mountain pines to rifts in granite precipices; and since it has also been shown that the extra moisture found in connection with the denser growths is an *effect* of their presence, instead of a *cause* of their presence; then the notions as to the former extension of the species, and its near approach to extinction, based upon its supposed dependence on greater moisture are seen to be erroneous.

The decrease in the rain and snowfall since the close of the glacial epoch in the Sierra is much less than is commonly guessed. The highest post-glacial water-marks are well preserved in all the upper river channels, and they are not greatly higher than the spring floodmarks of the present; showing conclusively that no extraordinary decrease has taken place in the volume of post-glacial Sierra streams since they came into existence. But in the meantime eliminating all this complicated question of climatic change, the plain fact remains; that *the present rain and snowfall is abundantly sufficient for the luxuriant growth of Sequoia forests.* Indeed all my observations tend to show that in case of prolonged drouth, the sugar pines and firs would die before Sequoia, not alone because of the greater longevity of individual trees, but because the species can endure more actual drouth, and make the most of whatever moisture falls. Only a few of the very densest fir and pine woods felt and weave a root-sponge sufficiently thick and extensive for the maintenance of perennial springs, while *every* Sequoia grove does.

Again, if the restriction and irregular distribution of the species be interpreted as a result of the desiccation of the range, then instead of increasing as it does in individuals toward the south where the rainfall is less, it should diminish.

If then the peculiar distribution of Sequoia *has not* been governed by superior conditions of soil as to fertility or moisture, by what *has* it been governed?

Several years ago I observed that the northern groves, the only ones I was then acquainted with, were located on just those portions of the general forest soil-belt that were first laid bare towards the close of the glacial period when the ice-sheet began to break up into individual glaciers. And last summer while searching the wide basin of the San Joaquin, and trying to account for the absence of Sequoia where every condition seemed favorable for its growth, it occurred to me that this remarkable gap in the Sequoia belt is located exactly in the pathway of the vast *mer de glace* of the San Joaquin and King's River basins, which poured its frozen floods to the plain, fed by the snows that fell on more than fifty miles of the summit. I then perceived that the other great gap in the belt, forty miles wide, extending between the Calaveras and Tuolumne groves, occurs exactly in the pathway of the great *mer de glace* of the Tuolumne and Stanislaus basins, and that the smaller gap between the Merced and Mariposa groves, occurs in the pathway of the smaller glacier of the Merced. *The wider the ancient glacier, the wider the corresponding gap in the Sequoia belt.*

Finally, pursuing my investigations across the basins of the Kaweah and Tule, I discovered that the Sequoia belt attained its greatest development, just where, owing to the topographical peculiarities of the region, the ground had been most perfectly protected from the main ice-rivers, that continued to pour past from the summit fountains, long after the smaller local glaciers had been melted.

Beginning at the south, the majestic, ancient glaciers are seen to have been shed off right and left down the valleys of Kern and King's Rivers, by the lofty protective spurs outspread embracingly above the warm Sequoia-filled basins of the Kaweah and Tule. Then next northward comes the wide Sequoia-less channel of the ancient San Joaquin and King's River *mer de glace.* Then the warm, protected spots of Fresno and Mariposa groves. Then the Sequoia-less channel of the ancient Merced glacier. Next the warm, sheltered ground of the Merced and Tuolumne groves. Then the Sequoia-less channel of the grand ancient *mer de glace* of the Tuolumne and Stanislaus; and lastly the warm, old ground of the Calaveras groves.

What the other conditions may have been that enabled Sequoia to establish itself upon these oldest and warmest portions of the main glacial soil-belt, I cannot say. I might venture to state, however, in this connection, that since the Sequoia forests present a more and more ancient aspect as they extend southward, I am inclined to think that the species was distributed from the south. While the sugar pine, its great rival in the northern groves, seems to have come around the head of the Sacramento valley and down the Sierra from the north. Consequently when the Sierra soil-beds were first thrown open to preëmption on the melting of the ice-sheet, Sequoia may have established itself along the available portions of the south half of the range, prior to the arrival of the sugar pine; while the sugar pine took possession of the north half, prior to the arrival of Sequoia.

But, however much uncertainty may attach to this branch of the question, there are no obscuring shadows upon the grand general relationship we have pointed out between the present distribution of Sequoia and the ancient glaciers of the Sierra. And when we distinctly bear in mind the great radical fact, that *all* the present forests of the Sierra are young, growing on moraine soil recently deposited, and that the flank of the range itself, with all its land-scapes is new-born, recently sculptured and brought to the light of day from beneath the ice mantle of the glacial winter, then a thousand lawless mysteries disappear, and broad harmonies take their places.

But although all the observed phenomena bearing on the post-glacial history of this colossal tree point to the conclusion that it never was more widely distributed on the Sierra since the close of the glacial epoch — that its present forests are scarcely past prime, if indeed they have reached prime — that the post-glacial day of the species is not half done, yet, when from a wider outlook the vast antiquity of the genus is considered, and its ancient richness in species and individuals; comparing our Sierra Giant and *Sequoia sempervirens* of the coast, the only other living species, with the twelve fossil species already discovered, and described by Heer and Lesquereux, some of which seem to have flourished over vast areas around the polar zone, and in Europe, and our own territories, during tertiary and cretaceous times,— then indeed it becomes plain that our two surviving species, restricted to narrow belts within the limits of California, are mere remnants of the genus, both as to species, and individuals; and that they probably are verging to extinction. But the verge of a period beginning in cretaceous times, may have a breadth of tens of thousands of years, not to mention the possible existence of conditions calculated to multiply and reëxtend both species and individuals. This, however, is a branch of the question beyond the present discussion.

In studying the fate of our forest king, we have thus far considered the action of purely natural causes only; but unfortunately *man* is in the woods, and waste and pure destruction are already making rapid headway. If the importance of forests were at all understood, even from an economic standpoint, their preservation would call forth the most watchful attention of government. In the meantime however, scarce anything definite is known regarding them, and the simplest ground-work for available legislation is not yet laid, while every species of destruction is moving on with accelerated speed.

In the course of last year's explorations I have found no less than five mills located on, or near, the lower edge of the Sequoia belt, all of which saw more or less of the "big tree" into lumber. One of these, located on the north fork of the Kaweah, cut over 2,000,000 feet of big tree lumber last season. Most of the Fresno group are doomed to feed the mills recently erected near them, and a company has been formed to cut the magnificent forest on King's River. In these milling operations waste far exceeds use; for after the choice young manageable trees on any given spot have been felled, the woods are fired to clear the ground of limbs and refuse with reference to further operations, and of course most of the seedlings and saplings are destroyed.

These mill ravages however, are small as compared with the comprehensive destruction caused by "Sheepmen." Incredible numbers of sheep are driven to the mountain pastures every summer, and their course is ever marked by desolation. Every wild botanic garden is trodden down, the shrubs are stripped of leaves as if devoured by locusts, and the woods are burned. Running fires are set everywhere, with a view to clearing the ground of prostrate trunks, to facilitate the movements of the flocks, and improve the pastures. The entire forest belt is thus swept and devastated from one extremity of the range to the other, and with the exception of the resinous *Pinus contorta*, Sequoia suffers most of all. Indians burn off the underbrush in certain localities to facilitate deer-hunting. Mountaineers carelessly allow their camp-fires to run, so do lumbermen, but the fires of the sheep-men or *Muttoneers*, form more than ninety per cent. of all destructive fires that range the Sierra forests.

Some years ago a law was enacted by the California legislature with special reference to the preservation of *Sequoia gigantea*, under which the cutting down of trees over sixteen feet in diameter became illegal, but on the whole, a more absurd and short-sighted piece of legislation could not be conceived; for all the young trees on which the permanence of the forest depend, may be either burned or cut with impunity, while the old trees may also be burned provided only they are not cut!

It appears, therefore, that notwithstanding our forest king might live on gloriously in Nature's keeping, it is rapidly vanishing before the fire and steel of man; and unless protective measures be speedily invented and applied, in a few decades at the farthest, all that will be left of *Sequoia gigantea* will be a few hacked and scarred monuments.

On Self-fertilization and Cross Fertilization in Flowers. By Thomas Meehan, of Germantown, Pa.

The author refers to a paper of last year in which he questioned the correctness of the growing impression that all plants with color, fragrance, or honeyed secretions, required or were benefited by cross fertilization. In this paper he takes positive ground.

1st. That cross fertilization by insect agency does not exist to the extent claimed for it.

2d. Where it does exist there is no evidence that it is of any material benefit to the race. On the contrary:—

3d. Difficulties in self-fertilization result from physiological disturbances that have no relation to the general welfare of plants or species.

[The paper gives details of experiments and observations with various plants in proof of the above propositions, and has been published in full in the "Penn Monthly" for Nov., 1876.]

Graft Hybrids. By Thomas Meehan, of Germantown, Pa.

Of late years an impression has prevailed that hybrids may be obtained by grafting as well as by seeds. Sachs makes no mention of this in his text book; but it has had a place in the literature of the horticulturist for over a hundred years. Bradley says that a variegated Jasmine, grafted on a common green stock, infused the variegation throughout the whole plant; and there is an idea among some horticulturists that an intermixture in apples can be obtained by uniting two halves of different buds, and grafting them together. Thousands of people have laughed at these notions. No one has tried them. But only a few years ago it was found Bradley was right; and we have in cultivation new variegated forms of *Abutilon* as well as some other things originated by the graft process. During the few past years it has been asserted that new varieties of potatoes have originated in this way. A tuber is taken and all the eyes cut out. A wedge with the eye of another kind is then inserted into the eyeless mass, and planted. The results are said to be true hybrids. Many of our best physiologists doubt this. I have not seen these cases; but I must say that the evidence offered is much stronger than much of that on which some popular theories have been built.

I tried the split bud grafting process not believing it would result in hybridity. I merely wished to test the popular notion. I am pleased to be able to say now that it is correct. New varieties can be obtained in that way. I took Rhode Island Greening and Red Astrachan; two very distinct varieties of apples in every respect. The grafts with a single bud were split as near through the centre as possible, and a piece of each kind fitted together so as to appear one complete scion. Twelve of these were grafted. Three grew. Two of these have fruited; neither are Rhode Island Greening and the two are unlike each other. One of these has a flower like the Rhode Island Greening; and the flower of Red Astrachan is rosy and in many ways distinct from the large white one of Rhode Island Greening; but the fruit is in many respects similar to that of Red Astrachan. The second variety has the flower similar to that of Rhode Island Greening, and the fruit somewhat the color of Red Astrachan, ripening about the same time, but is but half the size, very much flattened, and with a slender stem, near two inches long, and as much like that of a

Siberian Crab as can be. There is no doubt but two varieties distinct from their parents, and distinct from each other, have resulted from this graft process.

Some may suppose that the union of a Red Astrachan and a Rhode Island Greening Apple, should result in producing an exact intermediate; and that the union of buds in the several graft cases, should each produce identically the same; and therefore the two distinct varieties from the same process be a surprise. But no two children of the same parents are exactly the same; and this is the experience of plant hybridists. Our fellow member Mr. W. Saunders of London, Ontario, crossed the American gooseberry *Ribes cynobasti* with the Red Warrington an English variety, but both with hairy fruit. The hybrid product has smooth fruit, thus introducing a character not extant in either parent. And as regards variety I have, myself, from a single berry of a cross fertilized Fuchsia, produced several score of plants among which no two were alike. I do not know that there is any pomological value in the new varieties of apples I have raised, but I am delighted with the scientific results; proving that the idea of hybrids by bud grafting is more than a popular delusion.

A PRELIMINARY NOTE ON MENOPOMA ALLEGHANIENSE OF HARLAN.

By AUG. R. GROTE, of Buffalo, N. Y.

I HAVE been able to examine nearly one hundred specimens of the *Menopoma alleghaniense*, taken during the months of July and August in the Alleghany River at Olean, N. Y. The results of my study of the animal at the present time lead me to unite the more reddish, unicolorous specimens, which have been described by Holbrook as a distinct species under the name of *fuscum*, with the spotted specimens from which the original description seems to have been drawn, so that I believe we have one species from the tributaries of the Mississippi and not two. Between the two extremes, which are readily picked out, there seems to be all possible grades and from the same locality.

I have also to record the fact that the animal sheds a transparent membrane which I believe is the exterior layer of the skin. While observing this fact in the aquaria of the Buffalo Society of Natural Sciences, Prof. S. W. Garman and myself were able to find an almost complete skin, all the feet and the toes being readily perceivable as we floated and unfolded it in the water. This skin was observed at first gathered in the mouth of the animal which was apparently in the act of swallowing it. The verification of this last observation would be interesting, since a similar habit has been observed in the case of the common toad.[1] All individuals of the *Menopoma*, that I have observed during the two months of July and August in aquaria, have an intermittent swaying motion from side to side. While I have not been able to verify the conjecture, this movement of the body may be connected with the effort of the animal to cast its skin. On the other hand it may be a movement to attract the sexes, or connected with the breeding period. In *Dactylethra* and *Cyclorhamphus* Prof. S. W. Garman has observed a similar shedding of the skin. We may predict that the same thing occurs in the other more exclusively aquatic forms such as *Necturus tetradactylus* (= *Menobranchus lateralis* of Authors), *Amphiuma* and *Siren;*[2] as also in the forms that take to the land, as *Amblystoma*, *Plethodon*, *Desmognathus*, and *Demyctylus;* and then in *Megalobatrachus* of Japan.

I wish also to record the important fact that eggs are deposited by the *Menopoma* in August and September. Females opened on Aug. 21, contained well developed eggs attached by a membrane to the ovary. (I exhibit eggs taken from the aquarium this morning, Aug. 30th.) The yolk, which is about the size of a pea, is seen floating in a glary fluid enveloped in a membrane similar to that containing the albumen in a bird's egg inside the shell. The eggs are laid in strings, connected, and impregnation probably

[1] The following observation has been subsequently made by me on a specimen in the aquarium of the Buffalo Society of Natural Sciences. The wide mouth is opened several times to its fullest extent, by which means the skin is parted on the lips, and then rolls backward over the head. Before this, the transparent pellicle was observed to be loosely surrounding the surface of the animal from which it had separated. By short, jerky movements the *Menopoma* then withdrew its front legs from the old skin. The animal next moved in a forward direction, withdrawing itself from the skin, which was shoved back by the water until the skin was folded against the hind legs. The *Menopoma* then turned shortly round on itself, and, taking the skin in its mouth, drew it over the hind legs and tail. The skin was retained in the mouth and subsequently swallowed. The whole operation was quickly performed.

[2] I have since observed it in *Sperlerpes salmoneus*, from the Niagara River.

occurs at the time of their extrusion. After they are laid they increase in size by the absorption of water.[3]

The *Menopoma* probably frequents the muddy banks of the river to oviposit. In external appearance there is at this time a change, and we may say that the animal puts on its "marriage dress." The tail broadens, as also slightly the plaited extension of the skin along the sides of the body. There is, however, an individual variation in the extent of the lateral folds of the skin. In its habits the *Menopoma* appears to be nocturnal.

I have presented these facts in the biology of the *Menopoma* before this Association, since I believe they have not been previously known.

On the Brains of some Fish-like Vertebrates. By Burt G. Wilder, of Ithaca, N. Y.

[ABSTRACT.]

Specimens, preparations, drawings, or diagrams were shown of the brains of the following forms: *Myxine*, *Bdellostoma*, *Petromyzon*, *Mustelus*, *Acanthias*, *Carcharias*, *Chimæra*, *Amia*, *Lepidosteus*, *Salmo*, *Megalops*, and *Menobranchus*.

Attention was called to the following points:—

1. The great diversity of brain-structure among "fishes."
2. The fact that the brains of *Myxine* and *Bdellostoma* have not been satisfactorily homologized with that of *Petromyzon*.
3. That certain features of the brains of *Myxine* and *Bdellostoma* indicate that they are *retrograded* forms; this renders a knowledge of their embryology very desirable.
4. The close resemblance of the brains of *Petromyzon* and *Menobranchus*.[1]
5. That the anterior median mass of the Plagiostome brain is

[3] These summer eggs proved afterwards to be unfertile. A string of them was noticed to be swallowed by a male *Menopoma*. The spring of the year will probably prove the usual time of oviposition.

[1] The similarity of their skulls has been pointed out by Huxley, 20.

not developed as described by Huxley (20), and by the author (19, 183), but in the manner described and figured by the author in August, 1876 (21).[2]

6. That the homologies of the parts formed from the anterior cerebral vesicle of the embryo are not yet determined for the fish-like vertebrates.

7. That the brain of *Chimæra* resembles that of sharks as to its medulla, cerebellum, and optic lobes; that the anterior region resembles the anterior region of Ganoids in some respects; but that the brain as a whole presents peculiarities entitling the Holocephala to rank as a primary division separate from the Plagiostomes and Ganoids.[3]

8. The rudimentary lateral ventricles in the olfactory lobes, which were described by the author (19, 181 and 184) as existing in Ganoids and in several Teleosts, have since been found by him in *Salmo confinis* (the lake trout), and in *Megalops thrissoides* (the "tarpum").[4]

9. The new interpretation of the fish-brain by Maclay, which has been assented to by Gegenbaur, is contravened by the general condition of the parts during development; by the connections of the optic nerves; and by the origin of the fourth pair (patheticus, or trochlearis) behind the optic lobes in all vertebrates where it has been found at all.[5]

10. The differences between the brains of Ganoids and Teleosts are few and unimportant compared with those between the brains of Ganoids and Plagiostomes. The Teleosts, as to their brains,

[2] The references are to a list of works on page 266.

[3] A description with figures, of the brain of *Chimæra* is nearly ready for publication. It is proper to state that the author's conclusion respecting the relations of the Holocephala were reached from a study of the brain, and before seeing the paper on *Ceratodus* in which Prof. Huxley expresses a similar opinion based upon the consideration of the skull, pectoral fins and heart.

[4] In the plate, figs. 13, 14, are represented those parts in *Salmo confinis* and *Lepidosteus osseus*. As stated in the paper just referred to (19, 169) these cavities were fairly figured by Stannius as they appear in the sturgeon, although neither he nor later authors appear to have mentioned them. Since this paper was presented I have been enabled to examine Maclay's more extended work (18 taf. vii) and find that these lateral olfactory ventricles are plainly shown in his figures of the brains of *Amia* and of four species of *Acipenser;* but they are neither lettered on the plate, nor referred to in the text. The figure of the brain of *Lepidosteus* does not show them. For the head of *Megalops* I am indebted to Mr. E. G. Blackford.

[5] Johannes Müller (9, 215) first considered the origin of this nerve as a test of the homology of the lobes; but the point was more distinctly brought out by Prof. Huxley in some remarks upon the present paper at the session of Friday, Aug. 25, 1877.

may be regarded as a specialized offshoot, with a relation to the Ganoids similar to that which Birds bear to Reptiles.[6]

11. Errors of fact and of interpretation respecting the brains of the fish-like vertebrates are very common even in manuals and compendiums of the highest authority. It is desirable to reëxamine the structure and development of the brains of the various types as thoroughly as their skulls are now being revised by W. K. Parker and others.

On the Serrated Appendages of the Throat of Amia. By Burt G. Wilder, of Ithaca, N. Y.

Upon each side of the *copula*, or isthmus, which connects the shoulder-girdle of *Amia*,[1] with the hyoid arch, there are two appendages which are rarely mentioned by authors, and whose nature appears to be undetermined.

Their position is indicated in fig. 7; while figures 8–11 represent their appearance in the young and adult.

Historical Sketch. According to Dumeril (7, II, 410), these appendages are what "Linnæus referred to (1, I, 500) in the following phrase to which zoölogists who have spoken of *Amia* (Lacépede 2, V, 43, Bloch, 5,451, etc.) do not appear to have attached a definite significance. *Gula ossiculis* 2, *scutiformibus*, *e centro striatis.* Valenciennes (3, XIX, 415) supposed that he was impressed by the appearance of the branchiostegal rays which form, on each side, a sort of striated plate; but in the phrase cited reference is evidently made to the two small dentated pieces of

[6] A like conclusion has been reached by Parker from his study of the skull of the salmon (22).

[1] *Amia* is a fish found living in the Mississippi River and its tributaries, and in the great lakes. It attains a length of two feet, and is called by fishermen "mud-fish," "dog-fish," and "lake-lawyer." Under the tip of the lower jaw is a movable plate which does not exist in any other fresh-water fish of America. The adult male has a circular dark spot at the base of the tail (Jordan 23, 306). *Amia* is now usually regarded as a Ganoid, and its brain closely resembles that of *Lepidosteus* (the gar-pike); but it seems to be, as remarked by Gill (10), the "most teleosteoid" of that group.

which I am speaking, and which it is easy to see. I have also found them mentioned by Stannius (4, 88, note 6)."

In another part of the same paragraph, Dumeril describes them as follows: "The middle portion of the hyoid bone or *copula* bears, at its posterior extremity, beyond the inferior pharyngeals which constitute, after a fashion, a fifth pair of branchial arches, two small osseous plates, covered with short and fine teeth, quincuncially arranged."

It will be seen that the above description is not altogether correct. There is no osseous connection between the shoulder-girdle and the hyoid arch. The anterior plate occupies very nearly the middle of the isthmus. It is not small in comparison with the isthmus itself, and the posterior plate is twice as large. The "teeth" are not borne directly on the plates but on ridges; and neither the teeth nor the ridges are "quincuncially arranged." Dumeril does not state whether he has examined them himself.

The appendages are not mentioned in Franque's description of *Amia* (6), nor in the monographs or systematic works of Agassiz, Cuvier, Cope, DeKay, Gill, Günther, Huxley, Jordan, Müller, Owen, Rolleston, or Vogt.

I am not acquainted with any figure or detailed description of the appendages under consideration, and now offer the following imperfect account in the hope of calling attention to what seems to be a constant and almost peculiar feature of the *Amia*.[2]

Location and general appearance. In the adult *Amia* there are two appendages on each side. They are usually concealed from view by the operculum; but the tip of the hinder one sometimes projects beyond the operculum at a point a little above the base of the pectoral fin. The anterior appendage (fig. 10, *a*) is about 2 cm. long, and its anterior extremity is a little more than half its length from the union of the isthmus with the hyoid arch. Its hinder end is nearly opposite the median tip of the shoulder-girdle. It is wholly superficial, and its hinder border projects but slightly beyond its attachment. The posterior appendage (*h*) is about twice the length of the anterior and consists of three portions; a short triangular *root* just beneath the skin; a short but broad *base*, the deep surface of which is continuous with the skin; a

[2] At the time this paper was presented I supposed *Amia* alone to possess these appendages. I have since found in *Lepidosteus* what seem to be less developed representatives of the same parts. They will be referred to farther on.

long *free* portion, which gradually tapers backward to the tip which is less than 1 mm. wide. The root lies to the mesial side of the posterior extremity of the anterior appendage, but there is a distance of nearly 2 mm. between them. The posterior appendage inclines dorsad, and rests quite closely against the adjacent surface of the shoulder girdle.

Neither has any direct connection with bone. The attached surfaces rest upon the muscles which constitute the isthmus, but do not appear to be attached to them. While observing living Amias with reference to their respiratory function (19, 151) I never saw any movement of the appendages. The thickness of the posterior one is about ½ mm. It is quite flexible during life and while moist, but becomes more rigid when dried.

The free surfaces of both appendages are corrugated in the adult, as seen in fig. 11. The general direction of the ridges and furrows is across the length of the surfaces obliquely forward from the dorsal toward the ventral border. The ridges are more or less wavy in outline, and present irregularities of direction and arrangement, especially toward the tip and ventral border of the posterior appendage. But the distance between any two ridges is quite uniform; the number of ridges being about eighteen to the centimeter upon the anterior appendage, and about twelve upon the posterior. The transverse ridges do not always reach the ventral border upon the anterior two-thirds of the posterior appendage; the ventral third of the surfaces is in some cases nearly free, but may present one or more ridges running nearly parallel with the border, or more often, especially on the inner surface, there may be a series of short ridges trending dorsad and *forward* from the lower border to meet the dorsal series at open angles.

The anterior slopes of the ridges form an angle of about 45° with the surface; but the posterior slopes are nearly perpendicular. The crests are projected backward as numerous fine teeth which are barely visible to the naked eye, but under a low magnifying power present the appearance shown in figure 11.

Development. In the smallest *Amia* examined by me,[3] 5 cm. (about 2 inches) long, I find no trace of the anterior appendage. The posterior is shown in figs. 7 and 8. It is about 3 mm.

[3] For this, and for the other young Amias I am indebted to Professors W. S. Barnard, S. A. Forbes, and H. A. Ward.

long and presents upon the outer surface five ridges which are much longer in proportion to the whole appendage than in the adult. The serrations upon the free borders of the ridges are relatively large and few as seen in fig. 8; they are sharp papillæ rather than mere denticulations.

In a specimen 77 mm. (about 3 inches) long, the posterior appendage is nearly 5 mm. long and presents twelve ridges upon its outer surface (see fig. 9). The anterior appears as a series of eight soft and rounded oblique ridges. But in another specimen which is thicker but only 70 mm. long, the appendage is only 4 mm. long, and bears eight ridges. The anterior however is less well defined than in the larger example. In the largest young *Amia* in my possession, 10 cm. (about 4 inches) long, the posterior is 7 mm. long and presents fourteen ridges. The anterior is still soft and ill defined.

In none of these young examples does the tip of the posterior appendage reach the margin of the operculum; and a comparison of the lengths of the whole body and of the appendage shows that the latter increases not only absolutely but relatively. In the smallest the ratio is as 6 to 100; in the third it is 6½, and in the fourth 7. In an example 382 mm. long the appendage is 33 mm. long, or in the proportion of about 8⅗ to 100.

But there seems to be a limit to this increase, for in an example 475 mm. long the appendage is 38 mm., a ratio of 8 to 100.

Structure. The appendages consist mainly of fibers running longitudinally. I have not yet examined them under the microscope.

The anterior appendage is a single lamina, as is also the inserted root of the posterior. But the remainder of the posterior consists of two laminæ.

When cut transversely near the base the inner lamina is seen to be about two-thirds as wide as the outer, upon which it is applied so that the dorsal borders coincide, while the ventral line of union corresponds with the point of union already mentioned (page 261) between the regular series of ridges and those along the ventral border. The laminæ separate as if by sutures along both these lines of union. Between them is a compressed space through which runs what appears to be a nerve. But I have not yet traced its source, or ascertained the vascular supply of the appendages.

Supposed homologous parts in Lepidosteus. At the time this paper was presented I supposed that no other fish possessed parts comparable with these appendages of *Amia.*

A recent more careful examination of the hyoid isthmus of *Lepidosteus* (the "gar-pike") leads me to think that homologous parts exist in this genus. In several adult examples of *Lepidosteus platystomus*, and *osseus*, there are, upon each side of the isthmus, from two to five dermal bony plates forming a single series. The hindermost plate is usually the larger, but it never projects backward as in *Amia.* The free surface presents ridges and furrows the general direction of which is transverse. The number of plates is not constant in different examples or on the two sides, and sometimes two seem to be united into one. But their invariable presence, and their arrangement as a single and definite series suggests their homology with the appendages of *Amia.* Traces of them exist in a young *L. osseus* about 25 cm. (about 10 inches) long, but I have found none in examples 15 cm. long or in smaller ones.

Function. I am not aware that any use has been assigned to these appendages, and I have no suggestion to offer. The anterior is evidently passive. The posterior, even if voluntarily movable by the fish, is too flexible for offence, and is, moreover, covered by the operculum.

Morphological significance. Unless some function can be assigned to these appendages the conclusion that most naturally suggests itself is that they are remnants of organs which had greater size and performed some function in more or less remote ancestors of *Amia.* The position and general appearance of the posterior pair are not wholly contradictory of the idea that they may have been accessory branchiæ; but this could hardly be surmised respecting the anterior pair, or the supposed homologous parts of *Lepidosteus.*

The appendages should be examined in fossil *Amia* and *Lepidosteus*, and in other extinct Ganoids; likewise should careful search for them be made in all living Ganoids, and in the teleostean genera *Elops* and *Megalops* which possess some points of resemblance to *Amia.*

ON THE TAIL OF AMIA. By BURT G. WILDER, of Ithaca, N. Y.

[ABSTRACT.]

THE tail of *Amia* has been figured and described by Franque (6), Kölliker (13, p. 6, fig. 2), and Huxley (12, 20, fig. 6).

Kölliker's paper is known to me only through the quotations by Dumeril (7, 401). Franque represents only the osseous portions of the skeleton. Huxley gives both form and structure but not as it seems to me, quite accurately. Neither of these authors mentions the young *Amia*, or intimates that the form or structure of the tail may vary with age.

External form. Dumeril (7, 399, note 1) says that "the tail of *Amia*, as to its external appearance, differs in no way from that of the ordinary osseous fishes. Its heterocercy, however decided, is well manifested only by the skeleton." Huxley does not allude to the form but his figure does not very distinctly indicate any difference between the tail of *Amia*, and that for instance of some Siluroids, where the whole is rounded, and the greatest length is midway between the dorsal and ventral borders.

I have examined many examples of *Amia*, young and adult, and all manifested the following features. 1. The greatest length of the tail is considerably above the middle of its height.

2. The change from the nearly horizontal dorsal and ventral borders to the curved posterior border occurs farther forward upon the ventral side. These features render the ventral slope both longer and more gradual than the dorsal.

3. When the tail is fully expanded, as while the fish is swimming, the dorsal and ventral slopes meet so as to form a gentle curve and not an obtuse point as in Huxley's figure. This is well shown in figure 3, representing the tails of a young example in the condition assumed at death.[1]

The tail of *Lepidosteus* presents the same general features with some specific variation. Hence with both these ganoid genera the external form of the tail is decidedly, though not very obviously, unequal.

Structure. The terminal caudal vertebræ form an upward curve as shown by Franque. Huxley's figure and description (12,

[1] The tail represented in fig. 4 is slightly mutilated.

20) show that the notochord, enveloped by cartilage,[2] extends upward toward the dorsal border of the tail. In all the adults examined by me the termination of this compound rod is considerably nearer the dorsal border than is indicated by Huxley's figure, and presents a rather broad and but slightly rounded tip, with a central depression corresponding to the neural or spinal canal; see figs. 5 and 6. Here ends the distinct cartilage. Posterior to it, and between the two laminæ of the 21st or 22nd fin-ray (counting from below) is a tract of gelatinous matter which Kölliker (13, 6) as quoted by Dumeril (7, 401), seems to have regarded as the prolongation of the notochord. I have been unable to detect any difference between this, and the tracts of gelatinous matter between the laminæ of the other caudal fin-rays.

But that it may fairly be regarded as the prolongation of the notochord, degenerated, and not enveloped by a cartilaginous sheath, is rendered at least probable by the following considerations.

1. The condition of things in the adult *Lepidosteus* as described and figured by Kölliker (13, 8–11) and myself (19, 161); the notochord with its cartilaginous sheath forming a slender tapering rod extending between the halves of fin-rays to the junction of the middle and hinder thirds of the tail.

2. The existence of an undulation of the dorsal border of the tail of *Amia* corresponding with the termination of the supposed notochord.

3. The greater distinctness of this undulation in young individuals.

Transformations. In the Proceedings of this Association for 1875 (19), I described and figured the stages of transformation of the tail of *Lepidosteus*, showing that it is at first *protocercal*, nearly as in *Amphioxus* and *Petromyzon;* then markedly *heterocercal* as in various genera of sharks and sturgeons; and that finally, by the gradual loss of the "filament" the whole tail is apparently constituted by the infracaudal lobe. On page 162 I ventured to express the belief that the tail of *Amia* would be found to undergo similar transformations. The form and structure of the tails of the young individuals here figured encourage me in that antici-

[2] In my previous paper (19, 101) it is said that the "cartilaginous rod is called notochord by Huxley;" most unaccountably his explicit statement in another paragraph, respecting the fibrous or cartilaginous sheath, was overlooked by me at the time.

pation; the thickened anterior portion of the dorsal border ceases quite abruptly as if a filament had there been separated, or absorbed. But to determine the fact it is necessary to procure Amias just hatched from the egg, and to observe them until about 50 mm. long. The changes probably occur earlier, or more rapidly, than in *Lepidosteus.*

The importance of such information respecting the development of this Ganoid will be made apparent by the facts and considerations presented by Huxley (11, 12 and 16).

Variation in the shape of the tail. I am, at present, unable to determine the relation of the differences presented by figures 1–5 to the age or sex of the examples. They may be merely individual peculiarities. But the difference in form and color of the whole body between examples from two localities suggests the possible necessity of recognizing well-marked varieties, or even species.

In conclusion I call attention to the markings upon the head of the youngest *Amia.* The stripe from the snout across the eye, and to the dorsal angle of the operculum, is also well-marked in the young *Lepidosteus* and in the young *Menobranchus.* Both stripes are visible, though less distinct, in the Amias 70 and 100 mm. long.

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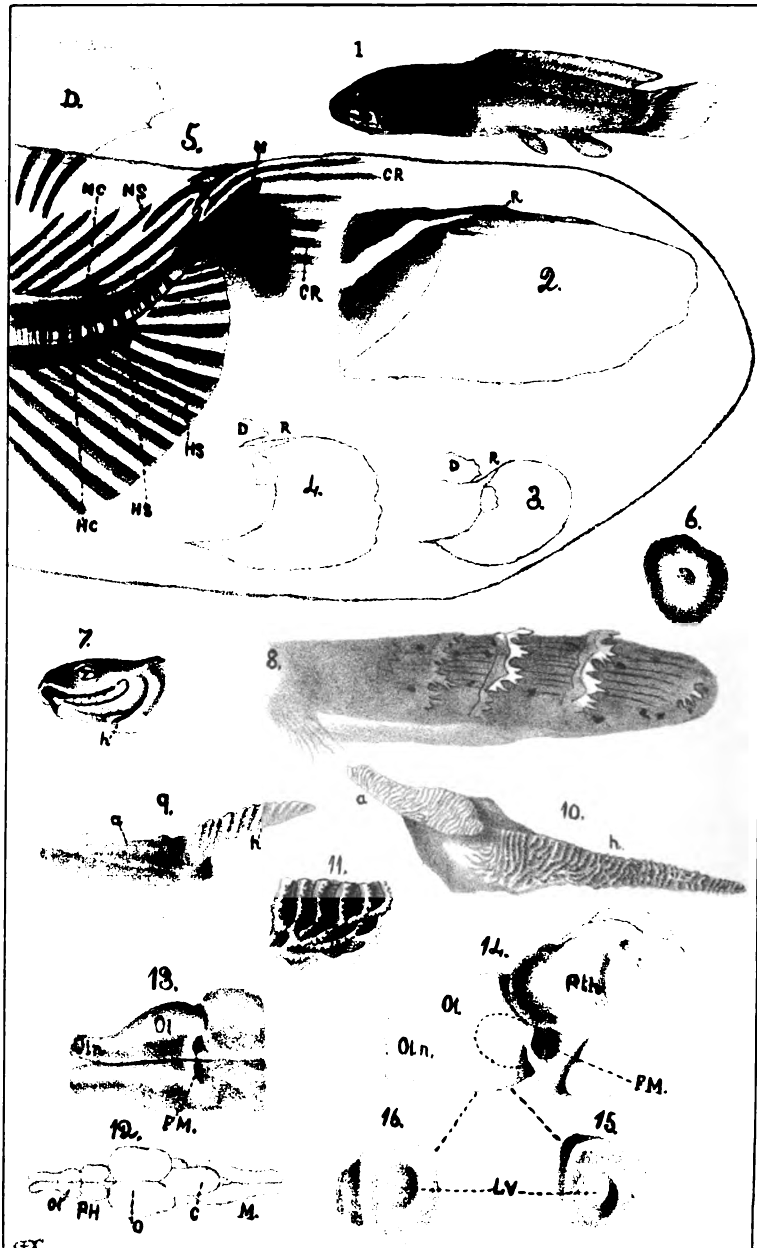

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EXPLANATION OF PLATE.

Fig. 1. Young *Amia*, natural size.

Fig. 2. Tail of young *Amia* 50 mm. long, enlarged; partly dissected.

Fig. 3. Tail of young *Amia* 77mm. long, natural size.

Fig. 4. Tail of young *Amia* 10cm. long, natural size. In figs. 2, 3, and 4, D indicates the hinder end of the dorsal fin, and R the thickened dorsal border of the tail, which may be the remnant of a caudal filament.

Fig. 5. Tail, partly dissected, of adult *Amia*, natural size. *NC*, neural canal laid open. *HC*, hæmal canal. *HS*, hæmal sinus. *NS*, neural spines. *HS*, hæmal spines. *N*, notochord, with its cartilaginous sheath, laid open so as to expose the neural canal. *S*, spot at the base of the tail. *CR*, some of the caudal fin rays, the rest being omitted from the figure.

Fig. 6. Tip of notochord with its cartilaginous sheath showing the depression corresponding to the neural canal; enlarged 8 diameters.

Fig. 7. Head of young *Amia* 55mm. long, natural size. The left operculum is removed so as to show the gills, and the posterior serrated appendage (*p*) upon the throat; the anterior is not yet developed.

Fig. 8. Posterior appendage of young *Amia* 50mm. long, enlarged 22½ diameters.

Fig. 9. Anterior (*a*) and posterior (*h*) appendages of young *Amia* 77mm. long, enlarged 3 diameters.

Fig. 10. Appendages of adult *Amia*, left side, natural size.

Fig. 11. Part of outer surface of posterior appendage of adult *Amia*, enlarged 2 diameters.

Fig. 12. Brain of *Salmo confinis* (lake trout) from above, natural size. *M*, medulla oblongata. *C*, cerebellum. *O*, optic lobes. *Pth*, prothalami, usually called hemispheres. *Ol*, olfactory lobes.

Fig. 13. Anterior region of the same, enlarged 4 diameters. The dorsal portion of the left prothalamus has been removed so as to show the shallow ventricles (*FM*) in the bases of the olfactory lobes.

Fig. 14. Anterior region of brain of *Lepidosteus osseus* (gar-pike), the left lobes removed so as to show the mesial surface of the right; enlarged 4 diameters. *FM*, the foramen of Monro leading into the lateral ventricle, the extent of which is indicated by the dotted line.

Fig. 15. Vertical transverse section of the right olfactory lobe of the same brain through the middle of the length of the ventricle.

Fig. 16. Section of the same just behind the rounded anterior extremity of the ventricle.

A BRIEF COMPARISON OF THE BUTTERFLY FAUNAS OF EUROPE AND EASTERN NORTH AMERICA, WITH HINTS CONCERNING THE DERIVATION OF THE LATTER. By SAMUEL H. SCUDDER, of Cambridge, Mass.

ALTHOUGH a large number of European insects extend far into Asia, only the butterflies of Europe proper[1] are here considered; and from these are omitted all such as are peculiar to the Mediterranean borders. In like manner only the American species found east of the Rocky Mountains are brought under consideration, and from these are excepted the extreme southern species, such as occur only upon the Mexican Boundary or the shores of the Gulf of Mexico. Thus restricted, the areas of Europe and Eastern North America are not very unequal; they are embraced latitudinally by the same isothermal lines, nourish the same cereals, and have formed the basis of many faunal and floral comparisons. Writers in comparing the *insects* of the two countries have, as a general rule, invited attention to their similarity although acknowledging an almost complete distinction of the species. Will a critical study of the butterflies of the two regions lead to this conclusion?

As above restricted, the European fauna is less than one-quarter larger than the American (207 American species to 250 European); but a greater disparity becomes apparent as soon as we look at the comparative numbers of the different groups of butterflies. Considering, in the first place, each of the four families into which butterflies are divisible, we notice that while more than half of the European fauna is made up of Nymphales (131 sp.), less than one-third of the American fauna belongs to that family, the number of species (65) being only half that of the European. In the next two families, the differences are not conspicuous, the Rurales being proportionally less abundant in America (43 sp. or 20·8 per cent. of the whole) than in Europe (57 sp. or 22·8 per cent. of the whole); while the opposite is the case with the Papilionides (36 sp. or 17·4 per cent. in America; 34 sp. or 13·6 per cent. in Europe). We return again to a striking disparity when we reach the Urbicolæ, there being 63 sp. (or 30·4 per cent. of the whole) in America; and less than half that number, 28 sp. (or 11·2 per cent. of the

[1] Staudinger and Wocke's Catalogue of the European Lepidoptera, and my list of the Butterflies of N. America, north of Mexico (part of it still unpublished) are used as the basis of the statements in this paper.

whole) in Europe. As contrasted with each other, then, Europe is peculiar for its wealth in Nymphales, America in Urbicolæ.

Looking more closely into the respective representation of the members of these families on either side of the Atlantic, we shall find resemblances and disparities equally striking. The accompanying table, giving the number of species in the different groups in the two countries, and their percentage of the whole butterfly fauna in the same, tells its own story, but it is worth while to call attention to one or two points: *First*, that the great disparity in numbers of Nymphales and Urbicolæ on the two continents is almost wholly due to the vast number of Oreades in Europe, and of Astyci in America. Of the former group there are 77 species, or 58·8 per cent. of the Nymphales and 30·8 per cent. of the total fauna in Europe, against 19 species (less than one-fourth of the number in Europe), or 29·2 per cent. of the Nymphales and 9·2 per cent. of the total fauna in America; of the latter group 44 species or 69·8 per cent. of the Urbicolæ and 21·3 per cent. of the total fauna in America against 9 species (one-fifth of the number in America) or 32·2 per cent. of the Urbicolæ and 3·6 per cent. of the total fauna in Europe. *Second*, that while the Ephori are twice as numerous in America (20 sp.) as in Europe (10 sp.), the balance is more than restored by the superior number of Adolescentes in Europe (in Europe 38 sp., two-thirds of the Rurales, or 16·2 per cent. of the whole fauna; in America 13 species, or less than one-third of the Rurales, or 6·3 per cent. of the total fauna). *Third*, that there is no similarity whatever between any of the minor groups of Papilionides, excepting in the Voracia, which number eight species in Europe and five in America; in the other groups we find the Fugacia twice as numerous in America (20 sp.) as in Europe (10 sp.), and the Frugalia numbering seven species in Europe against two in America; while in the other subfamily of Papilionides, there are six Parnasii in Europe and none whatever in America; and but three species of Equites in Europe against nine in America. *Fourth*, that while there is so great a disparity in the number of Astyci among Urbicolæ, the parallel group of Hesperides is almost equally represented in each country.

Notwithstanding such striking contrasts, a glance at the table would seem to show many resemblances perhaps equally marked; but upon analysis these will be seen to disappear. In illustration, we may examine two striking cases, Præfecti and Hesperides.

TABLE GIVING THE RELATIVE NUMBER OF SPECIES OF THE DIFFERENT GROUPS OF BUTTERFLIES IN EUROPE AND EASTERN NORTH AMERICA.

NAME OF GROUP.	NUMBER OF SPECIES.				PERCENTAGE OF TOTAL FAUNA.	
	Europe.		America.		Europe.	America.
Oreades (Meadow browns)..........		77		19	30·8	9·2
Heliconidæ..........................		0		2	0·0	1·0
Argonautæ..........................	3		3		1·2	1·4
Archontes (Emperors)............	5		3		2·0	1·4
Præfecti (Angle-wings)............	11		11		4·4	5·3
Dryades (Fritillaries)...............	20		16		8·0	7·7
Hamadryades (Melitæans)........	14		10		5·6	4·8
Najades..............................		53		43	21·2	20·8
Hypati (Snout butterflies)..........		1		1	0·4	0·5
Nymphales.........................		**131**		**65**	**52·4**	**31·4**
Vestales (Erycinids)................		1		2	0·4	1·0
Ephori (Hair-streaks).............	10		20		4·0	9.7
Adolescentes (Blues)..............	38		13		15·2	6·3
Villicantes (Coppers)..............	8		8		3·2	3·9
Plebeii (Lycænids).................		56		41	22·4	19·8
Rurales............................		**57**		**43**	**22·8**	**20·8**
Fugacia (Yellows)..................	10		20		4·0	9·7
Voracia (Whites)...................	8		5		3·2	2·4
Frugalia (Orange-tips)............	7		2		2·8	1·0
Danai (Pierids).......		25		27	10·0	13·0
Parnasii............................	6		0		2·4	0·0
Equites (Swallow-tails)...........	3		9		1·2	4·3
Papilioninæ.........................		9		9	3·6	4·3
Papilionides........................		**34**		**36**	**13·6**	**17.4**
Hesperides (Large skippers).....	19		18		7·6	8·7
Astyci (Small skippers)...........	9		44		3·6	21·3
Castnioides.........................	0		1		0·0	0·5
Urbicolæ (Skippers)................		**28**		**63**	**11·2**	**30·3**
Total..........................	**250**		**207**		**100**	**99·9**

The former group, Præfecti, is represented in both countries by the same number of species (11) and presents closer resemblances on the two continents than any other group of butterflies, because its genera are mainly genera of the north temperate zone; three species of the group, indeed, are identical on the two continents, and it seems probable (on grounds given in another paper) that two of these have been introduced from America to Europe, and one from Europe to America. If we assume this, and refer these species solely to their proper country, we shall find that of the six American and six European genera of Præfecti, four are represented on both continents. In the latter group, Hesperides, the number of species (18–19) is almost equal in both countries; one species is common to both (probably indigenous in Europe), but, as the genus to which it belongs is otherwise represented in each, it does not affect the result; of the eight American and four European genera belonging to this group, only two are common to both, and in these two the representation is very unequal; Thanaos having six species in America against two in Europe, and Hesperia fourteen in Europe against two (one of them the introduced species) in America.

In other groups the differences are very observable. Thus, of the nine American genera of Ephori, but one is represented in Europe, and even in this group (Thecla) the European species have a peculiar facies distinct from the American. Only one of the seven American genera of Fugacia, and two of the seven American genera of Equites are found across the Atlantic; and in the Astyci only three or perhaps four of the twenty-three genera found in America occur at all in Europe. Or, if we sum up the whole, we may say that of the 105 American genera of butterflies, only twenty-seven or twenty-eight (or about one-fourth) are represented in Europe; and of the others, there are but seven intimately related to European genera.

To analyze minutely these points of resemblance would demand more detail than can be given here; but a careful study will show that they are almost all confined to groups which are boreal in their aspect; and if we had excluded from this comparison the American species occurring only in Canada and northward, together with the European species found only in the high north; and had included the Texan and Floridan species on the one hand and the Mediterranean on the other, not only would the number of species have been considerably augmented, but the resemblances would have been greatly diminished and the differences more than proportionally increased.

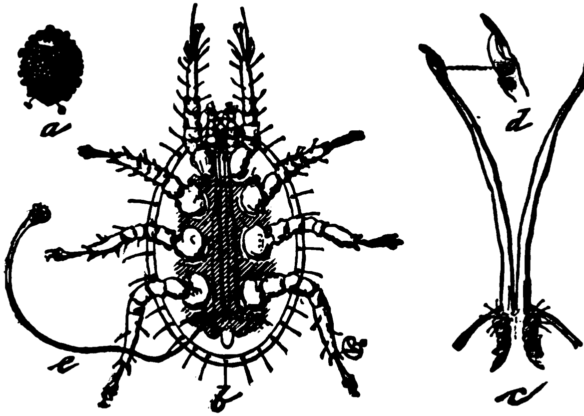
A word may be added concerning the identical species. They are but eleven at most, out of the 234 species found south of the Canadian border and east of the Rocky Mts. Four of them have doubtless been introduced from one country to the other; four more, which have been thought identical by some writers are boreal forms which reach down into the United States; the same may be said of one other, certainly identical. This leaves but two, *Cyaniris Pseudargiolus* and *Heodes Hypophlæas* (doubtless descended in a remote past from the ancestors of *Cyaniris Argiolus* and *Heodes Phlæas*) whose identity or intimate resemblance requires explanation. This is certainly a small number out of the hundreds of species on the two continents.

It will be evident from what has been stated that a considerable portion of the present butterfly fauna of the Eastern United States has been derived from the north, and a still greater portion from the south. If we are further to judge of the derivation of the present fauna by the geographical distribution of its nearest allies, we shall come to no uncertain conclusions. Omitting from consideration such species as are plainly derived from the north or the south, and the few species believed to have been introduced, there will be left about 140, which may be considered the foundation of the fauna, including nearly twenty genera peculiar to it. A careful study of the affinities of these 140 species shows that more than three-fourths (77–80 per cent.) of both genera and species have their nearest allies in the south,—mostly in Mexico and Central America. Nearly all of the remainder belong to genera represented around the entire temperate zone of the northern hemisphere, and a few are not distantly allied to East Indian genera. Excepting those which are closely connected either with European or with western American species, I do not recall a single genus peculiar to the region, unless it be Feniseca, which is unique. We may therefore conclude that by far the greater part of our present fauna was derived from the south.

In conclusion, it may be simply remarked that the features of this comparison would be still further intensified if we had entered into minute details; but enough has been said to show that but a tithe of the resemblances or distinctions pointed out could have been traced, if no regard had been paid to those minuter structural peculiarities, upon which genera should be based. As has been well said by a recent writer, it is "just this sort of nearer and more critical comparison" of structural forms, which "we now evidently need in order to discuss the question of geographical distribution to any purpose or advantage." The time is past when biological science can be satisfied with the superficial examination of striking features and the cursory descriptions of new forms. Resemblances are as important as distinctions; and the intimate and detailed study of every structural part of any creature will constantly lead to new points of comparison with allied animals. Until all of these are obtained, we have not reached the full solution of the prime question: Whence was it derived in time and space?

On an Extensile penetrating Organ in a Gamasid Mite. By Chas. V. Riley, of St. Louis, Mo.

There is a well known mite parasite of beetles and other Articulates, described in European works as *Uropoda vegetans*, which possesses the peculiarity of attaching itself to the hard, shelly parts of its victim by means of a thread-like filament that issues from the posterior part of the body. The body is broadly oval, depressed, in one piece, somewhat coriaceous above and yellowish brown in color. The filament is supposed by most authors to be secreted by special organs and to penetrate the victim and furnish nourishment. Careful study of a very similar species which infests Coleoptera in this country, and among others *Doryphora* 10-*lineata*, or the Colorado Potato-beetle, has convinced me that the anal filament is excrementitious, sticking to the beetle by a flattened disc and issuing from an anal vent just beneath the extremity of the body. I have, however, discovered a curious extensile and penetrating organ which seems to be homologous with the maxillæ, and which has remained unobserved by other investigators. It is composed of two parts, each terminating in a two-fingered claw, somewhat resembling that of a lobster. At rest these two parts lie just beneath the skin between the legs, their bases reaching to the anus, and their tips extending between the front coxae. When extended, they are usually brought closely together, and project the whole length of the animal beyond the head. The excremen-



UROPODA AMERICANA:—*a*, beetle infested with it—nat. size; *b*, ventral view, showing extensile organs between the legs, and (*e*) excrementitious filament; *c*, extensile organs protruded; *d*, claw of same—all greatly enlarged.

titious thread from the anus is fragile and easily breaks, but these curious organs enable the mite to hold on to its hard-bodied prey with greater tenacity.[1]

[1] As will be seen by the figure, these organs in repose extend so far back toward the anus that it is difficult to believe that they compose part of the mouth structure. Yet in carefully studying them I felt convinced that they were maxillæ, or rather the homologues of these organs in hexapods, and, in June, 1876, so informed Dr. A. S. Packard, Jr., to whom I submitted specimens. Through his courtesy and since the above paper was read before the Association, I have had the pleasure of perusing an elaborate and admirable article by P. Kramer, of Schleusingen, Prussia, on the natural history of certain genera in the family Gamasidæ, published in the *Archiv fuer Naturgeschichte*, 42d year, Part I, 1876. According to Kramar these hitherto undescribed organs (his *Scheerentaster*) occur in most Gamasid mites, though differing greatly in length and considerably in form in different species. He considers them 3-jointed, the basal joint simply cylindrical, the second likewise so at base, but ending in a strongly chitinized claw, generally toothed inside, and the third forming the inside finger of the claw, also generally toothed. In *Uropoda Americana* no true joints are discernible in the body of the processes, though there are restrictions. These maxillæ are evidently elastic and the anterior portion may be retracted more or less into the basal. Nor should I designate as a joint the thumb-like articulation of the terminal claw. Indeed, these claws seem to me to both of them articulate on the end of the process. In the species under consideration two teeth are sometimes discerned on the small thumb, but ordinarily they are not easily resolved.

This mite, except in the peculiar penetrating organ and in the body being in one piece, very closely resembles the descriptions of the *Gamasus* [*Carpais Latr.*] *coleopteratorum* of Europe, and has, I believe, very generally been considered the same. It has, however, all the characters of *Uropoda*, and I cannot think that the slight differences between *Gamasus* and *Uropoda* are of generic value. This American species may be known as *Uropoda Americana*, and I give herewith its characters more in detail.

Length 0·6 mm. Body in one piece, oval in outline, slightly produced in front, the back forming a polished, plano-convex carapace, with a thin and slightly dilate border, surrounded with 28 stiff hairs at regular intervals, with intermediate, more minute ones. Mandibles scissor-like, sharp-pointed; maxillary palpi 5-jointed, the joints sub-equal, with a few stiff hairs, and one longer setous hair from the basal joint; the extensile organs as described above, showing three faint constrictions but not articulate. Legs almost equal in length, the hind pair very slightly longest, and the third pair somewhat shortest; all armed with stiff hairs, and a vascular, elastic sucker at tip and two very minute claws; front pair with a long seta from apex extending along the outer side of the sucker.

I would more particularly call the attention of specialists to the curious penetrating organ here described, as its homology is by no means clearly made out. The excrementitious filament from the anus varies in length and thickness and is often two or three times as long as the mite, and while it generally issues from the anal vent in a continuous and unbroken thread, it is not unfrequently severed and attached to other parts of the venter.

On the Curious Egg Mass of Corydalus cornutus (Linn.) and on the Eggs that have hitherto been referred to that Species. By Chas. V. Riley, of St. Louis, Mo.

Our largest Neuropteron, belonging to the family *Sialidæ*, is *Corydalus cornutus*. It is not uncommon in the Eastern and Middle States, and is known in the Mississippi Valley by the vulgar name of the Hellgrammite. In the female the mandibles are quite formidable, but in the male they are curiously modified, and form long, incurved, smooth, prehensile organs of the form of the finger of a grain cradle, and evidently of use in enabling him to embrace his mate. The larva of this fly occurs in running streams, living mostly at the bottom, and hiding under stones in the swiftest parts. It has strong jaws, and in addition to the ordinary stigmata, it is furnished with two sets of gills, one set lateral and filamentous, the other ventral, and each composed of a sponge-like mass of short rust-brown fibres.

Its body terminates in two fleshy tubercles, each armed with a pair of hooks. It is best known in the full grown condition when, in seeking for a place in which to undergo its transformations, it travels and climbs on the shores of our rivers, and sometimes to great distances. Called a "crawler" by fishermen, it is greatly esteemed as bait. The pupa is quiescent and formed in a cavity in the ground. The supposed eggs of this insect were figured and described in the "American Entomologist," and in the Fifth "Missouri Entomological Report," as oval, about the size of a radish seed, and deposited in closely set patches of fifty and upward upon reeds and other aquatic plants;[1] and they have since been frequently referred to, no one questioning the accuracy of the conclusion of their discoverer, the late B. D. Walsh.

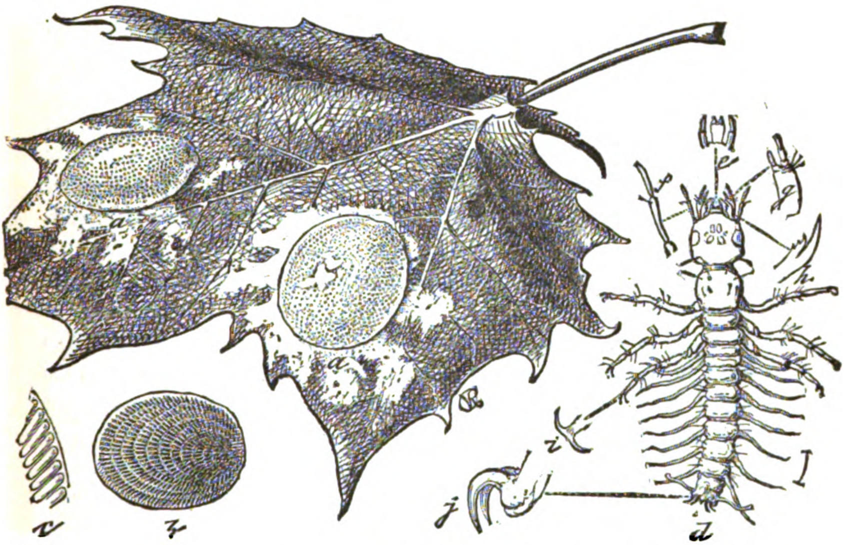
About the middle of last July, in sailing up the Mississippi River between Bushberg and St. Louis, my attention was attracted by sundry white splashes on the leaves of various plants that overhung the water; which splashings, at a distance, looked not unlike the droppings of some large bird. Approaching more closely to them, however, they were seen to consist of sub-oval or circular swellings, with more or less white splashed around them; and upon still closer inspection they proved to be egg-masses. They were generally attached one to the upper surface of a leaf of either Sycamore, Elm, Cottonwood, or Grapevine; but sometimes there were several on the same leaf, and at others they occurred on both sides of the leaf. It was evident that the leaves were objects of attachment only,[2] and from the fact that only those which overhung the water were selected by the parent, it was natural to infer that the species was aquatic in its larva state. Yet these egg-masses greatly puzzled me, as indeed they did all naturalists to whom I

[1] My own belief now is that these eggs in reality belong to *Belostoma grandis* (Linn.).

[2] Since this was written, I learn from Mr. J. A. Lintner of Albany, N. Y., that he has found these egg-masses attached to rocks in the Mohawk River, though he had no knowledge of their parentage.

referred them; for the eggs of the larger water-beetles were known, those of Corydalus were supposed to be known, and there was only one other insect in North America, viz., *Belostoma grandis*, large enough to be capable of laying such a mass. But the eggs were evidently not Heteropterous.

Patiently waiting till the eggs hatched, I recognized at once in



CORYDALUS CORNUTUS:—*a*, *a*, egg mass from above; *b*, same from beneath, just before the young hatch—nat. size; *c*, some of the outside eggs; *d*, larva; *e*, *f*, *g*, *h*, its mouth-parts; *i*, claw; *j*, anal hooks—all enlarged.

the young larva the characters of *Corydalus cornutus*, with the full grown larva of which I was familiar; and upon dissecting the abdomen of a female Hellgrammite, the nature of the curious egg-masses was fully confirmed in the perfect identity in shape and arrangement of those composing them, and of those in said abdomen.

The egg-mass of *Corydalus cornutus* is either broadly oval, circular, or (more exceptionally) even pyriform in circumference, flat on the attached side, and plano-convex on the exposed side. It averages 21 mm. in length, and is covered with a white or cream colored albuminous secretion, which is generally splashed around the mass on the leaf or other object of attachment. It contains from two to three thousand eggs, each of which is 1·3 mm. long and about one-third as wide, ellipsoidal, translucent, sordid white, with a delicate shell, and surrounded and separated from the adjoining eggs by a thin layer of the same white albuminous material which covers the whole.

The outer layer forms a compact arch, with the anterior ends pointing inwards, and the posterior ends showing like faint dots through the white covering. Those of the marginal row lie flat on the attached surface; the others gradually diverge outwardly, so that the central ones are at right angles with said object. Beneath this vaulted layer the rest lie on a plane with the leaf, those touching it in concentric rows, the rest packed in irregularly. Before hatching, the dark eyes of the embryon show distinctly through the delicate shell, and the eggs assume a darker color which contrasts more strongly with the white intervening matter.

The young crawl from under the mass and leave the vaulted covering intact. They all hatch simultaneously, and in the night.

The egg-burster[3] has the form of the common immature mushroom, and is easily perceived on the end of the vacated shell. The young larvæ crawl readily upon dry surfaces, with their tails hoisted in the air, and live for a day or more out of water; but when hatching out over an aquarium, they instinctively drop to the water, where, after resting for a while, with their bodies hanging down and their heads bent forward at the surface, they swim to the bottom by whipping the body from side to side, very much as a mosquito wriggler does. Here they secrete themselves and remain until, in the course of a few days, they perish. They cannot be reared in confinement, and running water is doubtless as essential to them as to the full grown larva.

The newly hatched larva differs from the full grown larva in the relatively longer legs and lateral filaments; in these last being smooth and not clothed with short hairs; in the abdomen not bulging at the middle, and in lacking the sponge-like gills beneath. The head is wider than the rest of the body, which tapers from the first to the last joint. The prothoracic joint is as long as, or longer than the meso, and metathoracic joints together, and the abdominal joints increase in length as they diminish in width. The legs are

[3] I am not aware that this special structure has been named. It is generally if not always, a part of the ambion, and is common to many insects, though varying much in form. It may be known as the *ruptor ovi*. Dr. Hagan has called it the "egg-burster," while erpetologists designate as the "egg-tooth" a structure having the same purpose.

nearly thrice as long as the width of the thoracic joints; the claws are movable and about ⅓ as long as the tarsus; the tibia and tarsus are sub-equal; the femur somewhat longer; the coxa and trochanter about as long as the femur: there is a whorl of bristles toward the end of the femur and tibia; the mandibles are stout, with two principal teeth, the basal with three notches, and the terminal one finely serrate: the maxillæ are elongate, reaching beyond the jaws, and with a simple inner, and a 2-jointed outer palpus, both having basal folds which often look like a basal joint: the antennæ are 3-jointed and reach beyond the jaws, the middle joint longest, the terminal nearly as long, and tapering: the labium is elongate-quadrate, tipped with two small tubercles, and with the palpi 2-jointed—the joints sub-equal. A few hairs occur on the sides of abdomen between the filaments.

Biological Notes on the Army Worm (Leucania unipuncta Haw.). By Chas. V. Riley, of St. Louis, Mo.

The Army Worm is one of the most destructive insects to North American Agriculture. At irregular intervals it sweeps through our meadows and grain-fields, about the time that wheat is beginning to ripen; often rendering them unfit for the mower or harvest-machine. It proves injurious from Maine to Texas, and from the Atlantic to the 100th meridian; and though the same species, or geographical races of it occur in other parts of the world, it is not known to be anywhere else so injurious. It is the larva of *Leucania unipuncta* Haw., a Noctuid with buff-colored wings, and characterized chiefly by having a conspicuous white speck on the disc of the primaries. Up to the year 1861 its parentage was unknown, and it is a singular fact, that notwithstanding the great abundance in which the insect occurs all over the country indicated, during certain years, the nature of the eggs, and the time, place and mode of oviposition remained unknown up to the present year. Two trains of circumstances, as I have elsewhere shown (8th Mo.

Ent. Rep.), serve to explain this fact. The one is, that during great Army Worm years, when the species most attracts attention, the worms are so followed by parasitic and predaceous insects, and so persecuted and destroyed by other animals, including man, that comparatively few of them survive long enough to produce the moths. The other, that in seasons when the insect does not abound, no one thinks of looking for the eggs.

The time and place of oviposition in this species is quite important from the economic standpoint. Structure is a very reliable index to habit, and anatomical study of the structure of the ovipositor, made last winter, convinced me that there was a third and more important reason why the eggs had remained undiscovered, viz., that they are secreted. With this clue, I have been able, the present year, to solve the mystery and to prove the correctness of the conclusion arrived at from structural study.

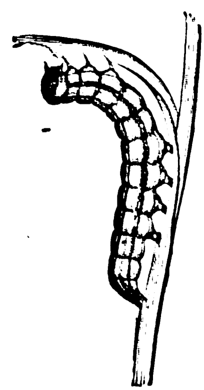
The eggs are indeed thrust in between the sheath and stalk of well-grown grasses whether cut or standing; or occasionally in between the natural fold of the green leaf, or the unnatural curl at the sides of a withered leaf. In low blue grass, where my first observations were made, they are almost invariably laid in the fold at the base and junction of the terminal blade with the stalk. The moth invariably endeavors to secrete them. They are generally laid in single rows of from five to twenty and upward, and accompanied with a white, glistening, viscid fluid, which glues them to each other and to the plant, and, when they are laid in the fold of a spear, draws the two sides securely over them, leaving but a glistening streak along the more or less perfectly closed edges. Each egg, when first laid, is spherical, $0{\cdot}4^{mm}$ in diameter, smooth, opaque white, with a very delicate and yielding shell, which, before hatching, becomes faintly iridescent, and shows the more sordid embryon within. The newly hatched larva is a looper, the two front pair of prolegs being so atrophied that it necessarily loops the body in crawling, as the full grown larvæ of another large family of moths — the Geometridæ — normally do. A large number of Noctuids in which the full grown larva has the normal complement of fully developed prolegs, exhibit this peculiarity in the early larval stages, and if the reduction of prolegs can be taken as evidence of progressive development, the Noctuids must clearly rank

Chrysalis of Army Worm.

beneath the Geometrids, though not so ranked by most systematists.[1]

The newly hatched Army Worm bears no resemblance to the full grown individual, and is so small and so much of a color with the pale base of a grass blade that it would be scarcely noticed, even where occurring in hundreds to the square foot. It develops very rapidly, going through five molts and attaining full growth in from two to three weeks.

There is one other mooted question in the natural history of the Army Worm which I have this summer been able to settle, viz., whether the species is single or double brooded. In a review of the matter in my 8th Report, I came to the conclusion that, in the more northern States, at least, or over the larger portion of the country in which it proves injurious, it is but single-brooded; and I am still of the opinion that such is the case. But I have proved that, like so many other species which are single-brooded further north, it is frequently, if not always, double-brooded in the latitude of St. Louis. By carefully feeding the moths reared from my first larvæ with sweetened water, and supplying them with grass in spacious vivaria, I succeeded in obtaining eggs from them. These eggs in due time hatched, and the second brood of worms gave me the moths again early in August. The worms were generally paler than those of the first brood, and being the second generation reared in confinement, they were less healthy. I obtained, in consequence, but five moths, all of them unfortunately females. One of these escaped, three died without showing any development of the ovaries, while the fifth died with the ovaries so well developed that the eggs, in a state of nature would probably have been laid within a week.



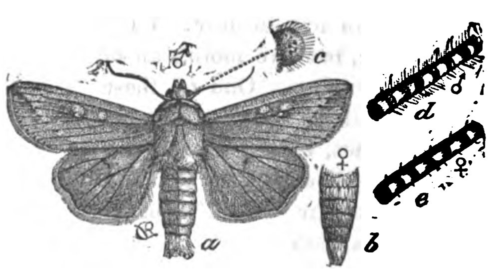
Full grown Army Worm.

It is very clear from the above recorded facts that the eggs of this insect do not as a rule, if at all, pass the winter at the foot of dry grass stalks, as was heretofore surmised. Nevertheless, the

[1] After the reading of this paper, it was claimed, and I think justly, by Dr. J. L. Le-Conte and Mr. S. H. Scudder, that relative rank must be judged of by characters within the groups considered, and not in comparison with other groups or orders; and as the newly hatched Noctuid often approaches in this respect the full-grown Geometrid larva, the latter are properly considered inferior in rank.

burning over of meadows and grain stubble in winter will act as a preventive of Army Worm injuries, for the reasons that the moth lays very early in the spring; that she prefers the full grown sheath and stalk even when dry, to the young green spears; and that she cannot well lay her eggs, for want of support, where the grass is yet sparse and thin as it is when first starting in a burned meadow. In my own experiments the females, in secreting their eggs, invariably showed a preference for old hay over fresh and growing grass. Finally, without entering into further details, I give the following as a revised summary of the history of the Army Worm.

The insect is with us every year. In ordinary seasons when it is not excessively numerous, it is seldom noticed,—1st, because the moths are low, swift flyers, and nocturnal in habit; 2nd, because the worms when young have protective coloring, and when mature hide during the day at the base of meadow grass. In years of great abundance the worms are generally unnoticed during early life, and attract attention only when, from crowding too much on each other, or from having exhausted the food supply in the field in which they hatched, they are forced from necessity to migrate to fresh pastures in immense bodies. The earliest attain full growth and commence to travel in armies and to devastate our fields and attract attention about the time that winter wheat is in the milk; this period being two months later in Maine than in S. Missouri; they soon afterwards descend into the ground and thus



ARMY WORM MOTH:—*a*, male moth; *b*, abdomen of female—nat. size; *c*, eye; *d*, base of male antenna; *e*, base of female antenna—enlarged.

suddenly disappear to issue again two or three weeks later as moths. In the latitude of St. Louis the bulk of these moths lay eggs from which are produced a second generation of worms which become moths again late in July or early in August. Farther north, there is but one generation annually. The moths hibernate and oviposit soon after vegetation starts in spring. The eggs are inserted between the sheath and stalk, or secreted in the folds of a blade. Mature and perennial grasses are preferred for this purpose by the parent. The worms abound in wet springs preceded by one or more very dry years. They are preyed upon by numerous enemies which so effectually check their increase whenever they unusually abound, that the second brood, where it occurs, is seldom noticed, and two great Army Worm years have never followed each other, and are not likely to do so. They may be prevented from invading a field by judicious ditching, and the burning over of a field in winter or early spring effectually prevents their hatching in such field.

It is thus that questions which have caused discussion for years, and given rise to various theories, are settled, and circumstances that seemed wonderful, and difficult to account for, are explained by a few careful observations and experiments.

TITLES OF OTHER PAPERS READ IN SECTION B.

THE WATER LIME GROUP OF BUFFALO. By A. R. Grote and W. H. Pitt, of Buffalo, N. Y.

ON THE SIPHON OF ENDOCERAS, A GENUS OF CHAMBERED SHELLS. By A. Winchell, of Syracuse, N. Y.

A SUPPLEMENT TO THE GLACIAL THEORY. By W. C. Kerr, of Raleigh, N. C.

A NOTE UPON THE PITCHSTONES OF ARRAN. By F. A. Gooch, of Cambridge, Mass.

ON THE MODE OF EXTRUSION OF THE OVA IN THE LIMPETS. By W. H. Dall, of Washington, D. C.

ON THE RECIPROCAL RELATIONS OF CERTAIN GENERA OF ARTICULATED BRACHIOPODS. By W. H. Dall, of Washington, D. C.

NOTE UPON THE GEOLOGICAL POSITION OF THE SERPENTINE LIMESTONE OF NORTHERN NEW YORK, AND AN INQUIRY REGARDING THE RELATIONS OF THIS LIMESTONE TO THE EOZOON LIMESTONE OF CANADA. By James Hall, of Albany, N. Y.

SOME NEW POINTS REGARDING THE TONGUE OF *Picus viridis*. By Joshua Lindahl, of Lund, Sweden.

ON A NEW SPECIES OF ARGULUS. By Albert H. Tuttle, of Columbus, Ohio.

ON SYCOTYPUS (BUSYCON) CANALICULATUS (Linn.). by F. W. Simonds, of Ithaca, N. Y.

THE SLIGHT MORPHOLOGICAL VALUE OF NATURAL ATTITUDE AND NUMERICAL COMPOSITION. By B. G. Wilder, of Ithaca, N. Y.

THE RELATIONS OF THE ROCKS OF OHIO TO THOSE OF PENNSYLVANIA AND NEW YORK. By J. S. Newberry, of New York.

THE GEOLOGY OF PETROLEUM. By J. S. Newberry, of New York.

THE ORIGIN AND MODE OF FORMATION OF THE GREAT LAKES. By J. S. Newberry, of New York.

NOTES ON THE MYRIAPODS OF OHIO. By Albert H. Tuttle, of Columbus, Ohio.

ON THE PETROSILEX PORPHYRIES OF NORTH CAROLINA. By T. Sterry Hunt, of Boston, Mass.

DESCRIPTION OF NEW FUNGUS ON THE LEAVES OF THE PEAR TREE. By W. H. Seaman, of Washington, D. C.

PRINCIPAL CHARACTERS OF AMERICAN PTERODACTYLES. By O. C. Marsh, of New Haven, Conn.

THE EDIBLE CRAB OF MARYLAND, CALLINECTES HASTATUS (Ordway). By P. R. Uhler, of Baltimore, Md.

PHYLLOTAXIS OF CONES. By W. J. Beal, of Lansing, Mich.

CAN THE UNIOS SEE? By W. J. Beal, of Lansing, Mich.

CROSS-FERTILIZATION OF APPLE-BLOSSOMS. By W. J. Beal, of Lansing, Mich.

SENSITIVE STIGMAS AS AN AID TO CROSS-FERTILIZATION OF FLOWERS. By W. J. Beal, of Lansing, Mich.

ON THE SOURCE OF THE ANCIENT GLACIERS OF NORTH AMERICA. By Otto Torell, of Stockholm, Sweden.

ON THE BURIAL PLACE OF THE YORKSHIRE MASTODON, DISCOVERED IN BROOME CO., N. Y. By T. B. Comstock, of Ithaca, N. Y.

PERMANENT SUBSECTION OF ANTHROPOLOGY.

PAPERS READ.

GREEK AND ETRUSCAN ART IN JEWELLERY AND ITS REVIVAL. BY ALESSANDRO CASTELLANI, of Rome.

IN a few words I hope to present the result of my researches on the subject of the Art of Jewellery as practised by the ancients, not only with reference to the forms which ornaments, serving as such brilliant additions to the female toilette, assumed at the periods referred to, but with reference also to the no less interesting processes of execution employed by the artists of those times. These processes are unhappily lost, with many secrets of a civilization which was the mother of our own, a noble inheritance of the greater part of which barbarous ages have robbed us.

It must with humility be confessed, that we see at present rising as if by enchantment, from the forgotten cemeteries of Etruria and of Greece, objects in gold, of a workmanship so perfect, that not only all the refinements of our civilization cannot imitate, but cannot even explain theoretically the processes of its execution. It appears that the Greeks and Etruscans had, so to speak, acquired a complete knowledge of all those practical arts in their highest degree of perfection, by the aid of which the most ancient people of the East wrought the precious metals.

Once initiated into the modes of treating the raw material, and of subjecting it to all the caprices of their imagination, the artists of Etruria and Greece had but to apply these processes to elegance and to the vast resources of the art, such as their own genius conceived. Thanks to the vivifying breath which animated and guided the intellect of that age in search of the beautiful, all the branches of this art felt their relationship to each other, and jewellery did not fall behind in the universal movement which tended to perfection. At a later period it could not sustain the high rank it had attained, and in the palmy days of Imperial Rome began to decline rapidly. I have not seen a single work in gold dating from a well-determined Roman epoch, even including the most artistic periods, which can in any degree whatever be compared for elegance of form or skill of workmanship with the archaic productions of Greek, or Etruscan art. Without doubt the Romans had traditionally preserved certain primitive forms belonging to their models, but to these models the imitations are in point of execution extremely inferior.

I will not speak here of the complete degradation into which the art had sunk on the fall of the Roman Empire when the material formed the only value of the ornament. Jewellery among the early Christians had but the rude simplicity which at that time belonged to all the productions of this lost art.

The transfer of the seat of the empire to Byzantium marked a new phase in the history of jewellery. It became quickly grafted on the Arab art, and by means of this new element acquired quite a different style from that which it had derived from the artists of antiquity. Enamels, precious stones, pearls and coarse chasings, all mounted together with an exuberance of barbaric luxury, constitute the characteristic traits of that Byzantine school, which, whilst it preserved in the general disposition of its ornamentation the square forms of Greek art, served so well for the transition between ancient and modern art of the period of the Renaissance. I will not speak of what jewellery had become in the hands of the Goths and of the Lombards. We have an example in the celebrated crowns of Toledo now placed in the museum of the Hôtel de Cluny. In these crowns, gold is treated as a village blacksmith would hardly at present treat tin or copper. In making this remark, however, I would by no means depreciate the incomparable scientific value of these rare objects.

After the close of the tenth century the art profited by the general aspiration of the public mind, just delivered from fears created by gloomy prophecies, towards a better future.

We need no other proof of this than what is furnished by Theophilus and his school, and by the relics of that time which have come down to us. By insensible advances the Art gradually developed itself up to the fifteenth century, when it suddenly expanded under the direction of the new Italian school, at the head of which stood, Maso Finiguerra, Caradosso, Cellini, and many other eminent artists, who accomplished wonders in it. But this Renaissance was not, as regards jewellery, a return to classic forms; on the contrary, an entirely new school sprang up. New experiments, new elements and new methods were introduced; chasings, engravings, enamelling and *nielli* were employed in endless variety; neither in design nor workmanship was there any reminiscence of antiquity.

The gold ornaments of Vulci, Cervetri, Chiusi, Toscanella and of Kertch, remain still buried in the mysterious tombs which held their ancient possessors. Had Cellini any knowledge of their existence and was he willing to take them as models? From the time of Cellini the art, instead of progressing, lost much of its luster, till it became entirely degraded in the hands of the Spaniards.

I will not enter into the history of this decay of jewellery, losing every day its artistic character to become more and more in modern times a mere object of trade and of paltry speculation. Grieved at witnessing in Rome the prevalence of this deplorable influence, my father, younger brother and myself believed that it might be a matter of some importance, in the midst of the universal improvement of taste, to give a purer and higher direction to the art to which we had devoted ourselves.

In the year 1830, some fortunate excavations brought to light the treasures hidden beneath the soil of Etruria. Every one was struck with admiration at the beautiful ornaments discovered in the cemeteries of this mysterious country; and my father was the first to form the design of imitating some of them. Encouraged by the praise and counsel of friends of the arts, among whom I may mention as holding the first rank the Duke Michelangelo Caetani, known as possessing the purest taste and the feelings of a true artist, he received at Rome the art of the jeweller by taking as models the most perfect examples that antiquity could furnish him.

The discovery of the celebrated tomb known as that of *Regulini Galassi at Cervetri*, was an event of the highest importance in regard to our enterprise. On the Papal Government expressing a wish to become possessed of the objects in gold found in this crypt, my father and I were called upon to examine them with the utmost care. We had thus an opportunity of studying the particular character of Etruscan jewellery, and, holding thereby in our hands the thread which was to guide us through our researches, we set earnestly to work. The subsequent discoveries of Campanari at Toscanella, and of the Marquis Campana at Coere, and the excavations made at Vulci with so much intelligence by our friend Francois, by Prince Torlonia, and by Mr. Noël des Vergers, have revealed new treasures to us and have furnished models of the most exquisite elegance.

Our first object was to detect the processes by which the ancients worked. We remarked that all their jewellery, except that intended for funeral ceremonies, instead of owing the raised parts to chisselling or engraving, were formed by separate pieces brought together and placed one upon the other. This it is, in my opinion, that gives it so peculiar and marked a character, derived rather from the expression, as it were, of the spontaneous idea and inspiration of the artist, than from the cold and regular execution of the workman. Its very imperfections and omissions, purposely made, give to the workmanship that artistic character altogether wanting in the greater number of modern works, which, owing to a monotonous uniformity produced by punching and casting, have an appearance of triviality depriving them of all individual character; that charm which so constantly strikes us in the productions of the ancients.

The first problem then that offered itself to our attention was to find the means of soldering together, with the utmost neatness and delicacy, so many pieces of extraordinary thinness. Among others, those almost invisible grains, like little pearls, which play so important a part in the ornamentation of antique jewellery, presented difficulties nearly insurmountable. We made innumerable essays, employing all possible agents and the most powerful dissolvents to compose proper solder. We consulted the writings of Pliny, Theophilus and Benvenuto Cellini; we neglected no other sources of instruction with which tradition could furnish us. We studied the work of Indian Jewellers and those of the Maltese and Genoese, but it was only in a remote corner of the Marches at *St. Angelo in Vado*, a little district hidden in the recesses of the Apennines far from every centre of civilization, that we found still in use some of the processes employed by the Etruscans. There yet exists, in fact, in this region of Italy, a special school of traditional jewellery, somewhat similar — not certainly in taste or elegance of design, but at least in method of workmanship — to the ancient art. The beautiful peasant girls of these districts, when at their wedding feasts, wear necklaces and long earrings called *Navicelle*, much resembling in workmanship the antique. We procured then from *St. Angelo in Vado*, a few workmen to whom we taught the art of producing Etruscan jewellery. Inheriting the patience of their forefathers, and caring nothing for those mechanical contrivances by which geometrical exactness is attained in modern jewellery, these men succeeded better than all whom we had previously employed in the imitation of that freedom of style, which is the particular characteristic of the art among the ancients.

In substituting arseniates for borax as solvents and reducing the solder to an impalpable file-dust, we obtained results of a sufficiently satisfactory nature. We profited, also by the chemical studies of my father in the coloring of gold. We dispensed, as much as possible, with the use of the punch and of the jet. Having come to the conclusion that certain works of the ancients, very delicately executed, must have been done by women, we confided to intelligent workwomen that which required the most delicacy. The result was excellent, especially in the placing and soldering of that little granulation which is carried over the face of most Etruscan jewellery.

In a memoir on antique jewellery which I read in 1860, at a meeting of the *Académie des Inscriptions et Belles Lettres* in Paris, I made the following conclusion: "Nevertheless, we are convinced that the ancients had some special chemical processes for fixing these strings of small grains of which we are ignorant; for, in spite of all our efforts, we have been unable to reproduce some exquisitely fine workmanship, and despair of being able to do so, unless aided by some new scientific discoveries. We do not, however, intend to discontinue our labors, and it is therefore with confidence, gentlemen, that I address myself to you. If your studies of antiquity in all its branches have brought to your notice any passages in the classic authors which may put us on the track of discovering the secret of which we are in search, be so good, in the interest of art, to point them out to us, and be assured that we shall feel grateful for your assistance."

This I said sixteen years ago. I have now the satisfaction to declare to you, who so nobly represent the Science of America and Europe in this Congress, that the lost art of the granulated work of the Etruscans and Greeks, has been at last entirely revived by us.

The specimens of gold ornaments in granulated work, which I exhibit in the Italian section (Main Building) in the Centennial Exposition at Philadelphia, will enable you to judge yourselves of the results of our studies of the art under its ancient forms, which have been, and will still continue to be, our models.

THE ANTIQUITES OF PORTO RICO. By O. T. MASON, of Washington, D. C.

WHENEVER any of the stone implements of the ancient inhabitants of Porto Rico have fallen into the hands of collectors, they have awakened admiration by their symmetry and uniqueness of form, by their variety and chasteness of ornament, and by the skill displayed in their manufacture. But they had not occurred in sufficient numbers to allow of classification and comparative study, until the Smithsonian Institution, in 1875, received by bequest the magnificent collection of Mr. George Latimer. This gentleman, having visited the West Indies in his youth, became so attached to the country, that, in 1828, he removed first to St. Thomas, and afterwards to Porto Rico, where he remained until nearly the time of his death, which occurred in Paris, Aug., 1874, from the effects of a surgical operation. During his residence he not only became a successful business man, but was honored by his country with the office of Consul-general of the Island, and was also, at the time of his death, consul for Holland and Austria. He had also been created by the King of Spain a "Knight of the Order of Isabella." He has, unfortunately, left no written descriptions of the objects, nor of the places and circumstances of their discovery. Actuated, at first by mere curiosity, he became afterwards enamored of the pursuit until he amassed a collection, which, for all time, will be the standard of reference for the antiquities of Porto Rico.

For the purpose of describing them more intelligibly, the objects are divided into pottery, celts, smoothing stones, discoidal and spheroidal stones, beads, cylinders, amulets, rude pillar stones, mammiform stones, masks, and collars.

The pottery is red in color, well baked, and sometimes varnished. It is rather coarse in quality, the clay appearing to be mingled in many instances with a very rough-grained shell or other degraissant. There are no complete vessels, and only a few fragments which show the former shape of those from which they were broken. The great majority of pieces in the collection are handles of dishes and vases. These represent principally monkeys and vampire heads, adorned with droll headdresses, which are ornamented with scrolled, circular, and zigzag lines. They seem to have been made separately from the vessel, and to have been luted on. There are some fragments of dishes with festoons evidently affixed in this manner. Similar pottery is found throughout the West Indies, in Central America, and in British Guiana.

The celts, one hundred and thirty-five in number, are of the highest beauty, and the most of them so much alike and so different from those of other countries, that Mr. John Evans, recognizing the type as purely Carib, hesitated to engrave a similar one from Scotland. It is figure 75 in "Ancient Stone Implements, etc., of Great Britain."

In carrying out the general shape, some variety is given by the form and size of the raw material. They vary in color from black to nearly white, many of them being a beautiful jade-like green; in cross section from circular to oblong-elliptical; in length from 1·75 to 12 inches; in width from ·75 to 6·5 inches. The chord of the edge is not always perpendicular to the axis. Some have semi-circular edges, others are nearly straight-edged.

In addition to this large class of almond shaped celts a few are entirely unlike them, and resemble so much those found in other localities as to hint at exchange of the objects, or a borrowing of patterns.

Of smoothing and sharpening stones there are a few in the collection. Four of them are pestle-shaped, smooth on the bottom, but the handle or upper part is more or less crooked or horn-shaped, and slewed to one side.

The mealing implements embrace the upper and the nether stone. The upper stones are nearly all pestle-shaped, but the smooth, oily under surface indicates the rubbing of chocolate or soaked grain rather than the pulverizing of dry materials. Several of the pestles in Mr. Latimer's collection are ornamented around the top either with a ridge and furrow, a human face, or a bird's head.

The lower stones are either dished mortars or metate stones. The dished mortars are bowl-shaped, boat-shaped, and semi-ovoid. In size they vary from the tiny paint mortar to those seventeen inches in length.

The metates have three or four legs, they are either rude or very highly finished. The rude variety are of a very porous volcanic stone, rest on three legs set obliquely so as to resist the downward pressure, and most of them sag slightly in the middle and to one side. A highly finished metate, made of a fine grained sandstone, is thin and very deeply sagged. In front three prominences represent the head and fore-feet of a turtle. The other end is abruptly elevated and crossed by a band carved with scrolled ornaments.

The spheroidal and discoidal stones are very similar to many in our museums from various localities. They may be natural formations, and their presence with other relics intimates to us how nature has, in many ways, been the instructor of man.

The beads are of the small and the massive varieties. They are exceedingly valuable as examples of the patience and skill of their makers in the art of perforating stone. A string of seventy chalcedony beads, about the size of peas, rounded and perforated (some of them in two directions), is the most remarkable specimen of stone polishing and boring that has fallen under the writer's notice. The amulets or small images are, with the exception of a few animal forms, of one pattern. A human figure is kneeling down, the arms and legs are pinioned back, and the shoulder blades are pierced for suspension. The perforation is sometimes through the head, and an animal face replaces that of a human being.

The rough pillar stones are mere slabs, without any definite shape, and the sculpture on them of the very rudest sort. I could discover little that was conventional about them excepting the fashion of carving a face on the stomach of the individual rudely represented by the whole stone. This singular fancy appears on one of the great pillar-stones sent from Nicaragua to the National Museum, by the Hon. E. G. Squier.

The mammiform stones have excited my curiosity more than anything else in the collection. These strange and beautiful objects generally represent a man lying on his stomach, his face more or less upturned, his mouth open, and his countenance wearing a tortured look. The other end of the stone represents the lower extremities of the body doubled up so as to expose the soles of the feet against the rump. On the back of the prostrate form is a conoid prominence, beautifully rounded up, straight or slightly concave in front, a little convex in the rear, bulging somewhat to one side, descending more or less below the top of the head and the rump, so as to form furrows. The whole affair reminded the writer of the legend of Typhœus placed by Jove beneath Mt. Etna. The bottom, always rough from use or want of finish is sometimes flat, sometimes convex, but most frequently warped up in the middle and hollowed out in a cymbiform cavity. In such cases the objects rest unsteadily upon the chin and knees, the under side of which are polished by wear. In the three characters of the head, the feet, and the *mamma*, there is to be seen a grading of elaboration.

The head of the rudest is a simple knob, then comes a human face without ornament, the most elaborate is ornamented with chevroned bands. The human face is often replaced by grotesque animal forms, either bird or mammal. The foot, like the head, has gradations of finish from the mere knob to the chevroned fillet, but in no case have I seen the human feet replaced by claws or hoofs.

The *mamma* runs through a series of forms, from the simple cone to the ridged and carved. The material of these objects is more various than that of the celts even, and they range in length from 12·5 to 2·75 inches. There is one singular specimen among them meriting a more extended notice. The front of the *mamma* is a grotesque human face. The rear is carved to represent a frog, whose nose forms the apex of the stone, and whose back and hind legs drawn up fill the remaining surface. The fore-legs pass down the sides of the cheeks and under the lower jaw, forming with the nose, a fillet around the human face. The Typhœan figure is wanting altogether.

The masks vary greatly in form. A few resemble the mammiform stones, but the mammals are replaced by a somewhat grotesque face, the Aztec nose forming the apex of the stone.

Another variety are flat, kite-shaped, with the human face carved partly in relief, on the flat surface, and are from 5 to 7 inches in length.

The Collars in the Latimer collection are thirty-five in number. In this are included three specimens so rude that it is not certain that they were intended for collars at all. None of the characteristic markings of the collars are visible on them. The largest is 23–35 inches external length; 8·4 inches, internal length; 17·7 inches external width; 7·3 inches internal width; and 5·8 inches thick.

Of those which are finished there are two classes, the massive-oval, and the slender, oblique-ovate, or pear-shaped. The latter are far more highly polished and ornamented than the former. In both classes there is a grade of elaboration, although a great deal of conventionalism as we shall see. The characters or marks which, by their peculiar *forms* and by their *presence* or *absence* give distinctness to the different specimens are the *shoulder*, the *shoulder ridge*, the *boss*, the *right panel*, the *left panel*, the *panel ornament*, the *marginal prominences*, the *panel border or scroll, and the marginal ridge or furrow*. The shoulder is a prominence on the upper limb of the circumference, resembling a knot. This projection, always midway between the margins, is sometimes on the right side, sometimes on the left, which gives rise to two sub-classes, the right-shouldered, and the left-shouldered. In the rudest forms the shoulder is wanting, in some it resembles a knot of a tree, in others it has a more or less distinct head.

The shoulder ridge is immediately below the shoulder and transverse. It varies from a small swelling to an elaborate encircling band, with ridges running from it, along the anterior and posterior margins quite around the specimen. The boss is a large, abrupt swelling at the bottom of the collar, and always oblique to its plane.

The panels are the expanded spaces on either side of the boss and always by their markings bear a definite relation to the shoulder. One of them is always more elaborately finished than the other, or, in some cases, only one side is ornamented at all, but in both cases the more richly carved panel is on the side opposite the shoulder. The panel ornament by its presence or absence is a very marked feature of these objects. In some cases it is a pecked chamfer, in others an oval depression, but upon the panel opposite the shoulders, the artist has expended his best efforts. The massive collars frequently have a gourd-shaped pattern, the neck being near the middle of the side, and the bottom of the gourd resting against the boss. The interior of this space is carved with chevroned lines. In the slender collars the gourd-shaped panel, without losing its identity, is transformed, and the centre carved with still more varied and elaborate designs. The marginal prominences are two gentle swellings one anterior, the other posterior, and always accompany the panel on the shouldered side. The panel border belongs to the ornamental panel, or the outside panels if we may be permitted to guess its function. It is on the anterior margin of the panel and consists of a double scroll in relief, having a triglyph or sphere, or human head in its centre. The marginal ridge or furrow is a wing-like bead that in the best specimens extends nearly or quite around the anterior and posterior margins.

In the collection is one object which to the writer appears to be a broken collar pierced for secondary use. If it is so, it is the only specimen which the writer has seen with the human face in the panels: if it is not, it is typical of an entirely new class.

As to the place of most of these objects in our classification we need to have no doubt. The pottery, the celts, the smoothing stones, the discoidal and spheroidal stones, the beads, the amulets, the pillar stones, these are just as intelligible to us as similar objects from other localities. But the mammiform stones and the collars are anthropological enigmas, and having finished my description I may be permitted in closing to indulge in a few guesses. The rough and hollowed surface of the mammiform stones suggests the grinding of something, paint, incense, narcotic, spice or some other precious substance. But against this theory is the fact that some are concave, some are flat, and others are convex. The furrows at the base of the *mammæ* seem to indicate the lashing to a handle for a war or a sacrificial club. The furrows are not worn by use, however, and in some there are no grooves. Their elegance of form and finish show that they were designed by their authors to be evidences of their skill, and as the statues of Greek and Roman gods are not now kept in temples of worship, but in galleries of art, we must relegate these beautiful objects to the æsthetic corner as souvenirs of the most artistic stone-age-people that ever lived.

The yokes are quite as puzzling. The two classes, the massive and slender would seem to indicate two classes of use; their right and left shouldering that they were to be used in pairs; their grades of elaboration, that there was a subordination of some kind involved in their use. But whether they were the symbolic regalia of sacrificial victims, of military heroes, of priestly orders, who marched in double file through the streets of Porto Rican cities now ruined and obliterated, the writer does not pretend to decide.

Peculiarities of the Femora from tumuli in Michigan. By Henry Gillman, of Detroit, Michigan.

In a paper entitled "The Mound Builders and platycnemism in Michigan," printed in the Smithsonian Report for 1873, brief mention is made by me of a certain compression of the femora which I had noticed as pertaining to the remains of those Platycnemic men from the ancient mounds along the Detroit and Rouge Rivers.[1] For various reasons, at that time and subsequently, I deferred entering more particularly on the subject. Since, the accumulation of more abundant material, and the careful comparison of that which I had already possessed, enable me to speak more confidently and at greater length as to the points involved.

The discovery of a single specimen of a perfect femur in a cave in Denbighshire, Wales, and evidently belonging to the Neolithic age, was the occasion of the first mention of the flattening of this bone. Prof. Busk called attention to the unusual compression presented by the shaft of this femur in the antero-posterior direction in the upper part, for the extent of about three inches below the trochanter minor. He states that at about two inches below that process the shaft measures $\cdot 9 \times 1\cdot 45$, whilst in three ordinary femora the bone at the corresponding part gives a mean of $\cdot 9 \times 1\cdot 15$, showing the cave femur to be "unusually expanded laterally in the upper part of the shaft. The consequence is to give the bone at that part a peculiar aspect, which is especially seen in an acute internal angle, and one rather less acute externally, instead of the usually rounded internal and external borders. He further states that "the distal extremity appears to be disproportionately large as compared with a recent well-formed bone of the same length," giving the comparative measurement of the condyles, and adds, "the lower part of the shaft is also somewhat expanded. But the chief peculiarity, as above remarked, is the compression of the shaft in the upper part."

Though Prof. Busk makes mention of several other femora from the same locality, as he says nothing of their possessing the peculiarity referred to, we can only conclude that they did not present it.

Of four femora from a tumulus in the same county, one has the characteristic to the extent that "the antero-posterior and trans-

[1] See also paper "The Ancient Men of the Great Lakes," by Henry Gillman, read at Detroit Meeting of A. A. A. S., August, 1875, p. 318 of "Proceedings."

verse diameters of the shaft, about 1½ inch below the trochanter minor, are ·85×1·4;" a second "not quite so much compressed in the upper part, measuring ·8×1·2." In the two remaining femora these diameters are "pretty nearly in the usual proportions."

To properly appreciate the peculiarities of the mound femora from Michigan, I have thought it best to exhibit their dimensions in tabular form. The following table presents those taken from the Rouge River mound. [See Table A, page 302.]

From this table it will be seen that not only have these femora, to a greater or less extent, the compression at the upper end of the shaft, at about 1½ inch below the trochanter minor, but they also present a similar peculiarity toward the distal end, or at a point 2 inches above the posterior border of the intercondyloid notch.

Without an exception, these 21 femora, in all probability representing 19 individuals, exhibit so distinctly the characteristic that it is immediately apparent on sight, and particularly by comparison with the normal or ordinary white femur. From the latitudinal indices, which give the amount or degree of this flattening, the range for the proximal end is from the maximum of ·592 to the minimum of ·859, affording a mean of ·718. Similar indices of the neolithic femora computed by me from Prof. Busk's data, give a maximum of ·607, a minimum of ·800, and a mean of ·673. It should, however, be remembered that in the case of those neolithic bones, Prof. Busk has selected four possessing the flattening, rejecting a large number which did not present it; while I have made no selection in the case of the Michigan femora, but give the dimensions of all the thigh bones which were preserved. The three ordinary (modern) femora referred to by Busk, have, I compute, a mean latitudinal index (proximal end) of ·780. The latitudinal indices for the ulterior extremity in the Rouge River femora are, maximum ·676, minimum ·774, the mean being ·726. But six bones enter into this last exhibit, representing probably five individuals, and those seemingly not possessing the extreme degree of the flattening. We have no means of making comparison in this respect with either "the 3 ordinary femora" or the neolithic bones. Prof. Busk makes no mention of this peculiarity other than implied by the incidental remark with respect to the single cave femur: "The lower part of the shaft is also somewhat expanded." So that it is to be presumed the difference from the

TABLE A.

DIMENSIONS, ETC., OF FEMORA FROM GREAT MOUND, RIVER ROUGE, MICHIGAN.

No., &c.	Length.	Least circumference.	Antero-posterior diameter and transverse diameter of shaft 1½ inch below trochanter minor.	Antero-posterior diameter and transverse diameter of shaft 2 inches above posterior border of intercondyloid notch.	Latitudinal index. Proximal end.	Latitudinal index. Ulterior end.	Perimetral index.	Diameter of head.
1. R. Strongly carinate	17·90	3·24	0·86 × 1·34	1·13 × 1·69	·641	·676	·184	1·83
2. L. " "	17·50	3·38	0·96 × 1·33	1·10 × 1·58	·721	·696	·193	
3. L. " "	17·90	3·58	1·00 × 1·30	1·20 × 1·55	·769	·774	·198	
4. R. Very strongly carinate	16·50	2·95	0·81 × 1·19	1·00 × 1·38	·680	·724	·179	
5. R. " " "		3·30	1·10 × 1·28		·859			
6. R. Slightly carinate		3·15	0·89 × 1·34		·664			1·82
7. L. Strongly "		3·55	0·98 × 1·38		·710			
8. R. " "		3·13	0·94 × 1·32		·712			
9. R. " "		3·42	0·94 × 1·30		·723			
10. L. Very strongly carinate		3·35	1·03 × 1·22		·844			
11. L. Carinate		3·25	0·96 × 1·43		·671			
12. R. "		3·30	0·92 × 1·38		·667			
13. R. Strongly carinate		3·03	0·94 × 1·27		·661			
14. L. Carinate		2·75	0·78 × 1·14		·684			
15. L. "		2·90	0·89 × 1·13		·787			1·49
16. R. "	16·00	3·01	0·85 × 1·10	0·97 × 1·28	·772	·757	·188	1·52
16. L. "	15·95	2·95	0·82 × 1·07	0·98 × 1·28	·766	·765	·185	1·47
17. L.		2·96	0·74 × 1·25		·592			1·58
18. L. Remarkably carinate			0·88 × 1·13		·778			
19. R.			0·79 × 1·30		·607			
19 L.		3·16	0·87 × 1·28		·679			
Means	16·96	3·20	0·90 × 1·27	1·08 × 1·49	·718	·726	·187	1·64

normal type was, even in this instance, too slight to be worthy of record, and in the other cases was not observable. But in this connection I would introduce for comparison the dimensions which I have taken at the corresponding part in the skeleton of a well-developed, perfect young man (white), who, though of short stature (5′,, 3″·2), presented in the femur the perimetral index of ·200, somewhat in excess of the normal English. This specimen gives the latitudinal indices of the proximal and ulterior extremities of the shaft as, respectively ·929 and ·968, thus closely approaching the circular form. This is worthy of comparison with "the 3 ordinary femora" of Busk. I append, in Table B, the full dimensions of this young man's femora.

TABLE B.

DIMENSIONS, ETC., OF FEMORA OF A YOUNG MAN (WHITE).

No., &c.	Length.	Least circumference.	Antero-posterior diameter and transverse diameter of shaft 1½ inch below trochanter minor.	Antero-posterior diameter and transverse diameter of shaft 2 inches above posterior border of intercondyloid notch.	Latitudinal index.		Perimetral index.	Diameter of head.
					Proximal end.	Ulterior end.		
1. Right.................. ...	17·38	3.48	1·10 × 1·15	1·40 × 1·45	.956	·965	·200	1·95
1. Left......................	17·38	3·46	1·02 × 1·13	1·38 × 1·42	·902	·972	·200	1·95
Means......................	17·38	3·47	1·06 × 1·14	1·39 × 1·43	·929	·968	·200	1·95

In Table C [see page 304] I give the measurements of the femora from the Circular Mound, on the Detroit River, Michigan.

The letters R and L, as in all the tables, denote whether the bone belongs to the right or left side. In view of the remarks already made, it is unnecessary to point out the different relations of the comparative dimensions here afforded, and which show the amount of the peculiarities referred to, at a glance.

In this connection reference should be made to the Table giving the dimensions of the femora in my paper on "The investigation of the Fort Wayne Mound, Michigan." The Circular Mound was situated but a few hundred feet to the S.W. of the latter tumulus. But the repetition of the table here is not necessary.

TABLE C.

DIMENSIONS, ETC., OF FEMORA FROM CIRCULAR MOUND, DETROIT RIVER, MICHIGAN.

No., &c.	Length.	Least circumference.	Antero-posterior diameter and transverse diameter of shaft 1½ inch below trochanter minor.	Antero-posterior diameter and transverse diameter of shaft 2 inches above posterior border of intercondyloid notch.	Latitudinal index.		Perimetral index.	Diameter of head.
					Proximal end.	Ulterior end.		
1. R. Carinate	....	....	0·93 × 1·25	....	·744	....	....	1·55
2. R.	....	....	1·00 × 1·32	....	·757	....	....	....
3. L.	....	3·32	0·88 × 1·33	....	·661	....	....	1·80
4. L. Very carinate	....	3·62	1·01 × 1·27	....	·795	....	....	1·62
5. R.	....	....	1·07 × 1·45	....	·738	....	....	....
6. R.	....	....	0·95 × 1·27	....	·748	....	....	....
7.	....	....	....	1·03 × 1·57	....	·656	....	....
8. L.[1] Very carinate	17·20	3·20	1·00 × 1·12	....	·852	....	·186	1·65
9. L.	....	3·24	0·92 × 1·33	....	·691	....	....	1·65
10.	19·33	3·25	1·13 × 1·30	1·24 × 1·67	·869	·742	·168	1·84
Means	18·26	3·32	0·98 × 1·29	1·13 × 1·62	·766	6·99	·177	1·68

In Table D are given the dimensions of a pair of femora from Chambers Island Mound, Green Bay, Wisconsin, and which also exhibit the peculiar flattening at both extremities. The individual they represent had, I have computed, a stature of about 5′ 10″. The bones are very carinate, and have the linea aspera strongly pronounced.

TABLE D.

DIMENSIONS, ETC., OF FEMORA FROM CHAMBERS ISLAND MOUND, WISCONSIN.

No., &c.	Length.	Least circumference.	Antero-posterior diameter and transverse diameter of shaft 1½ inch below trochanter minor.	Antero-posterior diameter and transverse diameter of shaft 2 inches above posterior border of intercondyloid notch.	Latitudinal index.		Perimetral index.	Diameter of head.
					Proximal end.	Ulterior end.		
1. Right	19·25	3·75	1·08 × 1·58	1·25 × 1·67	·683	·748	·194	Imperfect.
1. Left	19·25	3·60	1·15 × 1·55	1·31 × 1·65	·742	·794	·187	
Means	19·25	3·67	1·11 × 1·55	1·28 × 1·66	·712	·771	·190	....

[1] The flattening of this bone latitudinally at centre of shaft is quite marked, giving the following measurements: Fore-and-aft diam. = 1·30; transverse diam. = 0·95; giving a latitudinal index = ·730. The shaft is triangular at this part. The expansion, wanting at the extremities, seems to have passed into this shape. A similar trait, it is noticeable, pertains to the most carinate of the bones from the mounds.

Though this mound is outside of the boundaries of the State of Michigan, it still belongs to the region of the Great Lakes, and the bones are interesting as having been associated with platycnemic tibiæ.

I add a general table (Table E), in which, for convenience of comparison, the means of the dimensions of the femora from the different localities, etc., are shown.

TABLE E.

MEANS OF THE DIMENSIONS, ETC., OF FEMORA.

Locality, &c.	Length.	Least circumference.	Antero-posterior diameter and transverse diameter of shaft 1½ inch below trochanter minor.	Antero-posterior diameter and transverse diameter of shaft 2 inches above posterior border of intercondyloid notch.	Latitudinal index. Proximal end.	Ulterior end.	Perimetral index.	Diameter of head.
Great Mound, Rouge River, Michigan	16·96	3·20	0·90 × 1·27	1·08 × 1·49	·718	·726	·187	1·64
Circular Mound, Detroit River, Michigan	18·26	3·32	0·98 × 1·29	1·13 × 1·62	·766	·699	·177	1·68
Fort Wayne Mound, Detroit River, Michigan	17·92	3·25	0·99 × 1·32	1·21 × 1·63	·746	·740	·195	1·63
Chambers Island Mound, Green Bay, Wisconsin. One tall individual	19·25	3·67	1·11 × 1·55	1·13 × 1·67	·712	·771	·190	....
4 Selected Neolithic, from Denbighshire, Wales. (from Busk's data)	18·20	3·50	0·89 × 1·32	....	·673	....	·192	....
3 ordinary English (from Busk's data)	......	....	0·90 × 1·15	....	·780	....	......	...
Young Man (White)	17·38	3·47	1·06 × 1·14	1·39 × 1·43	·929	·968	·200	1·95

It will be seen that the indications are that the compression in the femora from the Michigan tumuli, excepting the Fort Wayne Mound, is accompanied, in general, by a reduction in the bulk of the bone. The mean perimetral index is inferior to that of the normal modern femur.

The size of the head is also a point which is worthy of notice, the diameter of the head in the Michigan femora as compared with that dimension in modern bones being greatly inferior, or about as 1·65 to 1·90. Though there are individual exceptions to this, they are too infrequent to change the general result, which is about as here given. That this cannot be accounted for by the relative shortness of the stature (the height of the skeleton in the older burials is markedly inferior, as computed by me, ranging perhaps from

4′,, 10″ to 5′,, 5″), is apparent on comparison with the modern femur. For instance, in that of the white young man already mentioned, though he was only about 5 feet 3 inches in height, we find this diameter of the full size, or exactly 1″·95.

It cannot but be supposed that this feature is in some way connected with the flattening of the femur, and that both peculiarities are related to the platycnemism of the tibiæ, of which they are an accompaniment. In every case these compressed femora (including the Neolothic) are associated with platycnemic tibiæ. They, doubtless, have their origin in a common cause, though in what direction we may look for this is not yet quite clear. As Herbert Spencer has so admirably said: "Localization of function is the law of all organization whatever, separateness of duty is universally accompanied by separateness of structure." Prof. Busk's inference that platycnemism may be due to the greater freedom and increased prehensile faculty of the foot is entitled to much consideration. It may be possible, however, that in the causes assigned for this and other variation of the osseous structure, the dietary of those primitive people has been too much overlooked.

An apparently greater length and obliquity of the neck in those ancient femora, did not fail to receive my attention. But while to the eye the general form of this part may have the appearance of possessing the characteristics mentioned, on my subjecting it to measurement I could not detect their existence to any appreciable extent. I also confess to being slow to attach much importance to characters which, as is well known, are so given to varying, the neck assuming a different length and direction not only in the opposite sexes, but at various ages, and under other dissimilar circumstances. For instance, in the extremely aged it not seldom assumes a horizontal position. Yet I would not pronounce that this is not a point worthy of further investigation.

The femora are, in general, I find, more or less decidedly carinate, with sharp and prominent linea aspera; and a section taken at or toward the centre of the shaft would present a triangular form, the apex pointing backwards. A marked feature is the rather decided angle of the outer as well as the inner border at those points toward the extremities which I have denoted as possessing the lateral expansion or fore-and-aft compression, and which in ordinary thigh bones have a rounded or almost circular form. In several of the bones the cortical substance is uncommonly thickened; but as this is an inconstant feature I have not dilated upon it.

These brief observations are offered as supplementary to my former contributions to the history of the mysterious mound-people, with the confidence that my remarks will be received with the increasing interest which the subject is yearly developing.

SOME OBSERVATIONS ON THE ORBITS OF THE MOUND CRANIA. By HENRY GILLMAN, of Detroit, Michigan.

OF late an impression has existed that the orbits of the Mound Crania, unlike the orbits of other races, present at base a circular instead of a quadrilateral outline. This idea had, I believe, its origin in a statement to that effect made by the late Mr. J. H. Foster. His assertion was founded, I think, on two crania from a mound in Illinois; but Mr. Foster gives no particulars, if I mistake not, and certainly no measurements to sustain the announcement of this very remarkable deviation from the ordinary type of orbit.

As the skulls in question had been deposited in the Academy of Science, at Chicago, and were reduced to ashes in the celebrated conflagration which overwhelmed that city in 1871, we are without the means of reference to the source of information, Mr. Foster, to the great loss of Science, unfortunately dying before the publication, in 1873, of his last and most valuable contribution to American Ethnology.

It is with some hesitation that I approach the subject. But I feel compelled to state that repeated evidence from the mounds in Michigan and elsewhere has unequivocally failed to confirm the opinion that the orbits of the mound crania have the peculiarity ascribed to them. Nor on questioning others who have had opportunity for ascertaining, have I been able to arrive at any other conclusion than that the orbits of such crania did not differ perceptibly from the ordinary quadrilateral form.

It may be assumed, in explanation, that the two skulls from Illinois are exceptional, and represent the true or typical cranium of the Mound Builder. I am quite aware of the very high claims which have been advanced for those skulls; yet I cannot easily be convinced that we have not exhumed from our Michigan mounds crania of as great or even greater antiquity, and as fully sustaining the title to being considered Mound Builder skulls.

That crania are not unfrequently met with having the angles of the orbital circumference more or less rounded or partially obliterated, there is no doubt; yet, I believe, there always remains the general quadrilateral aspect, perhaps somewhat interfered with by the deep, almost semicircular curve of the anterior border of the orbital plate of the malar bone; and a resort to measurement will usually prove such orbits to be no nearer to affording the circular base, than those in which the angular form is more pronounced; *i. e.*, the horizontal and perpendicular diameters generally approximate as closely in the latter as in the former.

It has been well remarked that "measures are the inflexible judges placed above all opinions supported only by imperfect observations." Though it might be supposed that this is not a subject for mere measurement, but a case in which the eye is fully competent to decide, in this discussion I prefer presenting some of the dimensions of the orbits of mound crania, comparing them with those of modern skulls of various races. I only regret that I cannot present fuller tables. Many of the older skulls are too imperfect (some of them being mere fragments) to give the necessary dimensions, and, besides, at first, I contented myself with simply observing, in this investigation, as to whether the form of the orbit was quadrilateral.

TABLE A.

DIMENSIONS, ETC., OF ORBITS, MOUND CRANIA.

Locality, etc.	Length.		Height.		Malar diameter.	Interorbital space.	Altitudinal index.		Perimetral index.		Interorbital index.
	Right orbit.	Left orbit.	Right orbit.	Left orbit.			Right orbit.	Left orbit.	Right orbit.	Left orbit.	
Circular Mound, Detroit River, Mich.	1·70	1·70	1·40	1.40	3·90	0·84	·823	·823	2·38	2·38	·215
Western Mound......................	1·89	1·83	1·50	1·50	4·34	0·88	·793	·819	2·83	2·74	·203
Fort Wayne Mound, Detroit R., Mich. Skull 5................................	1·78	1·80	1·37	1·38	4·04	0·70	·769	·766	2·44	2·48	·173
Means.	1·79	1·77	1·42	1·42	4·09	0·81	·795	·803	2·55	2·53	·197

The skulls referred to in Table A are ancient and of the orthocephalic range, the cephalic indices being respectively ·741, ·767, and ·773; the circumferences are 19·77, 21·65, and 20·90, while the position of the foramen magnum, in each case, is well advanced, affording the respective indices of ·514, ·507, and ·502. The altitudinal indices are inferior.

TABLE B.

DIMENSIONS, ETC., OF ORBITS, CHAMBERS ISLAND MOUND, WISCONSIN.

No., etc.	Length.		Height.		Malar diameter.	Interorbital space.	Altitudinal index.		Perimetral index.		Interorbital index.
	Right orbit.	Left orbit.	Right orbit.	Left orbit.			Right orbit.	Left orbit.	Right orbit.	Left orbit.	
1.* Imperfect	1·48	1·42	....	....	3·76	....	....	....	....	....	....
2	1·74	1·70	1·27	1·33	4·02	0·73	·729	·782	2·21	2·26	·182
3	1·56	1·56	1·35	1·35	3·65	0·76	·865	·865	2·11	2·11	·208
5.†	1·67	1·70	1·30	1·34	4·00	0·84	·778	·788	2·17	2·28	·210
Means	1·65	1·65	1·31	1·34	3.89	0·78	·791	·813	2·16	2·22	·200

I have placed in a separate table the dimensions of the orbits from the Chambers Island Mound, Wisconsin, as though they are those of mound skulls, but one of these (No. 2) falls within the orthocephalic category, the others being brachycephalic.

In this connection reference may be made to Table IV, of my paper on the "Investigation of the Fort Wayne Mound, Michigan," as though so very imperfect, some of the relative proportions will be found to be of interest, and the skulls of which the orbital dimensions are given are of the orthocephalic index. [Page 323.]

TABLE C.

DIMENSIONS, ETC., OF ORBITS, INDIAN CRANIA.

Locality, etc.	Length.		Height.		Malar diameter.	Interorbital space.	Altitudinal index.		Perimetral index.		Interorbital index.
	Right orbit.	Left orbit.	Right orbit.	Left orbit.			Right orbit.	Left orbit.	Right orbit.	Left orbit.	
1. Wyandotte Indian, Michigan	1·89	1·86	1·48	1·54	4·21	0·63	·783	·828	2·80	2·86	·149
2. Indian from Mackinac, Mich	1·68	1·72	1·39	1·37	3·91	0·63	·827	·796	2·33	2·36	·161
Means	1·78	1·79	1·43	1·45	4·06	0·63	·805	·812	2·56	2·61	·155

*Omitted from means. †No. of Skull in Series.

In Table C the measurements of the orbits of two Indian skulls are shown. The first of these skulls is that of a Wyandotte Indian, and was taken from land (Flat Rock, Michigan), which had been occupied by the White man for over sixty years. The second, from Mackinac, Michigan, is supposed to date back about 100 years. An iron hatchet and a lead pipe were found in the grave with it. Both are strictly brachycephalic crania.

TABLE D.

DIMENSIONS, ETC., OF ORBITS, MODERN WHITES.

Nationality, etc.	Length.		Height.		Malar diameter.	Interorbital space.	Altitudinal index.		Perimetral index.		Interorbital index.
	Right orbit.	Left orbit.	Right orbit.	Left orbit.			Right orbit.	Left orbit.	Right orbit.	Left orbit.	
White woman, New York; nationality unknown	1·60	1·57	1·23	1·27	3·73	0·64	·769	·809	1·97	1·99	·171
Swede; old woman; teeth gone; alveoli absorbed	1·60	1·57	1.35	1·37	3·72	0·77	·843	·872	2·16	2·15	·270
Irishman. (Prize fighter.)	1·62	1·52	1·42	1·45	3·90	0·86	·877	·954	2·30	2·20	·221
Perfect young man, New York; nationality unknown	1·55	1·50	1·60	1.55	3·68	0·65	1·032	1·033	2·48	2·32	·177
Means	1·59	1·54	1·40	1·41	3·76	0·73	·880	·917	2·25	2·19	·210

In Table D are shown the dimensions of the orbits of four modern White individuals, two women and two men. In these the excess of the mean altitudinal index as compared with the same index in the other tables is at once seen to be the leading characteristic. In other words the means of the horizontal and perpendicular diameters approach much nearer an equal length in these orbits than in the others. This result is palpably due to a marked inferiority in the length of the orbit of the Whites, and perhaps also to a slight general increase in its height.

The minimum length pertaining to the Whites, the maximum length is reached by the Indian, the mound orbits holding an intermediate position in this respect. In the comparison of the altitudinal indices, the Whites presenting the maximum, we find the mound orbits possessing the minimum, that index in the Indian being but slightly in advance of that of the mound orbits.

In point of size, as expressed by the perimetral index, the orbits of the Whites are greatly inferior to both the Indian and the mound orbits, if we except the Chambers Island group, which in this respect is slightly inferior to the Whites.

The Indian orbits have the smallest interorbital space, the mound orbits largely exceeding the Whites in this dimension. In the interorbital index, which expresses the relation held by the distance between the inner angles to that between the outer angles of the orbits (malar diameter), the Indian occupies decidedly the lowest range, the orbits of the other crania approximating in this respect, the Whites slightly leading the mound orbits. This seems to indicate the greater angle at which the mound orbits are set.

The size of the right as compared with that of the left orbit, contrary to what might be expected, is frequently inferior. This, however, with a single and trifling exception, is found to be confined to the Indian and mound orbits. Where there is a difference in the lengths of an individual's orbits, we often find the orbit in which the deficiency occurs compensated by having a superior height.

In truth, however, it is hardly fair generalizing to any great extent from material so limited. Yet most of the points made would seem to be well taken; and I have refrained from advancing others which in the course of the investigation have been suggested to me, till confirmed by the study of additional material.

Investigation of the Burial Mound at Fort Wayne, on the Detroit River, Michigan. By Henry Gillman, of Detroit, Michigan.

The mound, of which with its contents a description is here given, is situated on the north or Michigan bank of the Detroit River, a few hundred feet from the shore-line, and within the limits of Fort Wayne, occupying the southeast corner of the Parade Ground, and being immediately westward of the fort proper.

Though the permission of the War Department to examine the mound had been obtained by the Detroit Scientific Association in the latter part of the Autumn of 1875, various circumstances occurred to delay the investigation, which was not commenced until May 22, 1876. The work was conducted by the Association, several members specially interested participating, under the immediate direction of the writer, and, agreeably to the orders of the War Department, with the general supervision of the Post Commandant.

The courteous and efficient manner with which the objects of the work were facilitated on the part of General Stanley deserves more than merely a passing notice. The valuable aid rendered, and the kind personal interest and attentions constantly bestowed, throughout, by this distinguished soldier and accomplished gentleman, as well as by the several members of his command, place the Association under the most grateful obligations, and call for unqualified praise and thanks. Nor of the representatives of the Association whose assistance was of the most useful character, must I omit to mention the Messrs. Bela and C. B. Hubbard, and especially Mr. H. G. Hubbard, who was unremitting in his efforts.

The Structure at the date of this examination could hardly be supposed to present its original features. Though its location afforded it a certain kind of protection, it also exposed it to some dangers from which a more secluded site would have exempted it. There can be but little question that at least in height the mound is much reduced. Its rounded summit is at present but five feet above the general level of the parade ground. Almost circular, its east and west diameter, nearly seventy feet in length, is slightly in excess of its north and south diameter. This includes an outlying rim or border, ten to twelve feet in width and one foot above the general level, and which, surrounding the mound, has apparently been formed by the many years' washing down of the sand of which it is composed. Previous to the year 1870 the process of erosion must have proceeded with considerable rapidity. In that year attention was called to the process, and with the object of causing its arrest, the entire mound was sodded over, a small oak-tree being planted near the centre. A few oaks of no great age (probably not over fifty years) are scattered in proximity to the limits of the mound; but no tree grew on the structure with the exception of the single oak the planting of which we have mentioned.

The investigation was commenced by running a drift or trench, seven feet wide, east and west through the centre of the tumulus.

Within a foot or so of the surface small fragments of skulls and long bones with broken pottery were found, promiscuously mingled, being suggestive of a previous disturbance at this point of this place of sepulture. With these were exhumed the greater portions of a skull and pot, both in quite a fragmentary condition. The skull (No. 1 of series; See Table I) is very thin, and does not seem to be that of an adult. It is significant that this is the only one of the crania which comes within the brachycephalic range.

Extending from within a foot of the surface to the depth of $2\frac{1}{2}$ feet, we found a belt of charcoal of over a foot in width, running northward and southward across the mound, a little to the westward of the centre. This appeared to be the remnants of the trunk of a burnt oak; and with the exception of an isolated instance of a fireplace a short distance to the west of this, no other marked indication of fire having been used on the spot occurred, although there were occasional stains in the sand which may have been produced by the action of fire. A few pieces of charcoal were, however, not seldom found with each burial. Toward the centre of this belt of charcoal, at the depth of 2 feet, was exhumed skull No. 2. This fragmentary, roof-shaped cranium falls within the dolichocephalic range, presenting the cephalic index of ·725, and has prominent superciliary ridges and traces of an epactal bone. Beside it was found what may be called a chisel, made from apparently a species of grindstone. The implement is, I think, the only one of the kind which has been taken from the mounds in this vicinity, and is neatly made. It is nearly 5 inches in length, and is $\frac{3}{4}$ of an inch wide at the centre, the thickness at the same point being nearly $\frac{1}{2}$ an inch. It is formed by being ground square on the sides, and it gradually tapers to the extremities, one of which remains blunt and unfinished, while the other, though injured from use, had been brought to an effective, wedge-like blade.

Further toward the centre of the mound, at the depth of $2\frac{1}{2}$ feet, four crania with other bones were exhumed, each individual being provided with a burial vase or pot, probably for food, and the remains being massed within a very small space. As far as could be seen, the bodies had been interred in a crouching posture. In one instance this was quite apparent, the body lying on its side with the head to the east. A small stone axe or hatchet, slightly polished, the cutting edge broken from use, lay beside; and the accompanying burial vase is of a finer quality than usual, resembling the pottery from the mounds farther south (Ohio and Missouri). It is marked with an evenly indented cord pattern, the intersecting lines being laid on at wide and regular intervals, so as to form large diamond-shaped figures. With the other interments of this group the only stone implements discovered were a carved knife, very rudely chipped from flint, and a so-called sinker made from a flat piece of shale, ground smooth on the sides, edges, and ends, and perforated at one extremity—the perforation having plainly been made by boring from opposite sides.

At 2 feet below the surface, and at 3½ feet northward and 4 feet eastward of the centre were encountered the fragments of a skull (No. 3), much distorted from the pressure of the sand, with which burial had been deposited four well-wrought flint arrowheads and a lance head of the same material. The lance and arrowheads have the peculiar spiral twist, designed to give a rotary motion to the missile. In the latter is this peculiarity so finely developed, that on the front or pointed end being presented to view, the continuous curved line of the chipped edge (resembling a greatly elongated letter S, the arrow point at the centre) exhibits a perfect representation of Hogarth's line of grace and beauty.

At 11 feet east of the centre and 33 inches in depth, the cranium No. 5 and other bones were exhumed. This rather coarse roof-shaped skull slopes decidedly in the parietal region from the central longitudinal ridge, while the sides are vertical. It is the most perfect, and appears to be one of the largest, if not the largest of the skulls exhumed here. The cephalic index is ·773, the altitudinal index being inferior, or ·719, thus placing it within the orthocephalic range. The position of the foramen magnum is well advanced forward, in this respect sustaining the important distinction to which on former occasions I have called attention. The index of the foramen is in this instance ·502, and as the skull is well preserved, and free from any flattening or distortion, there can be no question as to the value of the evidence thus reiterated. The other dimensions, etc., are given in the annexed table. The body appeared to have been buried in a crouching but upright posture, subsequently falling slightly to one side, the head to the east. The pot placed with it also slanted in the same direction. It is of the ruder description of mound pottery, the ornamentation mostly consisting of indented lines inartistically produced by the indiscriminate application of a cord. As usual in our mounds, the base is rounded or semi-conical, so that the vessel is incapable of standing upright without support. It is the only specimen of the ceramic art which we were able to secure in a tolerably perfect condition, but a small portion of the rim at the mouth being wanting. A thin crust of a blackish substance adhered to the sides and bottom, within.

There now occurred an intermediate space, without a burial. Nothing but the pure fine sand of the structure was met with till we reached the depth of 3 feet 2 inches, when, at the exact centre of the mound, we uncovered a burial vase containing the remains of a human body mostly reduced to ashes. This proved to be one of the most decided isolated instances of cremation which has been met with in our mounds. Though it had crumbled to pieces this cinerary urn remained in position, upright, and holding in place the contents. It is of the common pottery of the mounds, and is ornamented with the ordinary cord pattern; and as the base of the urn was not reached short of the depth of 4 feet, this vessel, which was about 8 inches in diameter, must have been 10 inches in height. Incompletely consumed fragments of the leg bones indicated the incinerated subject to have been marked with platycnemism. Within the vase had been deposited a copper knife, of which only two small portions almost destroyed by oxidation, one of them being the point, remained. Like all other articles made of copper which have been recovered from our mounds, this knife had unquestionably been formed of the virgin metal from Lake Superior. If we may pronounce from the evidence afforded by this relic, the tool had simply been rudely hammered into shape. Two pieces of a Pyrula shell were also taken from the urn. The shell, the fragments of which were too far decayed to testify as to whether it had been worked or not, must originally have been brought from the sea-coast.

Further excavation revealed no other interments beneath the depth here attained. The clear fine sand, of a pale yellowish hue, bleaching to a grayish white on exposure to the air and sunshine, continued to the depth of six feet, when a hard compact stratum of ferruginous-like sand, half a foot in thickness, was reached. It was evident that this had never been disturbed. We had, therefore, penetrated below the level of the interments, and, in fact, below the level of the surrounding land. The cut was, however, deepened to $7\frac{1}{2}$ feet in order to satisfy all doubt on the subject. For the remaining depth, only coarse sand and gravel of the drift (pebbles intermixed) were met with.

From what we have here described, together with what the remainder of the excavation revealed, it would appear that the single case of cremation—the ashes, etc., deposited in burial-urn at the exact centre of the mound, at a depth greater than that of any of the other inhumations—constitutes the first interment in this place of sepulture; and that, therefore, in all probability, the remains are those of the individual in whose honor the mound was originally erected.

The work was continued by opening, about $2\frac{1}{2}$ feet to the eastward of the centre of the mound, a trench running south from the first cut, thus giving the entire excavation a T-shape.

At a foot below the surface, and but a short distance south of the first excavation, the remains of a pot and a skull with other bones, all in a fragmentary condition, were dug out. The skull, No. 6 of series, is evidently of less antiquity than the other crania from this place. The superciliary arches are not prominent. It is too imperfect to afford the usual dimensions. At six feet south of the centre, and two feet below the surface, two mingled burials, one on top of the other, also much decayed, occurred. There was no pottery nor stone implement found with these last relics. South of centre $14\frac{1}{2}$ feet, and $1\frac{3}{4}$ foot from the surface, pieces of a skull, much decayed, with other bones beneath, were taken out. To the depth of 3 feet a few scattered fragments of human bones were encountered; beneath which no further interments were met with, though the excavation was carried below the level of the base of the mound.

A trench similar to the other cuts we have described was now opened northward from the north line of the east and west excavation at its intersection with the centre of the mound,—thus giving the entire of the work the form of an irregular cross. Within from one foot to two feet of the surface broken pottery and pieces of charcoal with a quantity of a reddish-colored sand, the color supposed to be the stain produced by the remains of some pigment, were uncovered. About seven feet northward of the centre of the mound, and at the depth of 20 inches we reached an interment in which the body had been placed in a much-crouched semi-sitting posture. The leg bones were drawn over the arm bones, and the skull (No. 7), which had fallen forward partly on its face, lay, base upward resting upon the feet. Most of the bones were badly decayed. These last were all on, or slightly to eastward of, the east line of this cut.

As no other relics were discovered on this side of the mound, work was resumed on the south side, by widening the cut to the westward, it having, in the first instance, been made somewhat to the eastward of the centre. This necessitated the removal of the oak-tree already referred to as having been planted on the mound, and which stood about 4 feet south of the centre. Beneath this tree, at 2½ feet from the surface, were found two arrowheads chipped from stone, of the rudest workmanship, and a little to the east of these, a few bones of a bird. Immediately beneath, at the depth of 3 feet, we reached a pot finely and very regularly marked with the cord pattern, giving to it quite a distinctive appearance. Like all the others, with the single exception we have noted, it was in a fractured condition. About one foot to the northward of this pot and 2 feet from the surface, portions of a coarser pot, somewhat similarly marked, occurred. A few small pieces of human bones, much decayed, were found near by. The three skulls, Nos. 8, 9 and 10, with fragments of other bones, and a broken pot of a different pattern, were next disinterred, at that part of the mound 6 feet to west and 7 feet to south of the centre, being about 2 feet below the surface. The only other relic found associated with them was the greater part of a thick Unio shell. All were within an area of 2 feet square.

Further investigations of the mound afforded no results worthy of mention, and operations were brought to a close.

In Table I, [page 318] are shown such of the dimensions, etc., of the crania as could be obtained. It is to be regretted that their decayed condition so greatly interfered with procuring these. I have only to add with regard to those skulls and a number of others which crumbled to pieces in their removal, that their general similarity of outline was so apparent, it was at once recognized by me; and, also, that previous to making a single measurement, I pronounced them as not belonging to the broad or brachycephalic class, the only instance of the occurrence of a skull of that type being the very thin fragment (No. 1) already alluded to as a near-surface burial, and probably not adult. All the others either fall within the orthocephalic category, or in the

TABLE I.

DIMENSIONS, ETC., OF CRANIA FROM MOUND AT FORT WAYNE, DETROIT RIVER, MICHIGAN.

No.	Approximate relative capacity.	Circumference.	Length.	Breadth.	Height.	Breadth of Frontal. Maximum.	Breadth of Frontal. Minimum.	Latitudinal Index.	Altitudinal Index.	Index of Foramen magnum.	Frontal arch.	Vertical arch.	Parietal arch.	Occipital arch.	Longitudinal arch.	Length of Frontal.	Length of Parietal.	Length of Occipital.	Zygomatic diameter.	Length of Foramen magnum.	Breadth of Foramen magnum.	REMARKS.
1.	....	20·00	6·75	5·55	....	4·20	3·50	·822	....	....	....	....	....	....	....	4·75	5·00	....	....	....	....	Very thin. Probably not adult.
2.	....	21·00 ?	7·35	5·33	....	4·20	3·62	·725	....	....	11·50	11·80	12·80	11·40 ?	....	5·30	....	....	....	....	....	With indication of epactal bone.
3.	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	4·70	4·95	....	....	....	....	Distorted from pressure of sand.
4.	....	....	....	....	....	4·60	3·98	....	....	....	....	....	....	....	....	5·30	....	....	....	....	....	
5.	18·37	20·90	7·37	5·70	5·30	4·57	3·98	·773	·719	·502	11·37	12·80	13·30	11·25	14·77	5·14	4·82	4·80	5·35	1·45	1·25	Appeared to be the largest of the skulls.
6.	....	....	....	....	....	4·20	3·65	....	....	....	....	....	....	....	....	5.10	....	....	....	....	....	
7.	....	19·90	6·77	5·27	....	4·63	3·72	.778	....	....	....	....	....	....	....	4·80	4·87	....	....	....	....	
8.	....	....	7·20	....	4·63	....	....	....	·643	....	10·80	11·80	12·00	10·00	....	4·95	4·55	....	....	....	....	} Associated.
9.	....	....	....	5·08	....	4·40	....	....	....	....	....	11.92	12·90	9·40	....	....	5·00	....	....	....	....	} Associated.
10.	....	....	....	5·60	....	....	....	....	....	....	....	....	....	....	....	....	....	4·10	....	....	....	} Associated.
Means.	18·37	20·45	7·08	5·48	4·96	4·40	3·74	·774	·681	·502	11·22	12·08	12·75	10·51	14·77	5·00	4·75	4·45	5·35	1·45	1·25	

case of one (No. 2), as already stated, even enter the limits of the dolichocephalic range. (This is according to the classification adopted by Dr. Thurnam and Prof. Huxley.) They are decidedly roof-shaped, with vertical sides; the forehead is retreating, and generally the superciliary ridges are unusually prominent, while the length seems largely due to the protuberant occipital. The only skull from which it could be obtained, No. 5, gives the foramen magnum a forward position, the index being ·502. They are not of extraordinary thickness.

Of the bones of the arm, but three were perfect, a humerus, an ulna and a radius. The two former belonged to the burial No. 5, the radius with Nos. 8, 9, and 10. The humerus, which was not perforated (but one, a fragment, presented this characteristic), afforded a length of 13·0 inches, the least circumference being 2·73 inches. The same dimensions respectively of the ulna and radius are 11·25 by 1·25, and 9·40 by 1·40. In this individual, No. 5, who, I compute must have had a stature of about 5 feet 10½ inches, or exactly 70·54 inches, rather excessive for the Mound Builders, the lengths of the femora are 19·4 and of the tibiæ, 16·65 inches. The relations of these bones are shown by the following indices: Humerus taken as = 1·000; Ulna = 0·865; Radius = 0·723. Femur taken as = 1·000; Tibia = ·858. This, excepting the radius, which is relatively short, is largely in excess of the Indians of the mounds (as given by Wyman), and who, in their turn, exceed the Whites; and thus, in this respect the individual from the Fort Wayne Mound more closely approaches the simian proportions. Wyman dwells on the importance of those measurements, referring to the valuable investigations of Gould, Lawrence, Broca and others, who have pointed out the greater length of the arm in the modern Indian and especially in the negro, as compared with the Whites. And here I consider it of importance to present also a comparison of the proportion which the arm or rather the sum of the lengths of the humerus and ulna bears to the stature, in the instances mentioned. These I have deduced to be as follows:

INDIANS FROM THE MOUNDS:

Stature taken as = 1·000.
Arm (or Humerus + Ulna) = 0·353.

WHITES:

Stature taken as = 1·000.
Arm (or Humerus + Ulna) = 0·348.

BURIAL NO. 5, FORT WAYNE MOUND, MICHIGAN:

Stature taken as = 1·000,
Arm (or Humerus + Ulna) = 0·343.

We here perceive the unexpected fact that while the arm of the Mound Indians as compared with their stature is in excess of that of the Whites, the latter slightly exceed in this respect the individual from the Fort Wayne Mound. This, in view of the former result, is worthy of consideration, though affording only the evidence in the case of a single individual. The only instance except that of No. 5 in which the bones were perfect enough to afford the stature, gave that quantity as 62·5 inches. This was deduced from Burial No. 8.

In Table II [page 321] I present the results of the measurements of the Femora from this mound. Where so mentioned, the numbering corresponds with that of Table I, so that, generally, throughout the tables, the same number refers to the same individual. The letters R and L denote respectively that the bone belongs to either the right or the left side.

In general the femora are decidedly carinate. In massiveness they slightly exceed the normal English standard, affording the mean perimetral index of ·195. But their most marked peculiarity is the singular antero-posterior compression toward both extremities of the shaft, to which I have called attention in a former paper on the subject. While not developed to the extreme shown in the femora from the Detroit and Rouge River Mounds, it is still, it will be seen, too pronounced not to be considered of importance. As already shown, in normal White femora a section of the shaft at the points indicated would present a circular form, more or less irregular in outline; in the examples from the Fort Wayne Mound the mean of the fore-and-aft diameters of the shafts 1½ inch below the trochanter minor is ·746 of the mean of the transverse diameters, that being the latitudinal index; while similar measurements taken at 2 inches above the posterior border of the intercondyloid notch afford a mean latitudinal index of ·740.

The dimensions, etc., of the associated tibiæ are given in Table III. [Page 322.]

TABLE II.

DIMENSIONS, ETC., OF FEMORA FROM FORT WAYNE MOUND, MICHIGAN.

No., &c.	Length.	Least circumference.	Antero-posterior diameter and transverse diameter of shaft 1¼ inch below trochanter minor.	Antero-posterior diameter and transverse diameter of shaft 2 inches above posterior border of intercondyloid notch.	Latitudinal index.		Perimetral index.	Diameter of head.
					Proximal end.	Ulterior end.		
1. R. Carinate		3·52	1·04 × 1·32	1·28 × 1·60	·787	·800		
1. L. Carinate		3·28	0·95 × 1·32	1·24 × 1·56	·719	·794		
2. R. } Found near skull No. 6			0·90 × 1·25		·720			1·70
2. L. }			0·92 × 1·25		·736			
3. R. } Belong with Skull No. 3		3·23	0·95 × 1·35	1·00 × 1·42	·703	·704		
3. L. } Carinate		3·42	1·00 × 1·35	1·05 × 1·50	·733	·656		
4. R. Hardly carinate		3·23	1·00 × 1·24		·806			
Unknown. Hardly carinate			1·04 × 1·37		·759			
5. R. } Belong with Skull No. 5	19·40	4·10	1·27 × 1·47	1·36 × 1·82	·864	·747	·211	1·96
5. L. } Carinate and very massive	19·40	3·95	1·26 × 1·44	1·42 × 1·85	·875	·767	·204	1·99
6. R. } With Skull No. 6.		2·50	0·72 × 1·08		·666			
6. L. }		2·55	0·73 × 1·05	0·98 × 1·40	·695	·700		
7. R. } Belong with Skull No. 7		3·43	1·02 × 1·35		·755			
7. L. } Carinate		3·50	0·98 × 1·40		·700			1·75
8. R. }	17·20	3·15	0·95 × 1·23	1·33 × 1·76	·772	·756	·183	1·65
8. L. } With Skulls Nos. 8, 9 & 10		3·38	1·03 × 1·54		·668			1·67
9. L. }			1·26 × 1·52		·829			
10. R. } Carinate			0·94 × 1·29		·728			1·67
X. Unknown. Belongs with tibia so marked	15·70	2·27	0·83 × 1·25		·664		·145*	1·65
Means	17·92	3·25	0·99 × 1·32	1·21 × 1·63	·746	·740	·195	1·63

* Omitted from means as probably not being adult.

TABLE III.

DIMENSIONS, ETC., OF TIBIÆ FROM FORT WAYNE MOUND, MICH.

No., &c.	Length.	Transverse diameter, proximal end.	Least circumference.	Antero-posterior diameter and transverse diameter of shaft.	Perimetral index.	Latitudinal index.	Remarks, etc.
3. Belongs with Skull 4	....	....	....	1·12 × 0·82	....	·732	
×. Unknown belonging	....	....	....	1·10 × 0·76	....	·691	
4. R. } Belong with Skull 4.	....	....	3·20	1·36 × 0·87	....	·639	
4. L. }	....	....	3·23	1·37 × 0·90	....	·656	
5. R. } Belong with Skull 5.	16·65	3·20	3·46	1·62 × 1·13	·192	·697	} Heavy and massive bone.
5. L. }	16·65	....	3·55	1·66 × 1·13	·213	·680	}
6. R. Belongs with Skull 6	....	....	2·38	1·10 × 0·76	....	·691	
7. R. } Belong with Skull 7.	....	....	3·12	1·46 × 1·02	....	·698	
7. L. }	....	....	3·18	1·40 × 0·90	....	·642	
8. R. }	....	....	2·52	1·12 × 0·86	....	·759	
8. (?) L. } With Skulls Nos. 8, 9 & 10.	....	....	2·60	1·22 × 0·72	....	·590	
9. R. }	....	....	....	1·22 × 0·78	....	·639	
Means.	16·65	3·20	3·03	1·31 × 0·89	·202	·676	

The platycnemism here shown is not of the most extreme form, its maximum development having the index of ·590, its minimum ·759, or the mean index being ·676. The only perimetral index obtainable (No. 5) is somewhat in excess of the normal English standard, being ·202. And here I am obliged, from the examination of this as well as a quantity of other material, to state that the platycnemism of this region is not invariably of the "anterior" form, as I had been led to conclude; but is frequently decidedly "posterior," while occasionally it partakes of a combination of both forms: *i. e.*, the expansion is in both directions, a form of the characteristic which, for convenience, may be termed "intermediate," or "antero-posterior" platycnemism. In the "posterior" platycnemism the rear angle is frequently nearly as sharp as the angle in front. As in the Rouge River and Detroit River Mounds, the extreme cases are accompanied, in general, by the thickening of the cortical substance of the bone fore-and-aft, as well as by the sabre-like curvature of the shaft.

I here present another table which, however imperfect, is not without interest and value when compared with the results which I have obtained from other material.

TABLE IV.

DIMENSIONS, ETC., OF ORBITS, FORT WAYNE MOUND, MICH.

No. of Skull.	Length.		Height.		Malar diameter.	Interorbital space.	Altitudinal index.		Perimetral index.		Interorbital index.
	Right orbit.	Left orbit.	Right orbit.	Left orbit.			Right orbit.	Left orbit.	Right orbit.	Left orbit.	
3.	......	1·55	....	1·32	....	·68	....	·851	....	2·35	....
4.	1·55	1·45	....	......	3·97	·99	....	......	....	......	.249
5.	1·78	1·80	1·37*	1·38	4·04	·70	·769[1]	·766	2·44[1]	2·48	·173
6.	......	1·53	....	......	....	......	....	......	....	......	....
7.	1·60	1·50	....	......	3·77	·87	....	......	....	......	.230
8.	......	1·53	....	......	....	·75	....	......	....	......	....
10	......	1·60	....	......	....	·63	....	......	....	......	....
Means.	1·64	1·56	....	1·35	3·92	·77	....	·808	....	2·41	·217

*Omitted from means, as it would give false estimate.

In this table I have endeavored to show the dimensions and relations of such of the orbits of the crania taken from the Fort Wayne Mound as the decayed condition of the bones permitted. There is nothing in these examples to establish the claim made for the mound skulls, that their orbits are not quadrilateral, but approach more nearly a circular form than do those of other races. In view, however, of the discussion by the writer in his paper on the orbits of the mound crania of more satisfactory materials, it is unnecessary to offer here any extended explanatory remarks in this direction.

In conclusion, I would dwell on the special interest pertaining to those relics, associated as they were in a single mound, and exhumed so that their relations are preserved.

No indications are found here, such as are seen so abundantly present in the Rouge River tumulus, of this structure having been used as a dwelling-place or point of observation. It is clearly a sepulchral mound, and as such I have named and classed it.

A further study and comparison of the relics may possibly disclose points of interest which I have overlooked; but I have endeavored to make this discussion of the remains as exhaustive as their important nature demanded, and the very limited time at my command would permit.

The age of the mound is undoubtedly not so great as that of the Rouge River mound. The most ancient interments in the latter have no representation in the Fort Wayne mound. I feel safe, from the evidences afforded, in placing the age of this mound, or the date of its earliest interment—the single instance of cremation—at or toward the close of the period indicated by those isolated cases of cremation which I have described as occurring in the Rouge River mound.[1]

The corroborative testimony is valuable. It affords us a standard, not to be despised, for the establishing of a comparative chronology for, at least, our own region, about which other facts may group themselves, thus building up a system which at length may obtain an importance entitling it to a more general recognition and application.

The Rouge River mound presents unquestionable evidence that

[1] See paper, "The Ancient Men of the Great Lakes," by Henry Gillman, read at the Detroit Meeting of Am. Assoc. for the Advancement of Science, Aug., 1875, pp. 327, 328. 329, of "Proceedings."

1°, There was a time when cremation was not practised here, as indicated by the chalky or lime-like concretions of the earliest interments.

2°, That the custom slowly grew up, or was gradually introduced, till, at length, it apparently largely prevailed; to be succeeded by,

3°, Burial in the crouching posture, as practised by those platycnemic men, who were generally marked by small orthocephalic crania; and which, in turn gave place to,

4°, Those later interments which with their accompanying relics, stone implements and rude pottery, and the entire absence of the tools or metals used by the White man, indicate a period, be it of greater or less remoteness, at least antecedent to the advent of the European to these shores.

To these divisions might be subjoined,

5°, That denoted by the remains of the various nomadic Indian tribes—the red man as known to the early colonists, and when the sites of now populous cities were an unbroken wilderness.

It scarcely remains necessary for me to add that I consider the Fort Wayne mound to represent the 2d, 3d, and 4th of the periods here mentioned. And, I would suggest, may not the scale here indicated prove to be already applicable in the case of a large number of the sepulchral monuments throughout our country, and thus become the entering wedge in reducing to order and system many of the apparently heterogeneous elements which so constantly thwart the American archæologist?

On some Fragments of Pottery from Vermont. By Geo. H. Perkins, of Burlington, Vermont.

Among the various articles illustrating the Archæology of Vermont which have been discovered in different parts of the state, pottery is comparatively rare, even in fragments, and of course entire jars are still more so. Indeed I cannot learn that more than half a dozen perfect specimens of earthen-ware, found within the limits of the state, are now in existence. More than this number have been found, but, through the carelessness or in-difference of the possessors, many have been destroyed. French Canadians are numerous about the shores of Lake Champlain, and find many of the flints, etc., left by former occupants of that region, and they rarely care enough for such things to preserve them, though they seem very willing to vex the soul of the collector by describing choice articles which they have found and thrown away, or destroyed. For this reason, articles of Archæological interest are far less numerous than they should be. From what I have heard of jars given to children to play with, or throw stones at, or otherwise destroyed; and from the number of fragments, which I have seen in some localities, I am led to believe that the ancient inhabitants of Vermont made and used no inconsiderable quantity of earthen-ware jars and pots, and that some of these were elaborate in form and style of ornamentation. Pottery found in New England, does not ever, I believe, bear any of those imitations of the head of man or animals so common in pottery from the Mississippi valley; this is certainly true of the Vermont pottery, for, no trace of any figure of a living object has ever been discovered on the earthen-ware found in that state. During the past few months numerous fragments of this earthenware have come into my possession, and though many of these are of rather small size, yet, as they bear markings of interest and sometimes indicate the form and size of the jar to which they originally belonged, I have thought it worth while to describe them with somewhat of detail. It will always be a source of regret to Archæologists that the primitive people of this country did not discover a stronger and more durable material of which to fashion their jars. I suppose the experience which I have had in Vermont is that of collectors elsewhere, viz.: finding what evidently was an entire jar when buried reduced, by the simple crumbling away of too loosely mixed material to a mere heap of fragments and sand. The material is always a mixture of clay with small angular fragments of quartz and feldspar sometimes plentifully mixed with scales of mica. The sharp angles and edges of these bits of stone show that large pieces were broken up for the purpose, rather than that fine gravel or small pebbles were used. In some pieces these bits of stone are visible on the outer and inner surfaces, but usually the jar was covered on both sides with a layer of fine clay. In one case there was so large a proportion of mica pounded into fine pieces mixed with the clay that the whole surface has a glistening appearance. This fragment showed no marks of fire and was of a light drab color, as are one or two other pieces, but most of the Vermont pottery is dark red or brown, or, in many cases, black. Fortunately the upper parts of the jars were made thicker and stronger than the lower spheroidal portion and it was upon these more enduring portions of their jars that the makers placed almost the whole of the ornamentation. These ancient potters seemed to prefer a flat surface to work upon when decorating their wares, for those most highly adorned bear the ornamentation upon flat, or flattish portions, and those that are globular, without any flattened upper part, bear no ornamentation except about the rim. Most of the jars found in Vermont are of comparatively small size. One specimen was found which held twenty quarts, but most are far smaller. In the ornamentation curved lines are rare, except those that are very short, small circular and crescentic figures occur more commonly, as also do triangular and rectangular figures, these latter, with straight lines arranged in parallel groups, being most common of all. In thickness the earthen-ware that I have examined varies from 12 or 13^{mm} (·5 in.) to less than 5^{mm} (·2 in.). The so-called "Indian relics" are found much more abundantly over the northwestern portion of Vermont than elsewhere in the state. The large islands and peninsulas in the northern part of Lake Champlain and the main shore from Alburg to Charlotte and twenty miles inland were most thickly populated, or frequently visited, at least it is through this part of Vermont that the greater number and variety of specimens have been found. The pottery to be presently described is mostly from Essex or not far from that town.

For convenience the pieces will be designated by number.[1] Beginning with the simplest form of ornament we have two pieces (Numbers 1 and 2) which probably came from the same jar, though this is not certain. Number 1 is a part of the rim. It is covered with rectangular figures deeply stamped in rows which reached around the jar, probably. Those of the upper row are high and narrow, and as one corner of the implement used in making them was much more strongly pressed into the clay than the rest, this corner, always the lower right hand one, and the adjacent sides are clearly defined, while the opposite is often not marked at all and

[1] Drawings of the several pieces described accompanied the paper, but the expense of reproduction prevents their being published in the present volume.—EDITOR.

never otherwise than faintly, so that at a little distance the appearance of the figures is that of right angled triangles. Below this row is a second of smaller and similar figures but of different proportions, for while the figures of the first row, varying somewhat in size, are about 5·5^{mm} (·2 in.) high and 4^{mm} (·15 in.) wide, that is are higher than broad, the figures of the second row are about 3^{mm} each way. The other piece (No. 2) is covered with the same figures, except that these are wider than high being 6·5^{mm} (·25 in.) high and 4^{mm} (·15 in.) wide. All of these figures are in horizontal rows, each row being separated from that below it by a short space and (by a less space) each figure is separated from those on each side. The figures are so deeply stamped and so near together as to give a decidedly neat appearance to the surface. The material is very dark, thick and coarse, though strong. Number 3, is of similar material and is covered with small, deeply stamped squares arranged very regularly in horizontal rows, which are 3 or 4^{mm} distant from each other, the squares themselves being of somewhat different sizes but on the average not far from 2·8^{mm} (·1 in.) high and broad. They are as near together as possible without interfering with each other.

Another piece (No. 4) bears similar rows of small figures; but, in this case, they are not rectangular but semicircular, or crescent-shaped, the lower edge being in all cases strongly curved, the upper sometimes straight but more often a little concave. The lower side is more deeply impressed. The figures are of two sizes and the larger are arranged in horizontal rows, while the smaller are in vertical rows. The larger figures are about 3^{mm} high and 3·9^{mm} wide (·12 in.$\times$·15 in.) while the smaller are half as large, or less in some cases. In Lubbock's "Prehistoric Times," page 162, fig. 151, is figured a piece of pottery from a tumulus in West Kennet which bears markings more nearly resembling those just described than any others I have seen.

Several fragments of the rim of a very finely formed, light colored jar (No. 5) bear markings of a still different form. The upper edge of the rim of this jar is very strongly recurved so as to project over the outer surface below. The whole surface is very smooth, and as the materials used in constructing the jar were very finely pulverized and well mixed, it is more compact than usual. There is no ornament on the inside except two narrow lines of short, confluent, curved marks just above the top.

The edge of the rim is covered with a closely set row of transverse grooves, or notches, which are narrow and bear a rounded depression, as of a small blunt point, on each end, and as these nearly or quite meet, the inside of each groove has somewhat of the form of a figure 8. Below the projecting upper edge are two rows of small closely set, irregularly crescent-shaped figures only slightly stamped into the clay and below these are rows of larger broadly crescent-shaped, or sometimes rounded triangular, figures more clearly marked. These figures are of quite unequal size, and more care seems to have been taken in forming the jar than in adorning it. The diameter of the top of the jar was not far from 102^{mm} (4 in.).

A small piece (No. 6), which probably shows only a portion of the design placed upon the whole jar, bears simply three rows of small but deeply sunken circular depressions about 1^{mm} in diameter. They were apparently made by pressing a blunt point into the soft clay. A part of a straight necked jar, of dark, hard material (No. 7), is covered with transverse rows of somewhat irregular figures not very deeply sunken. The inside bears around the top two rows of long, narrow figures below which the smooth surface bears regular raised lines, as if smoothed with an edge that was slightly notched. From the regularity of these lines and the equal distances between them it would appear that they were purposely made. They are 2 or 3^{mm} distant from each other and about two-thirds of a millimeter wide. The upper edge of the rim bears two rows of small squarish figures and a similar row runs about the upper part of the outer surface. A little below this is a continuous series of similar, but larger, figures and below this two more, each row being about 4^{mm} from that above. In all cases one side of the figure is more deeply stamped than the rest and in our Vermont pottery it is almost always the case that, when the ornamentation consists of figures rather than lines, one side is much more deeply stamped than the rest, the surface of the figure sloping from this up to the general surface of the jar. It is without a single exception, so far as I have seen, the same side on any given piece of earthen-ware that is thus most distinct, and in those that I have examined the left side is much more often the deepest, though sometimes it is the right side. Below the rows of rectangular figures, which are 1 or 2^{mm} across, are four rows of long narrow figures with the upper part of a fifth row. These are by no means equal in size, though the variation is not large. They are 4^{mm} long (·15 in.) and about one-fourth as wide. The edges are slightly scalloped and they are stamped very closely. The figures are somewhat oblique in their arrangement.

A large piece of the rim of another jar of very similar form bears quite a different style of ornament. This piece (No. 8) bears four sorts of figures. The inside is smooth, but around the upper margin are vertical rows of broadly crescent-shaped figures, four figures in a row, the whole forming a broad band made up of the rows placed side by side. The upper edge of the rim is quite strongly bevelled downwards, and is covered with small depressions made by a small, sharp point. These little figures are so arranged as to appear in transverse rows when the piece is held in one position, and in four rows running around the rim when held in a different position. The outer edge of the rim projects a little over the surface of the outside forming a sort of lip. Below this the surface is somewhat concave for a short distance, and below this it enlarges and, perhaps, becomes spheroidal. Around the upper part just below the lip are several rows of small crescentic figures placed so near each other that they together form one compound band of considerable width. Below this are two rows of irregularly oval depressions broadest below and narrowed above. These figures are about 3^{mm} (11 in.) wide and somewhat more in height. From certain of the figures oblique lines or grooves extend downward to meet a similar set of depressions some distance below where there are three rows of them. The connecting grooves are very regular and straight and appear to have been grouped in threes, each group being inclined in the opposite direction to that of the adjacent group, the alternate groups being parallel, thus forming a series of V-shaped figures around the jar, which are about 23^{mm} wide at the top.

One large piece (No. 9), from its form apparently a part of the middle of a rather large jar, is profusely covered with figures of several sorts. None are deeply stamped and some are quite indistinct. They are all in horizontal lines of more or less regularity, though there is less care and skill shown than in most of the other pieces described. Beginning with what was probably the upper side we have a row of chevron lines, and $6{\cdot}5^{mm}$ below are three rows of long, narrow grooves some nearly straight, others slightly curved. These series of figures are separated by slightly elevated lines. About 7^{mm} lower down is another series or band of still narrower vertical grooves 5 or 6^{mm} long. Below this are nine or ten not very regular rows of figures of somewhat various form. Some are quite peculiar, being zigzag in outline and resembling the impression which a small, crooked key would make upon any plastic substance. This figure seems to have been frequently used, for, I have seen it on quite a number of different pieces of earthen-ware. Many of the figures on the part of which we are speaking are more or less oblong, with each end curved, some are very much like a section mark (§). These last figures are, as has been said, arranged in rows, but the rows are irregular and very near together so that the surface of the clay seems to have been covered with figures without much attempt at orderly arrangement. On one piece of a rim the decoration consists of horizontal grooves which look as if first made with a blunt point, or something of the sort, and then scalloped by using the side of the same point and pressing it along the grooves, making a continuous series of rounded impressions. This piece (No. 10) is from a rather large jar and is thick, heavy and well burned. The upper edge is covered with transverse grooves which are scalloped like those below. This edge is bevelled outwards. On the inside of the rim is a series of lines drawn obliquely and parallel reaching downwards 17^{mm}. The lower part of the piece is unornamented and contracted to form a broad groove for a neck, below this it, in all probability, became spheroidal, for I have never seen a jar from this region which did not have this form of base.

Another piece (No. 11) is ornamented with similar scalloped lines, but they are vertical, not horizontal. They are not parallel in all cases, though some are so, but the most interesting feature of this specimen is the gradation from straight to curved lines which it affords. Some of the lines are straight and almost vertical, others straight but oblique, others are oblique in one direction for a short distance and then change their direction making a chevron, while others change their direction twice, or more, and others are almost regularly curved. I notice this particularly because curved lines, except such as are very short, are, as has been noticed, very rare on Vermont specimens. On a very few pieces not now described very regularly curved lines appear, but the best example of this is, perhaps, a large piece which appears to be a part of the side of a jar of considerable size (No. 12). About the middle of this piece is a row of deeply stamped oblong figures 5–10^{mm} distant from each other and about 6^{mm} long. From the top of each of these figures a pair of divergent curved lines extends to the upper edge of the piece. Some of these lines are straight, but most of them are curved. The lines are not continuous; but made up of dashes, or impressions, such as would be made by a short square point shaped like the end of a small nail.

A very handsomely marked specimen (No. 13) of dark, compact earthen-ware bears only oblique parallel grooves deeply marked. It is a part of the upper portion of a jar. The top curves outward to form a lip, below which the surface is concave for a short distance, below this it becomes convex. The upper edge, or rim, is marked with deep transverse grooves. The outer surface is wholly covered with oblique grooves 3·5^{mm} distant from each other. The grooves over the upper part of this piece are all in the same direction; but below are others running in an opposite direction.

There remain to be described portions of the upper part of three jars, all of which seem to have been of similar shape. An almost perfect jar of this form is figured and described in the "American Naturalist," Vol. 5, p. 14, fig. 1. This figure is reproduced below. As the form is more artistic than usual and quite peculiar, it may be worth while to speak of it in this connection.

The upper part of these jars is quadrangular, with the upper edge curving downward from corner to corner slightly. From the edge the sides slope inwards until they are suddenly and strongly contracted into a circular groove of some 30 or 40^{mm} (or an inch or more) in width. Below this the jar is again quadrangular, and the sides slope outwards, and, of course, downwards, for a short distance, and then the lower portion becomes regularly curved and spheroidal. This lower part occupies about a third of the whole jar and the quadrilateral space above another third, the remaining third being occupied by the groove and upper quadrilateral part.

The ornamentation of all these jars is elaborate and artistic, consisting chiefly of oblique, straight lines, variously grouped and differing in each case. In one case (No. 14) the surface of the upper part is ornamented with parallel grooves crossing each other in two sets so as to form a series of diamond shaped figures 11^{mm} high (·4 in.) and $7{\cdot}5^{mm}$ wide (·3 in.). The upper edge is crossed by deep, oblique grooves about 4^{mm} distant from each other. About the contracted portion or neck is a row of long figures, above widest, and rounded and tapering to a long point below. They are very deep, and below them is evidence of a second similar row. All below this is wanting. The appearance of this specimen, from the evenness and regularity of the lines and their depth and distinctness is very neat and elegant. Another piece of a jar that was, most probably, of the same general form, bears oblique lines like those just described, but they are much nearer together, the space between any two grooves being not much wider than the grooves themselves, and the different sets do not cross. The grooves are all straight and regular, but are less deep and wide than in the preceding case. The ornament on this consists of series of oblique lines, which extend across the specimen, and are separated from the next by three lines, running in the contrary direction to the lines which make up each series. Each of these grooves is about 1^{mm} (·04 in.) wide. The lower edge of the ornamented portion bears a row of deep rounded triangular depressions about $5{\cdot}5^{mm}$ wide and about two-thirds as high, and below this is the contracted portion or neck.

Two jars were made upon very nearly the same pattern, one of these is that figured on the preceding page and already referred to, the other is known only from fragments, but enough remains to show that it was essentially identical in form and ornament with it. It is a little more complex in the style of its markings. The inside is smooth, except a row of notches around the upper margin, which seems to have projected inwards to a slight extent. Across the rim are numerous transverse grooves of somewhat different width and depth, but none very deep. This edge is bevelled slightly inwards, and from corner to corner of the jar it is concave. On the outside of the jar beginning with the top we have first a band of short, upright grooves or long notches about 5^{mm} (·18 in.) long and 1^{mm} wide. Directly below these are three somewhat irregular bands, distinct, though not deep, made by a square end, as they are precisely such as might be made by drawing a small nail over moist clay. These grooves are 2^{mm} wide, the three with the intervening spaces forming a band $7{\cdot}5^{mm}$ (·3 in.) in width. Below this is a space 3^{cm} or more in width covered with oblique lines variously arranged. Above this, that part covered with the lines described is vertical, but this, which bears the oblique lines, is inclined outwards from above down and this is due chiefly to the increasing thickness of the material, it being thickest just above the contracted portion. Along each side of the space covered by oblique lines, that is along each edge of the quadrilateral upper part of the jar, is a vertical series of circular depressions, the margin of which is more deeply sunk than the rest forming an annular figure.

These figures are $6{\cdot}4^{mm}$ (·25) in diameter, and there are four in each series. The arrangement of the oblique lines which fill the space between two rows of the circular figures is as follows: beginning with the left side, the upper corner is filled with a series of twelve lines inclined about forty-five degrees to the horizontal, from left to right, and about $1{\cdot}5^{mm}$ apart. Crossing these and so bringing them to an end, are three lines about equally oblique but inclined from right to left, regarding the top as the starting point, then comes a band of short, broad parallel grooves, which are drawn in about the same direction as the first named series of lines, the whole series being inclined in the opposite direction. Then come two lines running in the same way, so that we have as the whole series, three lines, a band of parallel transverse lines about 4^{mm} wide, then two more lines, the whole crossing and interrupting a series of lines running in the opposite direction.

From the point where the last described series meets the horizontal lines around the top of the jar there extends a similar series of transverse, parallel bands to the lower edge, diverging from the other lines and forming an inverted V (viz. Λ) with it. The space between these two series is filled with a series of oblique lines running from left to right, each line of course from the nature of the case being shorter than the one on the right. Dividing these lines about equally into two groups is a band of the same sort of transverse grooves which in this case become little more than long notches. The remaining portion of the specimen, that to the right of the lines mentioned, is filled with similar lines inclined from right to left, divided into two groups by a series of transverse short grooves. Around the lower, bevelled edge of this part of the jar is a series of vertical notches of considerable size and below this is the unornamented neck.

In attempting to describe these specimens of pottery I have realized very fully the difficulty of so picturing in words the characteristics of each as to bring them clearly before the minds of those reading the descriptions, and it is with no little misgiving that they have been undertaken. It has seemed to me, however, that there was sufficient interest connected with such specimens as those here mentioned, to warrant some notice of them even though it should be imperfect. I have spoken only of earthen-ware; a word in regard to stone pots may not be out of place.

So far as I can ascertain, pots of steatite, or any other stone, are very rarely found in Vermont, still they are not wholly wanting. On only one of those of which I have any knowledge is there anything that appears like ornament. This is a fragment of a steatite jar which seems to have been of conical or ovoid form, the apex being the base of the jar. It was found in Milton, Vermont. Only one side and the base remain. Along this side, extending from near the base upwards is a series of long, narrow rectangular figures in pairs, like a series of broad dashes, ═ ═ ═. The groove between them is not so deep as the rest of the surface and suggests the idea that first a rectangle was cut and then divided longitudinally. The figures are quite regularly cut and are raised above the general surface about 2·5^{mm} (·08 in.) The sides are not straight but bevelled from the top to the base so that the latter is broadest. There are three pairs of them on the fragment with one end of a fourth pair. They are not of exactly the same size but do not greatly differ. An average measurement would be, length 10^{mm} (·4 in.) and width 3^{mm} (·12 in.) for each figure of the pair. The general surface of the stone is smooth and well finished. The specimen is quite thick varying from 16·5^{mm} below to 12·7^{mm} above. It is probable that there were several, perhaps four, rows of these figures coming from the base upward but there is nothing to show whether this was the case or not. Another steatite dish has been found in New York near the Lake Shore which is of a square-oval outline, very shallow, flat below and furnished with a sort of handle at each end, and this specimen is probably of the same age as the one just described.

The Mood of the Verb in Conditional Clauses. By I. B. Choate, of Akron, Ohio.

The principle which forms the basis of the distinction between the indicative mood and the subjunctive, as they are employed in all cultivated languages, is the least clearly apprehended, if indeed it be correctly apprehended, as yet, by any who have treated this perplexing subject. First, we have had the usage of the ancients given as a purely arbitrary matter; especially among the Greeks. Then we have had the most ingenious theories devised by those who have made the ancient classics subservient chiefly to the end of grammatical criticism rather than models of composition, not only among the Germans but among English and American scholars. No explanation yet offered of the conditional sentences in Greek and Latin is satisfactory to me, because it requires, on the part of those who used those languages for the every-day purposes of conversation, a quicker apprehension and stronger powers of metaphysical analysis than it seems possible that any people, as a whole, ever possessed, although I credit the Greeks in particular with great acuteness of intellect which they owed largely to the exactions of their strictly logical language. I cannot believe that the Greek writer or speaker of average ability, following the rules and explanations given in our modern grammars for the construction of conditional sentences in his own language, could form such sentences readily even if he could understand what the rules mean, which I greatly doubt. Recognizing as I do the ability with which this matter has been treated, I should hesitate still more to offer any new theory upon it, and that theory so widely at variance with any before proposed, were it not for the warrant given myself and given all students by the Journal of Philology. In that publication for 1874, page 186, Prof. Goodwin, in an article on "the Classification of Conditional Sentences in Greek Syntax," says: "Until the generation has passed away which can remember Porson's controversy with Hermann about the common rules of iambic verse, surely no one can be charged with impertinence for suggesting doubts as to the correctness of any generally received principle in Greek or Latin grammar."

Thus shielded from the charge of impertinence, my interest in the questions to be considered gives me boldness to expose myself to the charge of folly should the theory I offer prove utterly groundless, or having grounds should nevertheless prove worthless. After quoting many examples from English writers to show that in translations from the Greek, as well as in original composition, no distinction is observed between the indicative and the subjunctive so far as form is concerned, Prof. Goodwin closes his able article with this paragraph: "These instances are quoted here not by way of criticism, but partly to show the utter want of any principle in modern English on the subject, and partly to incite some one who can speak with authority on English syntax to investigate the question historically, and show us, if possible, what is the correct usage according to the traditions of the language. If it is true (or if it *be* true), as I fear it is, that no one can define the correct usage of the present day, even so far as to tell us what is the distinction recognized by our best writers between *if it be* and *if it is*, or if no two opinions on this question would agree, such uncertainty and such laxity of usage are surely no credit to our scholarship or to our language." In another place he has said: "The question which goes to the root of the whole discussion of conditional sentences is one which every schoolboy is taught to answer at a very early stage in his classical studies — what is the essential force of the Greek subjunctive in protasis as opposed to the simple indicative, e. g. of ἐὰν πράσσῃ τοῦτο as opposed to εἰ πράσσει τοῦτο?" Now this distinction is the same in Latin as in Greek — the conjunction *si* of the former being the same word as εἰ of the latter. I therefore propose to consider this question with reference to the usage of the Latin. This matter cannot be investigated historically in either the Greek or the Latin language, because in them the conjunctions which determine the mood of the verb are *prehistoric remains*. We cannot be positive whether εἰ of the Greek be from εἶναι to be, or from ἐάω to permit, or from some other source; and the same difficulty attends the derivation of the Latin *si*. We must, therefore, seek the clue to this distinction in some language like our own English in which the present is not so far removed from the original form that we cannot trace its history.

And, first, as our language is not an inflected one we show relations as far as possible by separate words and not by variation of form. It is, therefore, only by comparison with the Latin that we are able to distinguish between the indicative and subjunctive. When, however, we make the distinction, we find that it depends upon whether the condition present itself to the mind of the speaker objectively, or whether it rise in his own mind and so be viewed subjectively. In the former instance, the mood of the condition will be indicative, in the latter, subjunctive. Now to determine this point we must have reference to the conjunction, which in nine cases out of ten is the word *if*. Upon this word Sir John Stoddart remarks that "it is not only the imperative of the verb to *give*, which has been used with a conjunctional force, but also the past participle *given* of the same verb." If it be at any time the past participle *given* there is nevertheless an imperative implied; as, for instance, in the statement of the problem: Given two sides and the included angle, etc., it is intended, Let there be given, etc. We may, then, consider it as always the imperative of the verb *give*. As such it must have a subject. That subject may be of the first or of the second person. How rarely do we find it expressed with the imperative in any language! According as we understand for the subject the speaker or the persons addressed we put the condition in the subjunctive or in the indicative mood. When the speaker intends "Let *me* give, grant, suppose," or "Let *us* give, grant, suppose, that a thing *be* so and so," he employs the subjunctive; for the condition expresses an idea arising in his own mind, not treated by him as a matter of knowledge or belief, but contingent, and assumed that he may base some statement upon it. On the other hand, if he intends "Give, grant or suppose *you* or *ye* that a thing *is* so and so," then he employs the indicative mood, for to his mind the condition is, in the expression of it, presented objectively. This is the one principle to which, it seems to me, can be referred the mood of every conditional sentence.

That the conjunction *si* of the Latin is similar in its origin and nature to our *if* in the sense of *grant* or *granted* appears from the use of the same word *se* or *si* in Italian, Spanish and French as a conjunction and as an adverb of affirmation, as *if* and as *yes*. In Spanish, for instance, we say *Si V. gusta; If you please; si, señor; yes, sir*. The former expression can be rendered, *Granted, i. e., Let it be granted that you please;* the latter, *Granted, sir*.

To test the theory let us apply it to a few familiar examples in Latin. Horace addressing the Muse, L. IV, Carm. III, V, 24, says "*Quod spiro et placeo, si placeo, tuum est*," in which it will be noticed that the condition is expressed in the indicative, not that he assumes as a fact that he does please; but, disclaiming the assumption, he leaves it to the judgment of the Muse and says *Grant you that I please, it is thy gift.* Again, Sat. L. II, VI, 39, he uses the expression "*Si vis, potes*," where it appears that the condition means "Grant you that you are willing," and then, upon that condition, the speaker adds the assurance "you are able." And, also, L. II, Epistola II, 155–159,

> "At si divitiæ prudentem reddere possent,
> Si cupidum timidumque minus te; mempe ruberes,
> Viveret in terris te si quis avarior uno.
> Si proprium est, quod quis libra mercatur et aere,
> Quædam, si credis consultis, mancipat usus;"

where we have the subjunctive and indicative brought together in successive sentences. Were the condition in the last line taken in the sense, "Let *us* suppose that you put confidence in lawyers," it would have its verb in the subjunctive and be useless in the sentence; but it takes the indicative in the sense "Grant you, etc."

This view of the particle *si* will render more striking the figure employed by Tacitus, Agr: XLVI, "Si quis, etc.," where he employs the indicative mood in the condition. But I need not multiply examples. They will be found abundant on the pages of the ancient writers. I have tried clearly to present this theory, hoping that, somehow, my efforts may contribute to the advancement of science.

TITLES OF OTHER PAPERS READ IN THE SUBSECTION OF ANTHROPOLOGY.

THE IROQUOIS GENS. By L. H. Morgan, of Rochester, N. Y.*

THE IROQUOIS PHRATRY. By L. H. Morgan, of Rochester, N. Y.*

THE IROQUOIS TRIBE. By L. H. Morgan, of Rochester, N. Y.*

THE IROQUOIS CONFEDERACY. By L. H. Morgan, of Rochester, N. Y.*

ON THE MYTHOLOGY OF THE NORTH AMERICAN INDIANS. By J. W. Powell, of Washington, D. C.

THE SCOPE OF ANTHROPOLOGY, AND THE CLASSIFICATION OF ITS MATERIALS. By O. T. Mason, of Washington, D. C.

THE INTERNATIONAL SYMBOLS FOR CHARTS OF PREHISTORIC ARCHÆOLOGY. By O. T. Mason, of Washington, D. C.

BRAIN-WEIGHT AND SIZE IN RELATION TO THE RELATIVE CAPACITY OF RACES. By Daniel Wilson, of Toronto, Canada.†

HYBRIDITY AND ABSORPTION AMONG THE RACES OF THE NEW WORLD. By Daniel Wilson, of Toronto, Canada.†

ON THE ANCIENT AND MODERN PUEBLO TRIBES OF THE PACIFIC SLOPE OF THE UNITED STATES. By E. A. Barber, of Westchester, Pa.

THE ARCHÆOLOGY OF EUROPE AND AMERICA COMPARED. By S. D. Peet, of Ashtabula, Ohio.

ON THE STATE OF SOCIETY IN THE PRIMITIVE AGE. By S. D. Peet, of Ashtabula, Ohio.

THE MUSEUMS OF INDUSTRIAL ART IN AUSTRIA. By Heinrich Frauberger, of Brünn, Austria.

* These several papers by Mr. Morgan will be printed in his work entitled "Ancient Society."

† These two papers by Professor Wilson are published in the "Canadian Journal."

EXECUTIVE PROCEEDINGS

OF

THE BUFFALO MEETING.

HISTORY OF THE MEETING.

THE TWENTY-FIFTH MEETING of the Association was held in the City of Buffalo, N. Y., beginning on Wednesday, August 23, 1876, and closing on the afternoon of the following Wednesday.

Several peculiar circumstances united in making the meeting one of unusual interest, and among them may be mentioned the fact that President ROGERS, who presided at this twenty-fifth meeting, also presided at the organization of the Association in Philadelphia, on September 20, 1848.[1] The Centennial Exhibition at Philadelphia was also the means of securing the attendance of numerous foreign savants who were in America as commissioners from various countries, and were most cordially welcomed to the Association both by its officers and the citizens of Buffalo.

To the Local Committee much credit is due for the arrangements made for the meeting, and, from the cordial support which they received from the citizens, it was evident from the first that the second meeting in Buffalo would greatly augment the kind feeling that had existed from the former meeting, which was the first reunion after the war.

The attendance of members and citizens at the general and sectional sessions was larger than at several preceding meetings.

Two hundred and fifteen names of members and foreign guests were entered on the register, while three hundred tickets were given out for the meeting.

Of the thirty-four members of the Standing Committee, twenty-four were present, including eight past presidents of the Association.

One hundred and forty-six members were elected; of these one hundred and seventeen have accepted, four have declined, one was already a member, and twenty-four have not yet replied to their notifications.

Seventeen members were elected fellows, of whom nine have accepted.

Notices of the death of nine members and fellows were received since the meeting of 1875.

[1] The discrepancy between the number of meetings and the number of years of existence of the Association is due to the fact that in 1850 and 1851 four meetings were held, while from 1861 to 1865, during the civil war, the meetings were suspended.

Of the four hundred and sixty-one names given in the list published after the first meeting of the Association in 1848, only forty-three now remain on the roll of the Association as continued from that list. Of these, Prof. JAMES HALL, of Albany, is the only original member who has retained uninterrupted membership from the organization of the Association of American Geologists and Naturalists, which Society, in 1848, formed the nucleus of the present Association.

The arrangements made by the Local Committee for the accommodation of the Association were most acceptable, and the rooms for offices and for the meetings of the several sections in the City school building were all that could be desired; while the Common Council Chamber, in the new and beautiful City and County building, furnished a large hall for the general sessions; the only drawback being the time lost in going from the general session each morning to the sections, but it will seldom be the good fortune of the Association to have all the sessions of a meeting under one roof. Two general evening sessions were held in St. James' Hall for the delivery of the Addresses of the President and Vice Presidents. A parlor was also provided by the Local Committee at the Tifft House, where many of the officers and members were quartered, for a general reception room and headquarters of the Association when not in session. Several evening meetings of Committees were also held at the Tifft House. The rooms of the Buffalo Academy of Natural Sciences were kept open throughout the week for the benefit of the Association, while the Academy of Fine Arts and the Young Men's Association also invited the Association to make use of their rooms.

One hundred and forty-seven papers were entered by title. Of these, nine were not considered by the Standing Committee for the lack of abstracts or from the titles being withdrawn by their authors; fourteen were rejected by the Committee as being already published, and for other reasons. Of the remainder, thirty-one were referred to SECTION A, eighteen to the SUBSECTION OF CHEMISTRY, nine to the SUBSECTION OF MICROSCOPY, forty-six to SECTION B, and twenty to the SUBSECTION OF ANTHROPOLOGY. The pecuniary condition of the Association being such as to demand strict economy, it was determined by the Standing Committee to limit the size of the volume of Buffalo Proceedings, hence only those papers were considered, in relation to their publication, that were received by the Permanent Secretary within the month after adjournment, as specified by the rules, general notices requesting the papers of their authors not being sent out as in former years. For the same reason very slight appropriations were made for illustrations, and the Association is indebted to Messrs. Osborn and Riley for those accompanying their papers.

The organization of the Permanent Subsection of Anthropology was an interesting and popular feature of the meeting, being well attended and maintained. The separation of this Subsection from Section B was also shown to be desirable by the circumstance that Section B was obliged to send off a Subsection of Geology on the last day of the meeting in order to dispose of the papers still before the Section. It is believed by those who took part in the Anthropological meetings that the interest will rapidly increase in this Subsection, and that its future meetings will be well supported.

A Permanent Subsection of Microscopy was organized, and though the sessions which it held were in great part devoted to perfecting its organization for future meetings, yet several papers were read and a general interest in the objects of the Subsection awakened to such an extent as to argue favorably for the next meeting.

The Permanent Subsection of Chemistry continued its organization with increased attendance and interest, and the able opening address of its chairman called and retained many attendants from the other sections.

The Entomological Club of the Association held several meetings during the week, the first being on the afternoon before the Association commenced. This Club is annually increasing in favor with the entomologists and calls many persons specially interested in this department to the meetings of the Association.

The Cambridge Entomological Club and the Buffalo Microscopical Club each held meetings during the week, to which those members of the Association interested were invited.

The bringing together once a year of all persons interested in the many departments of science has always been one of the primary objects of the Association, and it is greatly to be hoped that the numerous associations and societies of kindred objects with the American Association for the Advancement of Science will realize the advantages to be gained by having one of their meetings held in connection with the Association, and thus allow those who would enjoy meeting their colaborers at least once a year the opportunity of doing so at the least expense of time, and in connection with the liberal arrangements that are always made in behalf of the Association by the citizens of the place of meeting.

The Report of the General Secretary will give the further necessary details of the meeting, and it only remains to mention the entertainments and excursions so generously extended to the Association.

Besides securing reduced rates at the hotels in the city, very many of the members, and all of the foreign guests who would accept, were cordially received at the homes of the citizens of Buffalo, and though very little was done in the way of reduced fare over the railroads, the Local Committee succeeded in various ways in making the attendance at the meeting as little as possible a pecuniary outlay to members of the Association.

After the address of President HILGARD, the Association was received by the BUFFALO CLUB at an elaborate entertainment at their Club-house. After the addresses of the Vice Presidents an informal reception was given by the Academy of Fine Arts at its rooms, and other informal receptions were given during the week to small parties of members at the hospitable homes of several of the citizens. The officers of the Union Iron Company kindly took a number of members to visit their extensive works, and several short excursions were arranged by the citizens for members specially interested in various departments.

On Saturday, by the kindness of W. H. VANDERBILT, Esq., Vice President of the Central R. R., and under the special charge of Capt. E. P. DORR, an excursion for the Association and its friends was given to Niagara Falls, where the various places of interest, as Goat Island, Prospect Park, the Tower, Bridge, etc., were free to the party, while on the Canadian side Mr. and Mrs. JOHN T. BUSH opened their beautiful grounds and hospitable mansion and most generously entertained the large assembly.

On the Tuesday following, the citizens of Jamestown received the Association and a large number of citizens of Buffalo. By the kindness of President JAMES W. SCATCHERD a special train was provided for the party and, again under the charge of Capt. DORR of the Local Committee, a successful and interesting excursion was the result. On the arrival of the train at Jamestown the party was transferred to the steamer of the same name, placed at the disposal of the excursionists by its proprietor, O. E. JAMES, Esq., and a beautiful trip on the famous Chautauqua Lake was enjoyed. At Lake View the party was hospitably entertained, and late in the evening all returned safe to Buffalo.

On Wednesday afternoon the closing general session was held in the Common Council Chamber, and was followed by a ride, in carriages furnished by the Local Committee, to the large City Park, where a collation was provided at the Parade House, and after informal speeches and a general social time, the members bade adieu to their kind entertainers, and thus closed the last official day of the meeting in Buffalo.

F. W. PUTNAM,
Permanent Secretary.

REPORT OF THE GENERAL SECRETARY.

THE ASSOCIATION met in the Common Council Chamber of the City Building, Buffalo, at 10 A. M., on Wednesday, August 23, 1876, and was called to order by the retiring President, Dr. J. E. HILGARD, who, in a few appropriate words, resigned the Chair to the President elect, Prof. WILLIAM B. ROGERS.

After the invocation by the Rev. L. VAN BOKKELEN, Hon. PHILIP BECKER, the Mayor of the City, welcomed the Association on behalf of the municipal authorities of the city of Buffalo in the following words:

MR. PRESIDENT AND MEMBERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE: On behalf of the municipality of the city of Buffalo, I extend to you a friendly welcome, not to you only, but to the many distinguished gentlemen who are here from foreign lands, to join with you in your important work. Although this may be the first visit you have made to our city, you are not strangers to us. The fame of many of your number in the important department which you represent here, has preceded you, and made your names as familiar to us as household words. I assure you, we feel highly honored that you selected our fair city for this annual meeting of your Association. I hope that you will find your stay with us agreeable, and that the hours of relief from your labors while here, may be pleasantly spent in visiting the institutions and places of interest in the city and its vicinity. Without delaying you longer, gentlemen, I again assure you of a hearty and cordial welcome.

Hon. SHERMAN S. ROGERS then welcomed the Association on behalf of the citizens of Buffalo, saying:

MR. CHAIRMAN:—Permit me on behalf of the good citizens of Buffalo to supplement very gladly and cordially the welcome which has been extended by the municipal authorities to your honored Association.

We remember well and with great pleasure your former meeting in this city, and we are glad to believe that its memory has still a sweet savor among you.

Sir: If I remember rightly we were indebted for that meeting to the enterprise and enthusiasm of our young Society of Natural Sciences—which was then barely organized. Your presence here again attests the assistance you then rendered the infant institution. In honoring it you rendered an important service to us all. And to-day we are proud of that young society. It has not prematurely fossilized and we believe that it will attest that it does not belong to the order of Ephemera. On the contrary it shows an encouraging vitality, and gives promise of a long life of usefulness. For this we are largely indebted to you.

Sir: We cannot but feel that the time in some sort adds distinction to the honor you confer by again becoming our guests, and we remark with pride and pleasure the presence of many distinguished gentlemen from foreign countries who sustain official relations to the great World's Exposition by which sister nations are with us doing honor to the nation's Centennial.

And now, Mr. President, if the weather as well as the time, be propitious, we shall hope that this meeting will be remembered by all with even more pleasure than its predecessor. For it is an open secret that this precious week is not to be wholly devoted to Science, and we shall need the benediction of friendly skies to complete our enjoyment. Again, sir, the citizens of Buffalo bid you each and all a very hearty and hospitable welcome.

President Rogers, for the Association, responded as follows:

On behalf of the Association I thank the honorable gentlemen most heartily for their greeting. The American Association being locomotive in its character has occasion to test the hospitality and sympathy of the communities through which it moves; and these have everywhere been largely extended to it.

In return we may claim that its meetings stimulate inquiry and to a certain extent reward research, and that it may justly be recognized as an important means of distributing broadly the blessings of progressive knowledge.

Men of science are not only proverbially but practically poor. They work not for themselves but for the world, and they rightfully expect the helpful hand of the community.

When I think of Buffalo ninety years ago, with but a single trading house on Buffalo creek, and now look upon the magnificent city which has been built up, and to the culture of the inhabitants and their manifest interest in scientific as well as artistic pursuits, I feel that there is something in the spirit that has been here displayed which is magnificently characteristic of American thought and enterprise.

I can, therefore, appeal with confidence to the people of this city to give a hearty support to their scientific and kindred institutions; and among these I would especially commend the "Buffalo Society of Natural Sciences," which, although a comparatively young society, has by its contributions to science made itself favorably known on both sides of the Atlantic, and reflected honor on its members and the community in which it is sustained.

The work of the Association is unostentatious, and in a large measure withdrawn from merely popular themes, but there is always so much of generalized thought, apart from scientific details, in our discussions, that we may hope that the gentlemen and ladies attending them will find their interest unabated as the meetings progress; and we now most gladly welcome to our sessions all who may be disposed to attend.

THE PERMANENT SECRETARY read notices of nine members and fellows deceased since the last annual meeting.

Dr. LEVERETT BRADLEY died suddenly in Jersey City, N. J., on Sept. 6th, 1875, in the 77th year of his age. Joined the Association at the 15th meeting.

Prof. SAMUEL D. TILLMAN died in Jersey City, N. J., Sept. 4th, 1875, in his 63d year. Joined at the 15th meeting.

WILLIAM E. DOGGETT, of Chicago, Ill., died in Palatka, Fla., April 3d, 1876. Joined at the 17th meeting.

EBENEZER HANCE, of Fallsington, Penn., died April 7th, 1876, in his 81st year. Joined at the 7th meeting.

INCREASE A. LAPHAM was born in Palmyra, N. Y., March 7th, 1811, and died Sept. 14th, 1875. Joined at the 3d meeting.

HARVEY S. SENTER, of Aledo, Ill., died March, 1875. Joined at the 20th meeting.

Dr. LEWIS FEUCHTWANGER, of New York, died June 25th, 1876. Joined at the 11th meeting.

JACOB PAINTER, of Lima, Penn., died 1876. Joined at the 23d meeting.

Col. SAMUEL STONE, of Chicago, Ill. Died May 4, 1876. Joined at the 17th meeting.

The GENERAL SECRETARY announced the number of papers to be presented to the various sections received up to that hour; also a list of sixty-one candidates for membership, recommended by the Standing Committee, who were then duly elected.

The Association proceeded to the election of six Fellows as members-at-large of the Standing Committee. The election resulted in the selection of Professors Geo. F. BARKER, A. R. GROTE, SIMON NEWCOMB, ALEXANDER WINCHELL, F. W. CLARKE, and Hon. L. H. MORGAN.

The following invitations to hold the next annual meeting at Nashville, Tenn., at Atlanta, Ga., and at St. Louis, Mo., were read by the PERMANENT SECRETARY and referred to the NOMINATING COMMITTEE.

EXECUTIVE OFFICE,
NASHVILLE, TENN., AUGUST 7th, 1876.

WHEREAS, At a meeting of the American Association for the Advancement of Science held at Newport, August, 1860, an invitation was accepted to hold its next annual meeting at Nashville, Tenn.; and, whereas said meeting was prevented from being held by hindrances of Civil War, now then, we the undersigned, citizens of Nashville and of Tennessee, do respectfully and cordially invite the said great National Association to hold its next meeting in the Capital of Tennessee. We pledge our earnest endeavors to make the meeting here in every way a success.

Signed by
JAMES D. PORTER, *Governor of Tennessee.*
CHARLES N. GIBBS, *Secretary of State.*
JAS. L. GAINES, *Comptroller.*
W. MORROW, *Treasurer.*
LEON TROUSDALE, *State Supt. Public Instruction.*
W. R. HAMBY, *Adjutant General.*
J. B. KILLEBREW, *Com'r Agricultural Statistics and Mines.*
J. M. SAFFORD, *State Geologist.*
EDWIN H. EWING, *Pres. Board of Trus. Univer. of Nashville.*
EBEN S. STEARNS, *Chancellor University of Nashville.*
H. N. MCTYEIRE, *Pres. Board Trust. Vanderbilt University.*
L. C. GARLAND, *Chancellor Vanderbilt University.*
THOS. A. KERCHEVAL, *Mayor of Nashville.*
E. W. COLE, *Pres. N. C. & St. Louis Railway Co.*
C. M. MCGHEE, *Vice President E. T. V. & Ga. R. R. Co.*
E. D. STANDIFORD, *President of L. & N. & G't S. R. R. Co.*
J. G. M. RAMSEY, M. D., *Pres. Tenn. Historical Society.*
N. T. LUPTON, *Fellow Am. Assoc. Adv. of Science.*
J. BERRIEN LINDSLEY, *Fellow A. A. A. S.*

MAYOR'S OFFICE,
CITY HALL, ATLANTA, GA., AUGUST 6TH, 1876.

PROF. F. W. PUTNAM,

Permanent Secretary of the A. A. A. S.

SIR: At a meeting of the Mayor and General Council of this city, held this day, the following resolution was unanimously adopted.

WHEREAS, the American Association for the Advancement of Science, by its annual meetings for twenty-five years in the various cities of our country has done much to disseminate useful information, and has increased the interest in scientific pursuits and the practical applications of the teachings of science to the affairs of daily life; and, whereas, our sister city of Charleston, S. C., has thus far been alone favored in all the South by the presence of this large body of able men;

Resolved, that we do hereby tender to the Association a cordial invitation to hold its next annual session in Atlanta, Georgia.

C. C. HAMMOCK,
Mayor.

ATLANTA, GA., AUGUST 11, 1876.

PROF. F. W. PUTNAM,

Permanent Secretary A. A. A. S.

SIR:—The undersigned, citizens of the state of Georgia, representing the various interests of her people, having learned that there is a possibility of securing the next annual meeting of your Association in this city, and believing that such meeting would result in great benefit to the people of our whole state, and that it will afford them pleasure to entertain so learned and distinguished guests, do earnestly request your body to accept the invitation of our City Council.

Signed by
JAMES M. SMITH, *Governor.*
N. C. BARNETT, *Secretary of State.*
N. J. HAMMOND, *Attorney General.*
W. L. GOLDSMITH, *Compt. General.*
GUSTAVUS J. ORR, *State School Commissioner.*
THOMAS P. JANES, *Commissioner of Agriculture.*
J. W. RENFROE, *State Treasurer.*
JOSEPH E. BROWN.
JOHN B. PECK.
F. M. COKER, *Pres. Bank of State of Georgia.*
V. R. TOMMEY, *Pres. Georgia Banking and Trust Co.*
WM. PHILLIPS, *Pres. M. & N. G. R. R.*
GEORGE LITTLE, *State Geologist.*
E. L. JONES, *Pres. Georgia National Bank.*
CAMPBELL WALLACE, *President State National Bank.*
PERINO BROWN, *Cashier Citizen's Bank.*

CHAMBER OF COMMERCE,
ATLANTA, GA., AUG. 10, 1876.

PROF. F. W. PUTNAM,

Permanent Secretary A. A. A. S.

SIR:—We, the Board of Trade of Atlanta, Ga., do most heartily unite in the invitation of the Mayor and Council of this city, to your Association to hold your next annual meeting in their midst, assuring you of a warm reception by our people and of our own efforts to make your visit comfortable and agreeable.

BENJ. E CRANE,
Pres. Atlanta Chamber Commerce.

MAYOR'S OFFICE, ST. LOUIS, AUGUST 8, 1876.

PROF. F. W. PUTNAM,

Permanent Secretary of the A. A. A. S.

DEAR SIR:—Referring to the approaching session of your Association, I beg respectfully to suggest the city of St. Louis as a fitting place for the next annual meeting. Our citizens generally fully recognize the national importance of the Association, and will appreciate the honor of having its next session held within our limits.

I make this suggestion because several of our sister cities have been honored in this way, and it seems appropriate that the leading metropolis of the West should participate in a similar distinction. Under the restrictions of our charter I am unable to tender any specific hospitality on behalf of the city, but I can assure you of the deep interest taken by our people in the elevated objects of the Association, and of the hearty welcome they will extend to its members.

Respectfully,
H. OVERSTOLZ,
Mayor.

ST. LOUIS, MO., AUGUST 3, 1876.

PROF. F. W. PUTNAM,
Permanent Secretary A. A. A. S.

MY DEAR SIR:—With a full appreciation of the importance of the American Association for the Advancement of Science, and with an earnest desire to have its influence felt in this great metropolis of the Mississippi Valley, I herewith, on behalf of the Academy of Science of St. Louis; cordially invite the Association to hold its next annual meeting in the city of St. Louis. We have not, thus far, as have several of our sister cities of this great valley, been honored by having the Association meet with us, and I am confident that our citizens will welcome you with the pleasure and heartiness that attend all rare and long looked for visits. From expressions of prominent citizens, and of members of the Academy, I can promise you that all will be done within our power, to make the session of the Association, if held here, in 1877, both pleasant and profitable.

Yours, truly,
C. V. RILEY,
President Ac. Sc. St. Louis.

ST. LOUIS, AUGUST 11, 1876.

PROF. F. W. PUTNAM,
Secretary of the A. A. A. S.

SIR:—The proceedings of your Association have justly awakened a national interest in its objects. Its meeting in this city is greatly desired. As the President of the Missouri Historical Society of St. Louis, I respectfully present the cordial request of this Society, that the next meeting of your Association be held in this city. Its members will receive a most hospitable and congenial welcome, and the Association will secure to it hosts of worthy, useful, and permanent friends.

Very respectfully, your obedient servant,
JAMES G. BARDY.

STATE OF MISSOURI, EXECUTIVE DEPARTMENT.
CITY OF JEFFERSON, AUGUST 14, 1876.

F. W. PUTNAM, ESQ.,

Secretary A. A. A. S.

SIR:—In view of the high estimation in which we hold the American Association for the Advancement of Science, and of the great good which doubtless is disseminated by it in communities in which its annual meetings are held, we have to request that its next annual meeting be held at the city of St. Louis, where I am sure every hospitality and kindness will be extended to the individual members of the Association, and where the ability, talents and learning of the body will be as greatly enjoyed and appreciated as anywhere.

Trusting that your Association will grant this request, and that your approaching session will be one of especial benefit to the members and the country,

I have the honor to be, very respectfully, your ob't servant,

C. H. HARDIN.

The GENERAL SECRETARY presented the recommendation of the Standing Committee, that the morning sessions should close at 12.30 P. M., and the afternoon sessions begin at 2.30 P. M., which was adopted.

The STANDING COMMITTEE also recommended that the addresses of Vice President YOUNG and Vice President MORSE be delivered at St. James' Hall, this evening, beginning at 7.30 o'clock.

An invitation was received and accepted, to visit the rooms of the BUFFALO ACADEMY OF FINE ARTS at 9 o'clock this evening.

Announcements as to the places provided for the meeting of the several Sections of the Association were made, and the Association adjourned for the organization of Sections.

At 2.30 P. M., the Association met in Sections. *Section* A met in room No. 16 Central School. It was called to order by Vice President C. A. YOUNG, Chairman. Secretary, Prof. ARTHUR W. WRIGHT.

The organization was completed by the selection for Sectional Committee, Prof. H. T. EDDY, Prof. SIMON NEWCOMB, Dr. R. H. WARD; and for members of the Nominating Committee, Profs. E. N. HORSFORD, S. A. LATTIMORE, H. B. NASON, N. T. LUPTON.

SECTION B met in Room No. 17 Central School. Vice President EDWARD S. MORSE in the Chair. Secretary, Prof. ALBERT H. TUTTLE.

The organization was completed by the selection of a Sectional Committee consisting of Mr. W. H. DALL, Dr. BURT G. WILDER and Rev. J. G. MORRIS, and for members of the Nominating Committee, Dr. H. WHEATLAND, Prof. W. C. KERR, Prof. GEORGE LITTLE, Dr. J. A. WALKER.

PERMANENT SUBSECTION C met in Room No. 15 Central School, with Prof. GEO. F. BARKER, Chairman, and Dr. H. C. BOLTON as Secretary.

PERMANENT SUBSECTION D met in room No. 11 Central School, with Hon. L H. MORGAN, Chairman, and Prof. OTIS T. MASON as Secretary.

The address of Professor BARKER, Chairman of Subsection C, was delivered in the room occupied by Section A, immediately following the organization and adjournment of that Section.

In the evening the Association listened to the addresses of Vice President YOUNG of Section A, and Vice President MORSE of Section B, at St. James' Hall.

The Second Day's Session opened in the Council Chamber, at 10.30 A. M.

PRESIDENT ROGERS read the following names of savants from foreign countries in attendance at the meetings of the Association: Dr. OTTO M. TORELL, Stockholm; Dr. E. H. VON BAUMHAUER, Haarlem; Dr. E. M. VON BAUMHAUER, Haarlem; DR. JOSUA LINDAHL, Lund; Dr. THEODOR NORDSTROM, Stockholm; EMIL BRUGSCH, Cairo, A. DANINOS, Cairo, Prof. BEHMER, Cairo, WILLIAM SAGEL, Vienna; Dr. H. FRAUBERGER, Brünn; Dr. ERNST FLEISCHL, Vienna; ALESSANDRO CASTELLANI, Rome; DR. HUGO HAMBERG, Upsala; Dr. RUDOLPH KOENIG, Paris.

These gentlemen were invited to enroll themselves as members of the Association, and the President extended to them a hearty welcome to its sessions.

Attention was called to a Memorial Case deposited in the Office of the Superintendent of Education by MR. JULIUS E. FRANCIS.

The PRESIDENT presented to the Association a large volume containing a valuable part of the work of the distinguished Paleontologist Prof. JAMES HALL of Albany, and expressed the indebtedness of the Association to Hon. NELSON K. HOPKINS of Buffalo, for his influence, while State Comptroller, in procuring its publication.

The GENERAL SECRETARY read a list of twenty-five candidates for membership recommended by the STANDING COMMITTEE, and they were elected by the Association.

The matter of place of meeting for general sessions during the remainder of the meeting was referred to the STANDING COMMITTEE with power to act.

Invitations were received to visit the works of the UNION IRON COMPANY, and also the Library of the YOUNG MEN'S ASSOCIATION.

The Association then adjourned to meet in Sections.

The Third Day's Session opened in the Council Chamber, at 10.30 A. M. Upon recommendation of the Standing Committee twenty-eight persons were elected members.

Dr. F. A. P. BARNARD from the COMMITTEE ON WEIGHTS, MEASURES AND COINAGE, submitted a partial report with the request that the remainder of the report might be postponed until Monday.

PRESIDENT ROGERS introduced Professor HUXLEY to the Association.

In response MR. HUXLEY said: *Mr. President, and Ladies and Gentlemen of the American Association for the Advancement of Science:*—Permit me in the first place, to offer you my most hearty thanks for your exceedingly cordial, I will not say unexpected, welcome, because everything I have experienced in America since my landing has been something of this kind. But I thank you, for this very cordial and hearty welcome; words are inadequate to express how much I feel on this occasion. I am not by nature a man of many words, and have thought the highest eloquence was in condensing what one has to say, but I beg you to believe that my few and simple words on this occasion represent an emotion difficult to express. I have no scientific matter to speak of, and am quite unprepared to occupy your attention on such an occasion as this. Since my arrival in America, I have discovered that the great instinct of curiosity is not altogether undeveloped among you. I experienced something of this soon after landing by being interviewed by two active and intelligent members of your press. They were good enough to put before me a series of questions on matters of professional interest, either of which would have required a treatise to answer, so I was obliged to dismiss them with very slight satisfaction. [Laughter.]

I will state some few of the most salient impressions which have been made on me since my arrival. I have learned many things, and various things. In England we hear and read of America, and have our occasional controversies with her, but no Englishman who has not had the good fortune to visit this country, can form a real idea of what that word America means. We have no adequate idea of the extent of your country, its enormous resources, the distances from centre to centre of population, and we least of all understand the great basis of character which sprung from the other side of the Atlantic. A friend of mine in England went abroad for the purpose of seeing foreign countries, and came to America, but while here he said he could not find that he was abroad. Everything here was so English. The scenes which I have witnessed during my travels through your country are like those which we are familiar with in parts of England, and parts of Scotland. Your beautiful river Hudson reminds me more of a Scotch lake than anything else.

I have heard a great deal from your own writers about the degeneracy of the present American stock from the primitive English type. The late Nathaniel Hawthorne used an expression which rather rubbed us; he spoke of the distinction between English and American women, and told us English women were rather too beefy. Now that was his expression, not mine. [Laughter.] I confess the signs of the modifications of type spoken of do not strike me. The main difference between you and us seems to be one of shading only. As to stature, your men have rather the best of ours. In regard to the stature of ladies I cannot find much difference, and for fine, matronly forms, the average is as great here as on the other side. There has been some talk of the influence of your climate carrying you back to the North American type. I cannot say that

I can see any signs of that, unless it be in the development of that virtue of hospitality which prevails among all savages. I have visited your wigwams—and they are pretty good wigwams too. You entertain us with your best, and not only give us a good dinner, but are not quite happy unless we take the spoons and plates away with us! [Laughter.]

Another feature I have observed which fills me with a certain amount of shame, when I think of what is going on in our country. I have visited your great universities of Yale and Harvard, and have seen how your wealthy men contribute to scientific institutions in a way to which we are totally unaccustomed in England. The general notion of an Englishman who becomes rich is to buy an estate and found a family. The general notion of an American who becomes rich is to do something for the benefit of the people and to found an institution whose benefit shall flow to all. I need hardly say which I regard the noblest of these two.

It is commonly said there are no antiquities in America, and you have to come to the old world to see the past. That may be, so far as regards the trumpery of three or four thousand years of human history. But, in the larger sense, America is the country to study antiquity. I confess that the reality somewhat exceeded my expectations. It was my great good fortune to study in New Haven, the excellent collection made by my good friend, Professor Marsh. There does not exist in Europe anything approaching it, as regards extent, and the geological time it covers, and the light it throws on the wonderful problem of evolution, which has been so ably discussed before you by Prof. Morse, and which has occupied so much attention since Darwin's great work on species. Before the gathering of such materials as those to which I refer, evolution was more a matter of speculation and argument, though we who adhered to the doctrine had good grounds for our belief. Now things are changed, and it has become a matter of fact and history. The history of evolution as a matter of fact, is now distinctly traceable. We know it has happened, and what remains for discussion is the subordinate question of how it happened.

I presume you will excuse me if I find nothing more to say except to give you these rough ideas of my impressions about your country. I would have willingly offered you something more suitable for your attention. I wish you all good speed, and that this Association like its sister in Great Britain will sow the seeds of scientific inquiry in all the towns it visits, and thus help on the great work. I now again thank you for your excessive courtesy, for your affectionate reception of me, and now wish you good-bye.

DR. F. A. P. BARNARD made a verbal report from the COMMITTEE TO MEMORIALIZE THE LEGISLATURE OF NEW YORK FOR A NEW SURVEY OF NIAGARA FALLS, and it was voted that the Committee be continued.

DR. J. L. LECONTE made a partial report from the COMMITTEE ON PRINCIPLES OF NOMENCLATURE. The report was referred to the STANDING COMMITTEE for consideration.

THE COMMITTEE ON PRESERVATION OF FORESTS was continued.

The following resolution from the STANDING COMMITTEE was adopted.

Resolved, That a Committee of the Association be appointed to consider the propriety of holding an International Congress of Geologists at Paris during the International Exhibition of 1878, for the purpose of getting together comparative collections, maps and sections, and for the settling of many obscure points relating to geological classification and nomenclature, and that to this committee be added our guests Dr. OTTO TORELL of Sweden and Dr. E. H. VON BAUMHAUER of the Netherlands, who shall be requested to open negotiations in Europe looking to a full representation of European geologists at the proposed congress; the said Committee to consist of Prof. WM. B. ROGERS, JAMES HALL, J. W. DAWSON, J. S. NEWBERRY, T. STERRY HUNT, C. H. HITCHCOCK, and R. PUMPELLY, in behalf of the Association, with the addition of DR. OTTO TORELL and DR. E. H. VON BAUMHAUER.

After some announcements by the LOCAL COMMITTEE, the Association adjourned to meet in sections.

No meeting of the Association was held on Saturday, the day being occupied by an excursion to Niagara Falls.

The Fourth Day's session was held in the room occupied at previous meetings, being called to order at 10.30 A. M.

Fourteen persons were, on recommendation of the STANDING COMMITTEE, elected to membership.

Seventeen members were recommended by the STANDING COMMITTEE for election as Fellows of the Association, and upon a ballot being taken they were all duly elected.

Prof. F. A. P. BARNARD from the COMMITTEE ON WEIGHTS AND MEASURES, presented the remainder of the report of that Committee, the first part of which had been given in general session on Friday.

The report was referred to the STANDING COMMITTEE with instructions to make such modifications and disposition of it as the Committee saw fit. (The full report is printed in another part of this volume.)

On motion of DR. BARNARD, the title of the Committee was changed to read "PERMANENT COMMITTEE ON WEIGHTS, MEASURES AND COINAGE;" and it was also voted that the PRESIDENT be authorized to fill any vacancy which has occurred in said Committee.

Prof. JOSEPH HENRY presented to the Association the last publication of the Smithsonian Institution, pertaining to the Meteorology of the Continent. He accompanied the presentation with some remarks upon the career and condition of the Institution and the service it had been to science and men of science in this country. He also referred to the pleasure and gratification he experienced in being able to meet with the Association, and of the opportunity he had enjoyed of mingling once more with respected and cherished friends.

On motion of Prof. HINRICHS a vote of thanks was tendered to Pro

HENRY as secretary of the Smithsonian Institution for the courtesy always extended by it to the students of science.

On recommendation of the STANDING COMMITTEE, Prof. HUXLEY was added to the International Committee on the International Geological Congress.

The PERMANENT SECRETARY announced arrangements made by the UNION IRON WORKS to convey any members who might wish to visit their establishment to the works, starting at 1.30 P. M. Also an invitation to visit the rooms of the HISTORICAL SOCIETY.

The GENERAL SECRETARY announced that the STANDING COMMITTEE recommended that the invitation to hold the next meeting of the Association in Nashville, Tenn., be accepted; also, that the Committee recommended the selection of the following named gentlemen as officers of the Association for the ensuing year.

President, SIMON NEWCOMB, of Washington, D. C.
Vice President, Section A, E. C. PICKERING, of Boston.
Vice President, Section B, O. C. MARSH, of New Haven.
General Secretary, A. R. GROTE, of Buffalo.
Secretary of Section A, H. C. BOLTON, of New York.
Secretary of Section B, W. H. DALL, of Washington, D. C.
Treasurer, W. S. VAUX, of Philadelphia.

The Association adjourned to meet in Sections, and again in General Session on Wednesday at 2.30 P. M.

There was no meeting of the Association on Tuesday, the day being devoted to an excursion to Chautauqua Lake.

The Fifth Day's Session convened in the Council Chamber of the City Hall at 2.30 P. M.

Eighteen persons, on recommendation of the STANDING COMMITTEE, were elected members of the Association.

PRESIDENT ROGERS introduced to the Association Sir REDMOND BARRY of Melbourne, Australia, who in response expressed regret at not being able to attend the convention at an earlier period in its session, and the gratification he had experienced during his stay in this country in witnessing the great scientific and intellectual activity which prevails everywhere.

MR. WILLIAM SAGEL of the Austrian Commission to the International Exhibition was also introduced and responded briefly.

On the recommendation of the STANDING COMMITTEE the Report of the Committee on Weights, Measures and Coinage, as read at a previous session, with its accompanying resolutions, was adopted as the sense of the Association.

The PERMANENT SECRETARY was authorized to cast the ballot of the Association for the election of officers for the ensuing year, which resulted in the election of the following officers previously recommended by the Standing Committee.

President, SIMON NEWCOMB, of Washington, D. C.
Vice President, Sec. A, E. C. PICKERING, of Boston, Mass.
Vice President Sec. B, O. C. MARSH, of New Haven, Conn.
General Secretary, A. R. GROTE, of Buffalo, N. Y.
Secretary Sec. A, H. C. BOLTON, of New York.
Secretary Sec. B, W H. DALL, of Washington, D. C.
Treasurer, W. S. VAUX, of Philadelphia, Pa.

The recommendation of the STANDING COMMITTEE that the next meeting of the Association should be held at Nashville, Tennessee, beginning on WEDNESDAY, AUGUST TWENTY-NINTH, 1877, was adopted.

The PERMANENT SECRETARY offered the following resolution, which was adopted:

Resolved, That the thanks of the Association be tendered to the distinguished citizens of Atlanta, Ga., and of St. Louis, Mo., for their cordial invitations to hold the next meeting of the Association in those cities, and although unable to accept either of these invitations for 1877, the Association hopes at some future time to be able to hold meetings in the places named.

PRESIDENT ROGERS communicated to the Association the fact that Gen. COMSTOCK, U. S. A., commanding the Survey on the Western Lakes, had presented to the Association, through Prof. F. A. P. BARNARD, a copy of the last Government map issued by said Survey.

The following resolutions were read by the GENERAL SECRETARY and adopted by the Association:

Resolved, That the thanks of the Association be tendered to the citizens of Buffalo, as represented by their City Government, by the Buffalo Society of Natural Sciences, the Board of Trade, the Young Men's Association and the Buffalo Historical Society, for having invited the Association to visit Buffalo, and for the numerous courtesies extended to it during its present meeting.

Seconded by Major J. W. POWELL.

Resolved, That this Association offer to the citizens of Buffalo their congratulations upon the possession in their midst of so efficient, so courteous and so agreeable gentlemen as those comprising the Local Committee which has had charge of this most successful meeting; and that we hereby tender to this Committee our sincere thanks for their kindness and attention throughout our meeting.

Seconded by Dr. LE CONTE.

After the adoption of this resolution Hon. GEO. W. CLINTON, LL. D., Chairman of the Local Committee, addressed the Association as follows:

Mr. President, and Ladies and Gentlemen of the Association: Fortunately for you I come utterly unprepared to try to express at any length the pleasure, the pride, with which I have listened to the resolutions unanimously adopted. But something will bubble up to show how grateful I am. Were I to express the fullness of my gratitude, you would not adjourn until the sun had gone down into the west. Buffalo is proud that you have been her guests. Of all the glories of the centennial year, she will count as first the honor you have done her in assembling here. Our citizens will through their lives turn back to this meeting as full of pleasure and profit. I do not represent the city of Buffalo, but if the people of Buffalo were to instruct me to speak for them, they would instruct me to say something like this: We thank the Association, its members, each and all, for giving us the great pleasure of their social converse, and the pleasure of having seen so many men and ladies distinguished for their learning and goodness. They would say thank you that some of your scientific men have brought with you your wives, daughters, sisters—I might say to some of the younger ones, your sweethearts. If there is anything that is glorious and blest, it is a pure and charming woman. We have heard of a war between science and religion. You in the mass of knowledge you have spread before us, by all your conduct and teaching, have confirmed in us the firm belief that there is not, that there never has been, and never can be war between these two. There has been strife between science and bigots, there never can be between Christianity and science. They are parallel lines and cannot intersect each other. Science is glorious, but it is cold, icy in itself because it is purely reason. Have we not had sad experience that while the intellect may be exalted to the stars, the soul may grovel in the mire. Religion rests upon faith. It elevates us into adoration of the ineffable God our creator, our benefactor. If we were to make the choice there is I believe not a man here, surely not a woman, but would deem it better to have the ineffable charity which induced Philip Sidney instead of quenching his dying thirst to pass the cup of water to the wounded soldier,—better that charity than all the laurels science can offer.

Let me thank you for this: By putting the seal of your approbation upon our humble society you have inexpressibly strengthened it, and I now feel more sure than before that it will live, that Buffalo will continue to count it among her honors and will never let it die. We cannot monopolize you, but we do implore you that at the earliest opportunity you will renew this honor and pleasure so beautifully conferred by you upon our city.

Resolved, That this Association hereby expresses its sincere thanks to the Mayor and Common Council of the City of Buffalo, for the varied courtesies extended to us during the present meeting, especially for the use of their new and elegant Common Council Chamber, in which the General Sessions have been held, of the Central School in which the sections have met, and of the Mayor's Office.

Seconded by Prof. E. T. Cox.

Resolved, That the admirable success of the excursions made by this Association to Niagara Falls and to Chautauqua Lake, is a most direct evidence of the present existence of man upon the earth. That the evidence before us proves this man to be possessed of energy, tact, and address of a superior order, joined to high moral qualities. That the discovery of this man at Buffalo this Association regards as one of its highest scientific achievements, and that consequently to Capt. E. P. DORR, the Association desires to present its heartiest thanks, for the excursions above mentioned.

Seconded by Prof. GEO. F. BARKER.

Resolved, That the thanks of the Association be tendered to Mr. W. H. VANDERBILT; Mr. JOHN T. BUSH; the proprietors of Goat Island and Prospect Park; and to the Niagara Falls Suspension Bridge Company, for the excursion to the Falls, and the generous entertainment there.

Seconded by Prof. E. S. MORSE.

Resolved, That the thanks of the Association be tendered to the Directors, with the President, I. N. SCATCHART, and the Superintendent, P. C. DOYLE, of the Buffalo and Jamestown R. R.; to O. E. JONES, Proprietor; and T. E. GRANDIN, Captain of the steamer Jamestown; to Gov. FENTON, Judge MARVIN, Dr. J. HENRY RATHBONE (for the citizens of Jamestown) and the proprietors of the Kent House, for the very agreeable excursion on Chautauqua Lake with which they entertained the Association.

Seconded by Prof. SIMON NEWCOMB.

Resolved, That our thanks be presented to the "Buffalo Club" for the generous and elegant reception given by its members to the members of the Association, enlivened as it was by music and dancing, and made resplendent with the beauty and intellect of Buffalo.

Seconded by Prof. C. V. RILEY.

Resolved, That the thanks of the Association be tendered to the Buffalo Fine Arts Academy for the agreeable reception given it during the present meeting, and for the invitation extended it to visit their gallery.

Seconded by Prof. H. B. NASON.

Resolved, That the thanks of the Association be given to those citizens of Buffalo who have opened their houses with unbounded hospitality for the private entertainment of the members, allowing them for a season to share in the pleasure of their refined and beautiful homes.

Seconded by Prof. JOSEPH LOVERING.

Resolved, That the thanks of the Association be tendered to the President and officers of the Union Iron Company for the invitation to visit their works and for the courtesies extended to such members of the Association as were able to accept the invitation.

Seconded by the Rev. Dr. DALRYMPLE.

Resolved, That the thanks of the Association be given to the Officers of the Lehigh Valley and North Pa. R. R., the Erie Railway, the Lake Shore Railroad, and the New York Central Railroad, for the favors extended to the members, in attending the meeting and visiting Philadelphia.

Seconded by Prof. W. H. CHANDLER.

Resolved, That the thanks of the Association be given to the representatives of the press, and especially to the proprietors of the Buffalo Daily Courier, for the energy, promptness and fidelity which they have exhibited in bringing the doings of the Association to the notice of the public.

Seconded by Prof. H. C. BOLTON.

Resolved, That the thanks of the Association be presented to the officers who have conducted the General and Sectional Sessions of the present meeting to so successful an issue, especially to our President, Prof. WILLIAM B. ROGERS, of Boston.

Seconded by Prof. F. W. CLARKE.

After the passage of these resolutions Capt. DORR, from the Local Committee, announced that arrangements had been made for the members of the Association to visit the various City Parks, immediately after adjournment, upon which Dr. LE CONTE offered the following resolution which was adopted.

Resolved, That in accepting the invitation to visit the City Parks, the Association renews its thanks for the continued evidences of the boundless hospitality of the people of Buffalo.

The business of the Twenty-fifth meeting of the Association having been concluded PRESIDENT ROGERS, after some valedictory remarks, declared the Association adjourned to meet in Nashville, Tenn., on Wednesday, August 29, 1877.

T. C. MENDENHALL,

General Secretary.

REPORT OF THE PERMANENT SECRETARY.

THE following cash account embraces the period from August 4, 1875, to August 18, 1876, including the Detroit Meeting, and closing just prior to that of Buffalo. It will be noticed that though there were no extraordinary expenses during the year, the receipts were not sufficient to meet the cost of printing the volume of Proceedings and the other expenses of the Association within $823.44, which, added to the outstanding debt, of $137.25, leaves the Association in debt to the amount of $960.69, at this date. It will, therefore, be necessary to practice economy at the coming meeting, especially in regard to printing. The Permanent Secretary has continued to employ, at his expense, an assistant at the office of the Association at Salem, and, as stated in the last Report, the numerous details to be attended to renders it necessary that the time of one person should be exclusively devoted to the work.

Fifty dollars have been added to the Life Membership Fund during the year, and this fund now amounts, with accrued interest at date, to $242.92. The Treasurer reports that the Permanent Fund in his possession, with accrued interest, amounts, at date, to $1,718.93.

The Property of the Association now consists of the following:

Funds in hands of Treasurer..	$1,718 93
Life Membership Fund, temporarily invested by Perm. Sec........	242 92
	$1,961 85

About 10.000 copies of the several volumes of Proceedings.

About 600 copies of the first number of the Memoirs.

About 1,500 volumes, parts of volumes, and pamphlets, belonging to the Library.

The indebtedness is only to the Permanent Secretary, as above stated.

F. W. PUTNAM,
Permanent Secretary.

SALEM, August 4, 1876.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

In Account with F. W. Putnam, Permanent Secretary.

1875–76, *Dr.*	
To Expenses of Detroit Meeting	$ 138 89
Printing and Binding 996 copies Detroit vol.	1,910 59
Illustrations " "	67 25
Binding, includ'g 50 copies in cloth " "	33 75
Brass dies for stamping cloth covers	6 00
Wrapping Detroit volume for distribution	10 50
Extra copies of Addresses of President, Vice Presidents, and of Report of Com. on Weights and Measures	37 25
500 copies Constitution and List of Members	38 75
Circulars, Blanks, Tickets, Paper and Envelopes	132 75
Postage and P. O. box rent	208 20
Expressage	43 41
Moving Books to New Office	15 00
Rent of office to July 1, 1876	100 00
Fuel, Light, Wrapping Paper, Twine and other office expenses	79 89
Permanent Secretary balance due last acct.	137 25
Permanent Secretary salary for 1876	1,000 00
Life Membership Fund	50 00
	$4,009 51

1875–6, *Cr.*	
By Assessments previous to 24th Meeting	$ 198 00
Assessments 24th Meeting (Detroit)	1,506 00
Assessments 25th Meeting (Buffalo)	348 00
Entrance fees 24th Meeting	400 00
Entrance fees 25th Meeting	10 00
Fellowship fees	240 00
Life Membership commutation	50 00
Volumes sold and binding	296 41
Postage	41
Balance due Permanent Secretary	960 69
	$4,009 51

I have examined the above account and certify that the same is correctly cast and properly vouched.

H. WHEATLAND, *Auditor.*

SALEM, AUGUST 18, 1876.

STOCK ACCOUNT OF THE PERMANENT SECRETARY.

SINCE the account printed in the Detroit volume, and dated June 2, 1876, 996 copies of the Detroit volume were received from the printer, on July 20, and the following have been distributed: — To members, 662 copies of the Detroit volume and 19 of other volumes; by vote of the Standing Committee, 148 Detroit, and 74 other volumes; and 26 Detroit and 132 other volumes have been sold.

Of the Memoirs, 6 copies have been sold and 11 have been distributed by vote of the Standing Committee.

In the Detroit volume it was stated that a catalogue of the Library of the Association would be published as soon as advantage could be taken of the general catalogue of similar publications in the libraries of Boston and vicinity. This general catalogue is now being printed, and as soon as accessible the Association catalogue will be published. The importance of having the Association catalogue correspond with the general catalogue, the saving of expense to the Association, and the great benefit to be derived from the labors of the careful compiler of the general catalogue, are so evident as to render the present delay of publication of little moment; though the requests from members for a catalogue shows the desirability of its being printed as soon as circumstances will allow.

F. W. PUTNAM,
Permanent Secretary.

Salem, May 1, 1877.

INDEX.