BULLETIN

OF THE

PHILOSOPHICAL SOCIETY

OF

WASHINGTON.

VOL. IV.

Containing the Minutes of the Society from the 185th Meeting, October 9, 1880, to the 202d Meeting, June 11, 1881.

PUBLISHED BY THE CO-OPERATION OF THE SMITHSONIAN INSTITUTION.

WASHINGTON:

1881.

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WASHINGTON, D. C.

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CONSTITUTION

OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

Arlicle I. The name of this Society shall be The Philosophical Society of Washington.

Article II. The officers of the Society shall be a President, four Vice-Presidents, a Treasurer, and two Secretaries.

Article III. There shall be a General Committee, consisting of the officers of the Society and nine other members.

Article IV. The officers of the Society and the other members of the General Committee shall be elected annually by ballot; they shall hold office until their successors are elected, and shall have power to fill vacancies.

Article V. It shall be the duty of the General Committee to make rules for the government of the Society, and to transact all its business.

Article VI. This constitution shall not be amended except by a three-fourths vote of those present at an annual meeting for the election of officers, and after notice of the proposed change shall have been given in writing at a stated meeting of the Society at least four weeks previously.

STANDING RULES

FOR THE GOVERNMENT OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

JANUARY, 1881.

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1. The Stated Meetings of the Society shall be held at 8 o'clock P. M. on every alternate Saturday; the place of meeting to be designated by the General Committee.

2. Notice of the time and place of meeting shall be sent to each member by one of the Secretaries.

When necessary, Special Meetings may be called by the President.

3. The Annual Meeting for the election of officers shall be the last stated meeting in the month of December.

The order of proceedings (which shall be announced by the Chair) shall be as follows:

First, the reading of the minutes of the last Annual Meeting.

Second, the presentation of the annual reports of the Secretaries, including the announcement of the names of members elected since the last annual meeting.

Third, the presentation of the annual report of the Treasurer.

Fourth, the announcement of the names of members who having complied with Section 12 of the Standing Rules, are entitled to vote on the election of officers.

Fifth, the election of President.

Sixth, the election of four Vice-Presidents.

Seventh, the election of Treasurer.

Eighth, the election of two Secretaries.

Ninth, the election of nine members of the General Committee.

Tenth, the consideration of Amendments to the Constitution of the Society, if any such shall have been proposed in accordance with Article VI of the Constitution.

Eleventh, the reading of the rough minutes of the meeting.

4. Elections of officers are to be held as follows:

In each case nominations shall be made by means of an informal ballot, the result of which shall be announced by the Secretary; after which the first formal ballot shall be taken.

In the ballot for Vice-Presidents, Secretaries, and Members of the General Committee, each voter shall write on one ballot as many names as there are officers to be elected, viz., four on the first ballot for Vice-Presidents, two on the first for Secretaries, and nine on the first for Members of the General Committee; and on each subsequent ballot as many names as there are persons yet to be elected; and those persons who receive a majority of the votes cast shall be declared elected.

If in any case the informal ballot result in giving a majority for any one, it may be declared formal by a majority vote.

5. The Stated Meetings, with the exception of the annual meeting, shall be devoted to the consideration and discussion of scientific subjects.

The Stated Meeting next preceding the Annual Meeting shall be set apart for the delivery of the President's Annual Address.

6. Sections representing special branches of science may be formed by the General Committee upon the written recommendation of twenty members of the Society.

7. Persons interested in science, who are not residents of the District of Columbia, may be present at any meeting of the Society, except the annual meeting, upon invitation of a member.

8. Similar invitations to residents of the District of Columbia, not members of the Society, must be submitted through one of the Secretaries to the General Committee for approval.

9. Invitations to attend during three months the meetings of the Society and participate in the discussion of papers, may, by a vote of nine members of the General Committee, be issued to persons nominated by two members.

10. Communications intended for publication under the auspices of the Society shall be submitted in writing to the General Committee for approval.

11. New members may be proposed in writing by three members of the Society for election by the General Committee: but no person shall be admitted to the privileges of membership unless he signifies his acceptance thereof in writing within two months after notification of his election.

12. Each member shall pay annually to the Treasurer the sum of five dollars, and no member whose dues are unpaid shall vote at the annual meeting for the election of officers, or be entitled to a copy of the Bulletin.

In the absence of the Treasurer, the Secretary is authorized to receive the dues of members.

The names of those two years in arrears shall be dropped from the list of members.

Notice of resignation of membership shall be given in writing to the General Committee through the President or one of the Secretaries.

13. The fiscal year shall terminate with the Annual Meeting.

14. Members who are absent from the District of Columbia for more than twelve months may be excused from payment of the annual assessments, in which case their names shall be dropped from the list of members. They can, however, resume their membership by giving notice to the President of their wish to do so.

15. Any member not in arrears may, by the payment of one hundred dollars at any one time, become a life member, and be relieved from all further annual dues and other assessments.

All moneys received in payment of life membership shall be invested as portions of a permanent fund, which shall be directed solely to the furtherance of such special scientific work as may be ordered by the General Committee.

STANDING RULES

OF THE

GENERAL COMMITTEE OF THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

JANUARY, 1881.

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1. The President, Vice-Presidents, and Secretaries of the Society shall hold like offices in the General Committee.

2. The President shall have power to call special meetings of the Committee, and to appoint Sub-Committees.

3. The Sub-Committees shall prepare business for the General Committee, and perform such other duties as may be entrusted to them.

4. There shall be two Standing Sub-Committees; one on Communications for the Stated Meetings of the Society, and another on Publications.

5. The General Committee shall meet at half-past seven o'clock on the evening of each Stated Meeting, and by adjournment at other times.

6. For all purposes except for the amendment of the Standing Rules of the Committee or of the Society, and the election of members, six members of the Committee shall constitute a quorum.

7. The names of proposed new members recommended in conformity with Section 11 of the Standing Rules of the Society, may be presented at any meeting of the General Committee, but shall lie over for at least four weeks before final action, and the concur-

rence of twelve members of the Committee shall be necessary to election.

The Secretary of the General Committee shall keep a chronological register of the elections and acceptances of members.

8. These Standing Rules, and those for the government of the Society, shall be modified only with the consent of a majority of the members of the General Committee.

RULES

FOR THE

PUBLICATION OF THE BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

JANUARY, 1881.

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1. The President's annual address shall be published in full.

2. The annual reports of the Secretaries and of the Treasurer shall be published in full.

3. When directed by the General Committee, any communication may be published in full.

4. Abstracts of papers and remarks on the same will be published, when presented to the Secretary by the author in writing within two weeks of the evening of their delivery, and approved by the Committee on Publications. Brief abstracts prepared by one of the Secretaries and approved by the Committee on Publications may also be published.

5. Communications which have been published elsewhere, so as to be generally accessible, will appear in the Bulletin by title only, but with a reference to the place of publication, if made known in season to the Committee on Publications.

NOTE. *The attention of members to the above rules is specially requested.*

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON

FOR THE YEAR 1881.

President	Joseph Janvier Woodward.	
Vice-Presidents	J. K. Barnes,	W. B. Taylor,
	J. E. Hilgard,	J. C. Welling.
Treasurer	Cleveland Abbe.	
Secretaries	C. E. Dutton,	T. N. Gill.

MEMBERS OF THE GENERAL COMMITTEE.

Thomas Antisell, Garrick Mallery,
John R. Eastman, Simon Newcomb,
E. B. Elliott, John W. Powell,
William Harkness, Charles A. Schott.
Joseph M. Toner,

STANDING COMMITTEES.

On Communications:

C. E. Dutton, Garrick Mallery.

On Publications:

S. F. Baird, T. N. Gill,
C. Abbe, C. E. Dutton.

LIST OF MEMBERS

OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON,

Corrected to July 18th, 1881.

The names of the Founders of the Society, March 13, 1871, are printed in small capitals; for other members the dates of election are given.

§ indicates a life member by payment of 100 dollars.

* indicates absent from the District of Columbia, and excused from dues until announcing their return.

** indicates resigned.

? indicates dropped for non-payment of dues, or nothing known of him.

† indicates deceased.

N. B.—It is scarcely possible for the Treasurer to keep a correct record of those who are absent and excused from paying dues, unless members will keep him duly notified of their removals.

THOMAS ANTISELL.
Cleveland Abbe .... 1871, October 29.
Benjamin Alvord .... 1872, March 23.
Asa O. Aldis .... 1873, March 1.
Sylvanus Thayer Abert .... 1875, January 30.
Robert Stanton Avery .... 1879, October 11.

SPENCER FULLERTON BAIRD.
JOSEPH K. BARNES.
STEPHEN VINCENT BENÉT.
JOHN SHAW BILLINGS.
Orville Elias Babcock .... 1871, June 9.
Henry Hobart Bates .... 1871, November 4.
† Theodorus Bailey .... 1873, March 1.
Thomas W. Bartley .... 1873, March 29.
Samuel Clagett Busey .... 1874, January 17.

Emil Bessels..1875, January 16.
George Bancroft.....................................1875, January 30.
* Lester A. Beardslee...............................1875, February 27.
* Rogers Birnie.....................................1876, March 11.
Marcus Baker..1876, December 2.
Swan Moses Burnett..................................1879, March 29.
Alexander Graham Bell...............................1879, March 29.
William Birney......................................1879, March 29.
Horatio Chapin Burchard.............................1879, May 10.

HORACE CAPRON.
THOMAS LINCOLN CASEY.
† SALMON PORTLAND CHASE.
JOHN HUNTINGTON CRANE COFFIN.
† BENJAMIN FANEUIL CRAIG.
CHARLES HENRY CRANE.
Richard Dominicus Cutts.............................1871, April 29.
* Augustus L. Case..................................1872, November 16.
Robert Craig..1873, January 4.
Elliott Coues.......................................1874, January 17.
Josiah Curtis.......................................1874, March 28.
John White Chickering...............................1874, April 11.
* Frank Wigglesworth Clarke.........................1874, April 11.
Edward Clark..1877, February 24.
Frederick Collins...................................1879, October 21.
Thomas Craig..1879, November 22.
John Henry Comstock.................................1880, February 14.
Alexander Smythe Christie...........................1880, December 4.

WILLIAM HEALEY DALL.
† ALEXANDER B. DYER.
Clarence Edward Dutton..............................1872, January 27.
† Richard Crain Dean................................1872, April 23.
Henry Harrison Chase Dunwoody.......................1873, December 20.
† Charles Henry Davis...............................1874, January 17.
† Frederic William Dorr.............................1874, January 17.
Myrick Hascall Doolittle............................1876, February 12.
** George Dewey.....................................1879, February 15.
Charles Henry Davis.................................1880, June 19.
Theodore Lewis DeLand...............................1880, December 18.
† AMOS BEEBE EATON.
EZEKIEL BROWN ELLIOTT.
** GEORGE H. ELLIOT.
John Robie Eastman..................................1871, May 27.
* Stewart Eldredge..................................1871, June 9.
Fredric Miller Endlich..............................1873, March 1.
? Charles Ewing.....................................1874, January 17.

* Hugh Ewing..........1874, January 17.
John Eaton..........1874, May 8.

* ELISHA FOOTE.
William Ferrel..........1872, November 16.
Edgar Frisby..........1872, November 16.
† John Gray Foster..........1873, January 18.
Edward T. Fristoe..........1873, March 29.
Robert Fletcher..........1873, April 10.
Edward Jessop Farquhar..........1876, February 12.

THEODORE NICHOLAS GILL.
* BENJAMIN FRANKLIN GREEN.
Henry Goodfellow..........1871, November 4.
Grove Karl Gilbert..........1873, June 7.
Leonard Dunnell Gale..........1874, January 17.
* James Terry Gardner..........1874, January 17.
George Brown Goode..........1874, January 31.
Henry Gannett..........1874, April 11.
* Edward Oziel Graves..........1874, April 11.
Edward Miner Gallaudet..........1875, February 27.
Francis Vinton Greene..........1875, April 10.
Francis Mathews Green..........1875, November 9.
Edward Goodfellow..........1875, December 18.
Alexander Young P. Garnett..........1878, March 16.
* Walter Hayden Graves..........1878, May 25.
* Francis Mackall Gunnell..........1879, February 1.
Bernard Richardson Green..........1879, February 15.
William Whiting Godding..........1879, March 29.
James Howard Gore..........1880, March 14.
* Adolphus W. Greely..........1880, June 19.
Albert Leary Gihon..........1880, December 18.

ASAPH HALL.
WILLIAM HARKNESS.
FERDINAND VANDEVEER HAYDEN.
† JOSEPH HENRY.
JULIUS ERASMUS HILGARD.
ANDREW ATKINSON HUMPHREYS.
Henry W. Howgate..........1873, January 18.
* Edward Singleton Holden..........1873, June 21.
† Isaiah Hanscom..........1873, December 20.
* Edwin Eugene Howell..........1874, January 31.
Henry Wetherbee Henshaw..........1874, April 11.
David Lowe Huntingdon..........1877, December 21.
George William Hill..........1879, February 1.

*Peter Conover Hains ---------- 1879, February 15.
*Franklin Benjamin Hough ---------- 1879, March 29.
William Henry Holmes ---------- 1879, March 29.
Ferdinand H. Hassler ---------- 1880, May 8.
William B. Hazen ---------- 1881.

THORNTON ALEXANDER JENKINS.
William Waring Johnston ---------- 1873, June 21.
*Henry Arundel Lambe Jackson ---------- 1875, January 30.
William Nicolson Jeffers ---------- 1877, February 24.
Arnold Burgess Johnson ---------- 1878, January 19.
Joseph Taber Johnson ---------- 1879, March 29.
Owen James ---------- 1880, January 3.

*Reuel Keith ---------- 1871, October 29.
John Jay Knox ---------- 1874, May 8.
Albert Freeman Africanus King ---------- 1875, January 16.
†Ferdinand Kampf ---------- 1875, December 18.
** Clarence King ---------- 1879, May 10.
Jerome H. Kidder ---------- 1880, May 8.
Charles Evans Kilbourne ---------- 1880, June 19.

†JONATHAN HOMER LANE.
Nathan Smith Lincoln ---------- 1871, May 27.
**Henry H. Lockwood ---------- 1871, October 29.
**Stephen C. Lyford ---------- 1873, January 18.
William Lee ---------- 1874, January 17.
*Edward Phelps Lull ---------- 1875, December 4.
Eben Jenks Loomis ---------- 1880, February 14.

†FIELDING BRADFORD MEEK.
MONTGOMERY CUNNINGHAM MEIGS.
†ALBERT J. MYER.
William Myers ---------- 1871, June 23.
†Oscar A. Mack ---------- 1872, January 27.
William Manuel Mew ---------- 1873, December 20.
†Archibald Robertson Marvine ---------- 1874, January 31.
†James William Milner ---------- 1874, January 31.
Garrick Mallery ---------- 1875, January 30.
Otis Tufton Mason ---------- 1875, January 30.
William McMurtrie ---------- 1876, February 26.
Aniceto Gabriel Menocal ---------- 1877, February 24.
Martin Ferdinand Morris ---------- 1877, February 24.
*Montgomery Meigs ---------- 1877, March 24.
*Joseph Badger Marvin ---------- 1878, May 25.
Fredrick Bauders McGuire ---------- 1879, February 15.
? Clay Macauley ---------- 1880, January 3.

SIMON NEWCOMB.
WALTER LAMB NICHOLSON.
*Charles Henry Nichols ------------------1872, May 4.
Charles Nordhoff------------------------1879, May 10.

†GEORGE ALEXANDER OTIS.
John Walter Osborne---------------------1878, December 7.

JOHN GRUBB PARKE.
PETER PARKER.
*TITIAN RAMSAY PEALE.
†BENJAMIN PIERCE.
Charles Christopher Parry ----------------1871, May 13.
** Carlisle P. Patterson -------------------1871, November 17.
* Charles Sanders Pierce -------------------1873, March 1.
Orlando Metcalf Poe -----------------------1873, October 4.
John Wesley Powell ------------------------1874, January 17.
** David Dixon Porter ----------------------1874, April 11.
* Albert Charles Peale----------------------1874, April 11.
Robert Lawrence Packard -----------------1875, February 27.
Henry Martyn Paul ------------------------1877, May 19.
* Henry Smith Pritchett --------------------1879, March 29.
Daniel Webster Prentiss- -----------------1880, January 3.

* Christopher Raymond Perry Rodgers------1872, March 9.
* Joseph Addison Rogers-------------------1872, March 9.
John Rodgers-------------------------------1872, November 16.
* Henry Reed Rathbone---------------------1874, January 17.
* Robert Ridgway --------------------------1874, January 31.
†John Campbell Riley----------------------1877, May 19.
Charles Valentine Riley -------------------1878, November 9.
William Francis McKnight Ritter----------1879, October 21.

BENJAMIN FRANKLIN SANDS.
†GEORGE CHRISTIAN SCHAEFFER.
CHARLES ANTHONY SCHOTT.
WILLIAM TUCUMSEH SHERMAN.
James Hamilton Saville--------------------1871, April 29.
Ainsworth Rand Spofford -----------------1872, January 27.
? Frederic Adolphus Sawyer----------------1873, October 4.
John Sherman -----------------------------1874, January 17.
* John Stearns -----------------------------1874, March 28.
* Ormond Stone ----------------------------1874, March 28.
? Aaron Nicholas Skinner -----------------1875, February 27.
Samuel Shellabarger ----------------------1875, April 10.
David Smith -------------------------------1876, December 2.
Edwin Smith -------------------------------1880, October 23.

* Montgomery Sicard ---------------------- 1877, February 24.
Henry Robinson Searle ---------------------- 1877, December 21.
Charles Dwight Sigsbee ---------------------- 1879, March 1.
John Patten Story ---------------------- 1880, June 19.

William Bower Taylor.
William Calvin Tilden ---------------------- 1871, April 29.
? George Taylor ---------------------- 1873, March 1.
Joseph Meredith Toner ---------------------- 1873, June 7.
Almon Harris Thompson ---------------------- 1875, April 10.
William J. Twining ---------------------- 1878, November 23.
David P. Todd ---------------------- 1878, November 23.

** Jacob Kendrick Upton ---------------------- 1878, February 2.
Winslow Upton ---------------------- 1880, December 4.

George Vasey ---------------------- 1875, June 5.

* Junius B. Wheeler.
Joseph Janvier Woodward.
William Maxwell Wood ---------------------- 1871, December 2.
Francis Amasa Walker ---------------------- 1872, January 27.
James Clarke Welling ---------------------- 1872, November 16.
James Ormond Wilson ---------------------- 1873, March 1.
* George M. Wheeler ---------------------- 1873, June 7.
* John Maynard Woodworth ---------------------- 1874, January 21.
Allen D. Wilson ---------------------- 1874, April 11.
? Charles Warren ---------------------- 1874, May 8.
* Joseph Wood ---------------------- 1875, January 16.
* Christopher Columbus Wolcott ---------------------- 1875, February 27.
Lester Frank Ward ---------------------- 1876, November 18.
Charles Abiathar White ---------------------- 1876, December 16.
Zebulon L. White ---------------------- 1880, June 19.
William Crawford Winlock ---------------------- 1880, December 4.

† Mordecai Yarnall ---------------------- 1871, April 29.
Henry Crissey Yarrow ---------------------- 1874, January 31.

Anton Zumbrock ---------------------- 1875, January 30.

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

185TH MEETING. OCTOBER 9, 1880.

The President in the Chair.

The minutes of the last meeting were read and adopted.

The President notified the meeting of the decease of Prof. PEIRCE' whereupon

Mr. ELLIOTT moved the appointment of a committee of three, to be appointed by the Chair, to draft resolutions in accordance with the notice just given and submit the same at the next meeting.

The Chair appointed as Committee: J. E. HILGARD, J. H. C. COFFIN, and WM. FERRELL.

The treasurer notified the meeting that Vol. 3 of the Bulletin had been published, and that a copy would be forwarded to all members not in arrears.

Mr. C. ABBE communicated the first part of a paper on the Aurora Borealis, referring to studies made by him on the appearance of the aurora of April 4, 1874. He spoke of the difficulty which beset the consideration of the explanation of the appearance of the aurora, and especially of obtaining the altitude of the arch. The present modes of measuring the height yield only negative results, as shown by the experiments of Bravais and Martin, using the trigonometrical method. The second mode employs the varying amount of dip at separate localities, using it according to Galles' method, which assumes the dip of the needle to be of the same amount in the upper regions of the air as at the earth's surface, which has not been proved. Mr. Abbe also referred to Gauss' formula for calculating the direction and intensity of magnetism for all localities, and the defects in Galles' method of calculating the heights of auroras, and concluded that we should look with doubt upon all results obtained.

Mr. Abbe then alluded to a third method which has been used by Prof. Newton: this method is based on the assumption that the Aurora describes an arc running round the earth in a circle parallel to the region of greatest frequency of the aurora; this method involves too many assumptions to justify its adoption. It seems impossible to obtain harmonious results from observations at one locality compared with another; nor can the results be made to harmonize with the three methods.

Mr. Elliott alluded to a generally accepted belief that auroras exist at variable heights in the atmosphere, and synchronous with its existence disturbance of the magnetic needle occurs and great electric disturbance, shown by the irregular working of telegraphic apparatus. In the high regions of the air the currents encounter much less resistance than at the earth level.

Mr. Osborne made remarks on observations made by him on auroras at Melbourne, and on the appearances of the magnetic light in the southern hemisphere.

Mr. Powell considered that auroras could occasionally appear in the lower strata of the atmosphere, and referred to an observation of his own in which the arch was placed between the observer and a mountain.

Mr. Farquhar called attention to the frequent accounts given of the occurence of the aurora at low levels in high latitudes (as in Norway;) and as regards the direction of the flashing of the rays as proceeding from below upwards or *vice versa*, this might be an error of observation, similar to observations on the direction of currents or direction of electric light or of magnetism.

The President remarked in closing the discussion that more careful and systematic observations were necessary to determine the height and position of the auroral streamers, and to substantiate the conclusion that the same streamers could not be seen by observers a few miles apart. He cited the general fact of auroras being seen in the north and not in the south over wide stretches of latitude, as one which seems to him difficult to explain on any theory that the aurora was a local phenomenon.

The meeting then adjourned.

186TH MEETING. OCTOBER 23, 1880.

The President in the Chair.

The minutes of last meeting were read and adopted.

The President notified the meeting of the decease of General A. J. MYER, one of the members of the Society.

Dr. TONER moved the appointment of a committee to draft resolutions suitable to the occasion.

Committee appointed: Messrs. J. C. WELLING, CLEVELAND ABBE, GARRICK MALLERY.

The committee appointed at the last meeting of the Society, to report a resolution commemorative of the decease of Prof. PEIRCE, reported as follows:

Resovled, That the Philosophical Society of Washington put on record their appreciation of the eminent services to science rendered by the late Prof. Benjamin Peirce, of Harvard University, some time since Superintendent of the United States Coast Survey, and during that time a member of this Society. His introduction of the new modes of condensed mathematical thought into celestial mechanics, and his development of new algebraic methods to their uttermost limit, will ever mark him as one of the most powerful mathematicians of our age.

Mr. ALVORD said he had a warm sympathy with this just and appropriate tribute to the memory of Benjamin Peirce. Though he could say much in admiration of his genius and of his works, he would now only make an allusion to a mathematical discussion in which Prof. Peirce referred to his friend Agassiz, for whom he always expressed a warm regard.

In the spring of 1865 Prof. Peirce invited the speaker to attend the meeting, at Northampton, in August of that year, of the National Academy of Science, at which he expected to read a paper. On reaching the room was found arranged around the walls about a dozen large drawings to illustrate the "*Path of the Sling,*" which was his topic. He had obtained an equation of this path. The curve exhibiting this path was very simple in his first drawings and very complicated in the last, according to the changes made in the constants entering into the equation, but the law on the equation of the curve remained the same. The last drawings disclosed highly complex and involved curves not unlike the epicycloids.

Prof. PEIRCE said that these drawings had greatly interested Prof. Agassiz, then absent in his voyage around Cape Horn. It was a striking example of the great varieties and possibilities in nature, buried in the same law. These curves, however apparently different, were traced by the use of the same identical equation, and between the examples exhibited by Prof. Peirce of course myriads of intermediate curves existed. It is obvious that the attraction of all this to Agassiz was the anolagy to organisms in botany and in zoology where groups and species obey some common generalization.

A son of Prof. Peirce has stated that this discussion was never printed, and it is feared that a large share of his brilliant original conception will never be published.

Mr. ELLIOTT referred in warm terms to the genial disposition of Prof. Pierce, and to the encouragement always given by him to young investigators, a characteristic by which he was marked.

Mr. ELLIOTT mentioned that he was the fortunate possessor of a presentation copy of the "Linear Associative Algebra" referred to by Prof. Hilgard, a work which could not fail to impress the investigator with respect and admiration for the great genius of the author.

Prof. HILGARD said he would supplement his first characterization of the ideal algebra, and would call that work the exhaustive treatment of a given mode of investigation, a method of research carried to its uttermost limit and completely exhausted.

Mr. ALVORD stated that Prof. Peirce undoubtedly did a good deal to further the cause of astronomical science by obtaining appropriations to test the value of heights on the Union Pacific Railroad for astronomical observations. In August, 1868, at Chicago, the American Association for the Advancement of Science recommended the establishment of an observatory in that region. Prof. Peirce, as Superintendent of the Coast and Geodetic Survey, had observations made at Sherman Station by Prof. C. A. Young, and on the Sierra Nevada by Prof. Davidson. All this paved the way for the endowment and establishment of the Lick Observatory. These experiments led to the conclusion that the atmosphere of California was most favorable to such observations. The more recent tentative observations of Mr. Burnham at Mount Hamilton confirm these views, and give promise of great success at the Lick Observatory.

Prof. ABBE said that while the scientific and public works of Prof. Peirce would always be spoken of with admiration, his social characteristics were equally interesting. Prof. Abbe could never forget the first time he shook hands with the venerable mathematician in 1860, when he felt that there was a bond of union and sympathy between them. Almost the first words he ever heard him utter gave a glimpse of the man himself. He had heard Prof. Peirce say that the true poet—he who writes the most elevated poetry—is the pure mathematician.

Remarks by Mr. EDWARD GOODFELLOW.

It was my privilege, more than a quarter of a century ago to be ordered to duty under Prof. Peirce's direction, to aid him in certain investigations he was making in behalf of the Coast Survey, with the object of ascertaining the most probable value to be assigned to observations of moon culminations in the determination of differences of longitude.

He was then in the prime of life and upon the threshold of that great fame which his works brought to him but a few years later. He impressed me as a man of thorough kindliness of heart. I came to Cambridge an entire stranger; he interested himself personally in obtaining for me home-like lodgings, and not unfrequently would come to my room to explain in detail, or to write out at length, formulæ which in his own very concise forms had been to me an entire puzzle.

Among the Harvard students he was very popular; his text-books though were less liked than himself. It was a common saying among the collegians, that Prof. Peirce took for granted, in his books, that every one had as clear an insight into mathematics as he himself had.

I was on duty at West Hills, one of the Coast Survey stations on Long Island, in 1865, when Prof. Peirce came to see Mr. Bache, then just returned from Europe, but not with improved health.

Two years later, the death of Prof. Bache created a great vacancy. At that time the character and qualifications of the man who should succeed him in that high office were thoroughly understood. A recognized pre-eminence among scientific men, an ability to form an independent judgment respecting the problems of geodesy involved in the work—these were essentials. It is enough to say of Prof. Peirce that his appointment amply fulfilled these requirements. Foremost among the geometers of his own land, and regarded as in the front rank of foreign mathematicians, Prof. Peirce, during the first years of his superintendency, developed an administrative ability, which, in the methods of its exercise, won for him the friendly regard and respect of both the older and younger officers of the survey. Recognizing, with a fine tact and courtesy, the conditions entailed upon officers engaged in field work—much physical hardship, small pay, and slow promotion—he established a system of gradual increase of pay at certain intervals, and according to merit.

With Government officials, members of Congress, and all whom it was necessary to consult in obtaining appropriations for the survey, Prof. Peirce was never at fault; he knew how to use the legitimate methods of success; and he will long be remembered, not only as a great mathematician, but as the able director of an important national work.

President Newcomb said, as one who had known Prof. Peirce only a little less than a quarter of a century, it might not be inappropriate for him to say a few words, although much that he would have said had been anticipated by those who had already addressed the Society.

One of the most interesting points in Prof. Peirce's character was the fact that he was anything but a mathematician, as conventionally understood—cold, unsympathizing, living in an atmosphere above the rest of the world. Prof. Newcomb had never known any one who had a better heart.

Several members had spoken of the encouragement given by Prof. Peirce to those who first entered upon their life career. The speaker's first interview with that distinguished mathematician had been indelibly impressed upon his mind. What struck him most forcibly about Prof. Peirce at that time was the perfectly unsophisticated way in which he put one at ease, and the total freedom from anything like dignity or pretentiousness which one might suppose would be seen in so great a man. An interesting trait in Prof. Peirce's intellectual character was his disposition to look at the philosophical side of things. Altogether, his mathematical works were as much treatises on formal logic as they were on formal mathematics. The paper on multiple algebra, referred to by Prof. Hilgard, had very much of that character.

Prof. Peirce's method of judging men was peculiar. Among his students he recognized only two classes—those who knew and those who did not know. Owing to the general vivacity of his character he invested the driest subjects with interest. Those who listened to his elocution almost fancied that they understood the highest things he talked about.

Mr. LESTER F. WARD made a communication on the

ANIMAL POPULATION OF THE GLOBE.

He stated that he had recently had occasion to compile, chiefly from official sources, the statistics of live stock in the various countries of the globe from which any data could be obtained, and thought that some of the general results arrived at might possess sufficient scientific interest to warrant laying them before the Society.

The whole number of countries from which information of this character had been collated was twenty-seven, embracing all the countries of Europe except European Turkey, the several British Colonies in Australasia, the Island of Ceylon, Cape Colony and Natal in South Africa, Mauritius, the Dominion of Canada, Newfoundland, Jamaica, the Argentine Republic, Uruguay, Chili, and the United States. The species of animals of which cognizance was alone taken were: horses, mules, asses, horned cattle, sheep, goats, hogs, buffaloes, and reindeer. The reports were very incomplete except with respect to the four leading species, viz: horses, cattle, sheep, and hogs.

The total number of each species actually reported upon was as follows:

Horses - - - - - -	47,181,384
Mules - - - - - -	3,474,391
Asses - - - - - -	2,217,166
Mules and asses, not distinguished -	11,849

Horned cattle - - - -	157,598,521
Sheep - - - - - -	382,763,015
Goats - - - - - -	15,704,911
Hogs - - - - - -	81,691,331
Buffaloes - - - - -	89,281
Reindeer - - - - -	96,567
	690,828,416

The only species for which an estimate had been made of the total number in the world was the sheep. Mr. Robert P. Porter had made such an estimate, which, though varying from the official data in many of the above countries, afforded a basis for extending the figures already obtained to the remaining portions of the globe, and according to which the ovine population of the earth would reach 577,763,015. Using this result as a basis, a very rough estimate of the number of each of the remaining species in regions not already covered by actual enumerations would place the aggregate number of all the species named throughout the world at a little upward of one billion head and their distribution would then be about as follows:

Horses - - - - - -	70,770,597
Cattle - - - - - -	236,397,781
Sheep - - - - - -	577,763,015
Hogs - - - - - -	100,000,000
All other animals - - - -	32,391,247
	1,017,322,640

Reasons were, however, given for regarding this estimate considerably too low, both as to the number of sheep, upon which it is based, and also in the aggregate, and the speaker thought that the latter would probably reach nearly a billion and a half.

Comparisons were then made with the human population. According to a recent work by Baron Kolb the population of the 27 countries, from which reports were obtained, amounted, in 1878, to 366,100,000. This would give, upon an average, in all these countries, 130 horses, 430 cattle, 1,046 sheep, 224 hogs, and 29 of all the remaining animals taken together, to each 1,000 human beings, and for all these species combined, 1,887 animals to each 1,000 of population.

The latest issue of Behm & Wagner's *Bevölkerung der Erde*, (No. 6,) gives the present population of the earth at 1,456,000,000. If the above estimates of the number of each of these classes of animals in the entire world could be relied upon, they would show, for each 1,000 of human population, 50 horses, 166 cattle, 407 sheep, 70 hogs, and 23 of the other species taken together, or 716 of all the kinds enumerated. But, as above stated, these figures are probably far too low, and, if the truth could be known, it would probably be found that the animal population within these limits would not fall far below the human population.

The paper was concluded with some general observations on the moral bearings of the question of animal domestication. It was held that these facts constituted a sufficient justification of man's general treatment of the brute creation; that a larger amount of animal life exists under man's influence than could exist without it; that he creates more life than he destroys; that his methods of destruction are less painful than those of Nature; that it is to his interest to treat animals well, to supply them with abundant food, and relieve them from those constant fears, both of enemies and of want, which characterize their condition in a wild state; and that when life is taken, it is done quickly and as painlessly as possible; that the reverse of all this is the case in Nature, and hence a great amount of human sympathy is wasted on the creatures under man's control in consequence of ignorance of a few facts and principles.

Observations on the foregoing paper were made by Messrs. Elliott and Gill.

The meeting then adjourned.

187th Meeting. 10th Annual Meeting, November 6th, 1880.

Vice-President Hilgard in the Chair.

Thirty-nine members present.

Meeting called to order by the Chair.

The Secretary read proceedings of the last annual meeting (168th meeting) held November 16th, 1879.

The names of members elected since the last annual meeting were announced.

Preliminary to voting, the list of paid up members was read.

The election of officers for the ensuing year was conducted in accordance with the rules of the Society, with the following results:—

President,	JOSEPH JANVIER WOODWARD.
Vice-Presidents,	W. B. TAYLOR, J. C. WELLING, J. E. HILGARD, J. K. BARNES,
Treasurer,	CLEVELAND ABBE.
Secretaries,	T. N. GILL, C. E. DUTTON.

MEMBERS OF THE GENERAL COMMITTEE.

JOHN W. POWELL,	SIMON NEWCOMB,
WILLIAM HARKNESS,	E. B. ELLIOTT,
GARRICK MALLERY,	CHAS. A. SHOTT,
JOHN R. EASTMAN,	THOMAS ANTISELL,

JOS. M. TONER.

It was moved by Mr. COFFIN—

That the consideration of the subject of annual reports to be made by the officers of the Society, be referred to the General Committee, for such action as they may deem desirable.

Adopted.

It was also moved by Mr. COFFIN—

That the General Committee be requested to provide some means for obtaining an annual address from the retiring President and report the same to the Society.

Adopted.

Society then adjourned.

188TH MEETING. NOVEMBER 20, 1880.

The President, Mr. J. J. WOODWARD, in the Chair, and 58 members present.

The newly-elected President addressed a few remarks to the Society, expressive of his high appreciation of the honor conferred upon him by his election as President of the Society, and conveying assurance of his desire and earnest efforts to fill the office acceptably, and to aid in rendering its meetings interesting and instructive.

The Chair announced the appointment of a Committee on Communications, viz: Mr. C. E. Dutton and Mr. Garrick Mallery.

Mr. J. C. Welling then presented, pursuant to a resolution of the Society passed at its 186th meeting, the following preamble and resolution relative to the decease of an honored fellow member, viz., the late General Albert J. Myer:

Whereas in the death of Brigadier General Albert J. Myer, late Chief Signal Officer of the Army, this Society has been called upon to mourn the loss of one of its founders as well as one of its most distinguished members, therefore, be it

Resolved, That in testifying our deep regret at the sudden termination of the useful life of General Myer, while as yet he was apparently in the mid-career of his activity, we, at the same time, would record our admiration of those energetic qualities which he brought to every sphere of duty he was called to fill, and by virtue of which he was able, on the one hand, to organize a system of military signaling highly valuable to the Government in the late war, and, on the other hand, to develop a wide field of usefulness by directing the whole energy of the signal service to the study and the practical applications of the science of meteorology, in both which provinces he displayed a remarkable talent for control and great liberality of public spirit.

Resolved, That these proceedings be entered upon the minutes of the Society.

The first communication of the evening was by Mr. John Jay Knox, entitled

THE DISTRIBUTION OF LOANS IN THE BANK OF FRANCE, THE NATIONAL BANKS OF THE UNITED STATES, AND THE IMPERIAL BANK OF GERMANY.

Mr. Knox first gave a brief outline of the operations of the Bank of France during and since the late Franco-Prussian war. While it appears that the bank deals in very large amounts of money, particular attention was drawn to the fact that it also distributes among the people smaller amounts than the smallest banks in this country, and, in its annual reports of its transactions, prides itself upon the fact that it has rendered services to so many of the humblest citizens. After reciting the amount of commercial paper discounted, the amount of advances on collateral securities, and the amount of securities of the French Government held by it, he proceeded to quote from the bank reports of 1879 the classification of the Paris bills received at the bank:

Bills of 10 fr., or $2 each, and under - -	7,842
Bills of 11 fr. to 50 fr. each, or $2.20 to $10,	392,845
Bills of 51 fr. to 100 fr. each, or $10.20 to $20,	623,232
Bills of above 100 fr. each, or $20 - -	2,878,294
Total - - - - - -	3,902,213

The average value of the bills thus discounted at Paris, in 1879, was 859 francs or $171.80. At the branches of the bank, of which there are ninety, the average amount of the bills discounted was 992 francs or $198.40. Similarly in the year 1878, this average value was, at Paris, 892 francs or $178.40, and in the branches of the bank 992 francs or 198.40. The averages for both the bank and its branches were for 1878, 944 francs or $188.80, and for 1879, 900 francs or $180.00.

The bank of France receives these bills from bankers who keep accounts with it as it discounts only for its depositors. These bankers in turn discount them for small brokers who receive them for this purpose from the working classes. The bills are presented at the bank with accompanying schedules. The rate of interest is the same on small bills as on large ones, and no charge is made beyond this ordinary discount or interest. The greater part of these small bills are promissory notes and issued from small manufacturers, and also from workmen on their own account, known as makers of the *Articles de Paris.* The annual exports of such articles amount it is said to twenty-five millions of dollars, and they consist of nic-nacs, toys, dolls, cheap bronze jewelry, and similar products.

Mr. Knox also gave a classification of the notes and bills discounted and held by the National Banks of the United States on October, 2. 1879, when the total amount of loans was $875,013,107.

Geographical Divisions.	Number of banks.	$100 and less.	Over $100 and less than $500.	$500 and over, but less than $1,000.	$1,000 and over, but less than $5,000.	$5,000 and over, but less than $10,000.	$10,000 and over.	Total.	Amount.	Average.
New England States	547	30,167	54,965	20,444	33,621	10,082	4,590	153,869	$240,552,893 63	$1,563 36
Middle States	641	115,285	132,032	39,484	50,854	11,453	5,276	354,384	416,600,226 30	1,175 56
Southern States	175	15,752	24,480	7,862	8,936	1,283	416	58,729	45,890,807 95	781 40
Western States and Territories	685	90,141	81,563	27,590	31,812	5,381	1,800	241,287	171,969,179 22	712 72
United States	2,048	251,345	296,040	95,382	125,223	28,199	12,081	808,269	$875,013,107 10	$1,082 58

The number of pieces of paper discounted, as will be seen, was 808,269, and the average of each discount, $1,082,59. If the average time of these bills was sixty days, and the banks held continually the same amount, the number of discounts made during the year would be nearly five millions (4,849,614), the total discounts more than five thousand millions (5,250,000,000), which would be equal to a discount of $700 annually for each voter, or $500 for each family in the country. The number of notes and bills of $100 each or less at the date named was 251,345, or nearly one-third of the whole; the number of bills of less than $500 each was 547,385, or considerably more than two-thirds of the whole; while the number of bills of less than $1,000 each was 642,765, which is more than three-fourths of the whole number.

Among the States having the smallest average loans were the following: New York, exclusive of the cities of New York and Albany, $499; Pennsylvania, exclusive of Philadelphia and Pittsburgh, $566; Maryland, exclusive of Baltimore, $505; Kansas, in which the average was $353; Iowa, with an average of $375; West Virginia, of $350; Delaware, $556; New Jersey, $566; Minnesota, $621; Vermont, $645; North Carolina, $667; Tennessee, $651; Maine, $740; Indiana, $711; New Hampshire, $815; South Carolina, $846; Georgia, $882.

The Imperial Bank of Germany has a capital of $30,000,000, and is located in the city of Berlin.

The total number of bills of all kinds discounted during the year 1879 was 2,374,394, amounting to $852,175,650; the average amount of each bill being $358.90. The bills are classified as follows: There were 533,564 town bills, amounting to $263,663,280—average $494.15 each; the number of bills on places in Germany was 1,834,351, amounting to $578,693,335, and averaging $315.47 each; and the number of foreign bills was 6,479, in amount $9,819,035, and averaging $1,515.52 each. The average amount of loans and discounts for the year was $82,073,500.

Mr. E. B. Elliott inquired whether it is desirable that bills of such small amounts as those discounted by the Bank of France should be discounted in this country; if so, what plan could be suggested?

Mr. Knox replied that the savings banks, which receive deposits from all classes and in small amounts, might make small loans.

The laws restrict their investments to the best classes of securities. If there is any class oppressed by the want of loans it is poor people. They have a little money or negotiable property laid aside, upon which they frequently want to borrow, but they find nobody willing to loan upon it. Their only resource is to go to the note shavers and curbstone brokers, who charge them an exorbitant interest. Their wants, in his opinion, could be met by the savings banks.

Mr. J. J. WOODWARD read a communication entitled

RIDDELL'S BINOCULAR MICROSCOPES.—AN HISTORICAL NOTICE,

which is printed in full in the American Monthly Microscopical Journal for December, 1880.

[Abstract.]

Mr. WOODWARD exhibited a large binocular microscope, which he stated had been made for the late Dr. J. L. Riddell, then Professor of Chemistry in the University of Louisiana, during the winter of 1853–4 by the Grunow Brothers, of New Haven, Connecticut, and presented to the Army Medical Museum in April, 1879, by Dr. Riddell's widow.

He said that, although the proper merit of Riddell as a discoverer in this connection had been duly acknowledged by such high continental authorities as Harting and Frey, and even by some English writers, it had been strangely ignored by others, and that even so fair and usually so accurate an author as Dr. Wm. B. Carpenter had fallen into the error of asserting that "the first really satisfactory solution of the problem was that worked out by M. Nachet;" an error the more remarkable in view of the manner in which Riddell's discovery was published and discussed in England, and of the manner in which it had been used by the opticians of that country.

Mr. WOODWARD then offered evidence to show that Riddell was the first to discover and publish the optical principle on which all the really satisfactory binocular microscopes made prior to the present year depend, as well as the inventor of two efficient and still much employed methods of applying that principle; one suitable for the simple or dissecting microscope, the other for the compound microscope.

Riddell's discovery was, briefly, that the cone of rays proceeding from a single objective may be so divided by means of reflecting prisms, placed as close behind the posterior combination of the objective as possible, that orthoscopic binocular vision can be obtained both with the simple and the compound microscope. This discovery, together with an account of one method of carrying it out, and a suggestion of the feasibility of other methods, was published by Riddell in the New Orleans Monthly Medical Register for October, 1852, p. 4, and subsequently in the American Journal of Science and Arts, for January, 1853, p. 68. This article was reprinted in London, in the Quarterly Journal of Microscopical Science for April 1853. (Vol. I, 1853, p. 236.)

The contrivance described in this first paper was found by Riddell to give orthoscopic binocular vision when used without eye-pieces, but when ordinary eye-pieces were employed a pseudoscopic effect was obtained. This he obviated by the use of erecting eye-pieces; but, soon after his first paper was published, Riddell devised a second plan, which gave orthoscopic binocular vision with ordinary eye-pieces, and which he subsequently always used for the compound microscope, reserving his first plan for the dissecting (simple) microscope.

A brief notice, containing, however, a correct description of Riddell's second plan, was published in the New Orleans Monthly Medical Register for April, 1853, (p. 78,) and reprinted in London in the Quarterly Journal of Microscopical Science, Vol. I, 1853, (p. 304.) Subsequently, July 30, 1853, Riddell exhibited a dissecting (simple) microscope on his old plan and a compound microscope on his new plan to the American Association for the Advancement of Science, and read a paper describing those instruments, and pretty fully discussing the principles involved. This paper was published in the Proceedings of the Association, Vol. VII, for 1853, (p. 16,) and in the New Orleans Medical and Surgical Journal for November, 1853, (p. 321.) It was reprinted in London in the Quarterly Journal of Microscopical Science for January, 1854, Vol. II, (p. 18.)

Mr. Woodward then related the manner in which Riddell's discovery was discussed at the time, in England, by Messrs. Wheatstone and Wenham, and on the continent by M. M. Harting and Nachet. He showed that Nachet's modification of the compound microscope was suggested by Riddell's first instrument, and that Nachet's excellent binocular dissecting (simple) microscope is, in its optical parts, a literal copy of the binocular dissecting (simple) microscope exhibited by Riddell at the Cleveland meeting in July,

1853. This is also true of the binocular dissecting microscopes made of late years by Beck, of London, while the highly lauded erecting binocular microscope of Mr. J. W. Stephenson, F. R. M. S., (1870–72,) is, in its optical parts, a copy of the binocular compound microscope exhibited by Riddell at the Cleveland meeting. The latter instrument, as then exhibited, although optically efficient, was roughly put together by Riddell's own hands. The instrument exhibited by Mr. Woodward was ordered by Riddell of the Grunow Brothers, in August, 1853, and delivered to him by them in March following. In its optical parts it is a copy of the model exhibited at the Cleveland meeting, but some improvements were made in the mechanical details of its construction.

J. S. Billings then made some remarks upon

THE SCIENTIFIC WORK CARRIED ON UNDER THE DIRECTION OF THE NATIONAL BOARD OF HEALTH.

Prof. Ira Remsen, of the Johns Hopkins University, has made for the Board an investigation on the organic matter in the air. By the use of tubes, filled with prepared pumice stone, all the nitrogenous matter in the air to be examined, was removed, and its quantity determined by the usual tests for free and albuminoid ammonia.

Air contaminated by being drawn through water containing decaying meat does not yield more than the usual quantity of albuminoid ammonia.

Air contaminated by being drawn over comparatively dry decaying organic matter yields more than the usual quantity of albuminoid ammonia.

Air contaminated by respiration yields more than the usual quantity of albuminoid ammonia.

The simple statement of fact that a given sample of air yields an abnormally large quantity of albuminoid ammonia is not sufficient to enable us to draw a conclusion with reference to the purity of the air. We must know at what season of the year the air was collected, and whether in the city or country; in fact, we should know everything possible concerning the air, and then let the conclusion finally drawn be a resultant of all the facts. It is probable, however, from what is now known, that the determination of the amount of albuminoid ammonia yielded by air may, under many circumstances, furnish us with important information concerning the quality of the air, but great caution is necessary in dealing with this principle of examination.

A series of investigations upon the effects of various soils upon ordinary sewage has been carried on under the direction of Prof. Pumpelly, of the United States Geological Survey, assisted by Prof. Smythe. The preliminary experiments related to the removal of living organisms from air and fluids by passing these through filters of various kinds, and then testing their effects upon solutions containing organic matter and susceptible of fermentative or putrefactive changes. A very large number of such solutions have been prepared and preserved under various conditions, and in no case has anything like fermentation or the development of the lower organisms been observed, unless under circumstances where the lower organisms could be introduced from without, thus giving strong negative evidence against the theory of spontaneous generation. The filtration of air from such germs was found to be a comparatively easy matter. Passing it through an inch of fine sand deprived it of the power of producing fermentative changes. On the other hand, the removal of bacteroidal organisms from water was much more difficult, filtration through many feet of fine sand being insufficient to effect it. The results reported by Wernich are confirmed, viz., that air passing over putrefying fluids or moist putrefying surfaces does not take up organisms therefrom, nor does it become contaminated by passing over dried bacteria films on smooth compact surfaces such as glass or iron. From woven stuffs, however, it is readily contaminated, and wherever there is dust there is danger.

The results obtained by Dr. Bigelow in attempting to destroy the vitality of dried bacteria films by means of gaseous disinfectants were then mentioned. It is found that time is an important element in the matter, and that long exposures are necessary to secure complete destruction of vitality of such organisms. This may explain the failures to disinfect the Plymouth and the Excelsior by gaseous disinfectants.

Drs. H. C. Wood and H. F. Fremont have made a number of experiments on the inoculation of diphtheria on the lower animals with negative results. The theory of Oertel that this disease is due to specific bacteria is not confirmed by their observations. They state that their results seem to indicate that the contageous material of diphtheria is of the nature of a septic poison which is locally very irritating to the mucous membrane, and that the disease may be often a purely local affection to be treated by local remedies.

Dr. G. M. Sternberg has been repeating the experiments of Klebs and Tommasi-Crudelli on the bacillus malariæ. He finds in the malarious swamps around New Orleans, organisms not distinguishable from those figured by the authors referred to, and on cultivating them in gelatin solutions obtains a similar bacillus. He has not however obtained any specific effects by injecting these organisms into the blood of animals and is unable to confirm the conclusions announced by Klebs.

Dr. Chas. Smart, U. S. A., has been engaged on water analysis, and for the last seven months on the adulterations of food. From an analysis of over six hundred samples he concludes that while there is a considerable amount of adulteration in such articles as ground coffee and spices there is not much that is dangerous to health—in the words of the last British Parliamentary Commission we are cheated but not poisoned. Poisonous colors derived from lead and antimony are found in some candies.

The educational work of the Board was then referred to, and more especially its efforts to secure a uniform and satisfactory mode of reporting mortality statistics.

At the conclusion of Mr. Billings' remarks the society adjourned.

189TH MEETING DECEMBER 4, 1880.

The President in the chair.

Forty-eight members present.

The minutes of the last meeting were read and adopted.

The Chair announced to the Society the election and acceptance of the following new members: ALEXANDER SMYTHE CHRISTIE, WILLIAM CRAWFORD WINLOCK, and WINSLOW UPTON.

The Chair also announced the appointment of Mr. WILLIAM HARKNESS as an additional member of the Standing Committee on Communications.

The Society then listened to the address of the retiring President, Mr. SIMON NEWCOMB, on

THE RELATION OF SCIENTIFIC METHOD TO SOCIAL PROGRESS.

AMONG those subjects which are not always correctly apprehended, even by educated men, we may place that of the true significance of scientific method, and the relations of such method to practical affairs. This is especially apt to be the case in a country like our own, where the points of contact between the scientific world on the one hand, and the industrial and political world on the other, are fewer than in other civilized countries. The form which this misapprehension usually takes is that of a failure to appreciate the character of scientific method, and especially its analogy to the methods of practical life. In the judgment of the ordinary intelligent man there is a wide distinction between theoretical and practical science. The latter he considers as that science directly applicable to the building of railroads, the construction of engines, the invention of new machinery, the construction of maps, and other useful objects. The former he considers analogous to those philosophic speculations in which men have indulged in all ages without leading to any result which he considers practical. That our knowledge of nature is increased by its prosecution is a fact of which he is quite conscious, but he considers it as terminating with a mere increase of knowledge, and not as having in its method anything which a person devoted to material interests can be expected to appreciate.

This view is strengthened by the spirit with which he sees scientific investigation prosecuted. It is well understood on all sides that when such investigations are pursued in a spirit really recognized as scientific, no merely utilitarian object is had in view. Indeed it is easy to see how the very fact of pursuing such an object would detract from that thoroughness of examination which is the first condition of a real advance. True science demands in its every research a completeness far beyond what is apparently necessary for its practical applications. The precision with which the astronomer seeks to measure the heavens, and the chemist to determine the relations of the ultimate molecules of matter has no limit, except that set by the imperfections of the instruments of research. There is no such division recognized as that of useful and useless knowledge. The ultimate aim is nothing less than that of bringing all the phenomena of nature under laws as exact as those which govern the planetary motions.

Now the pursuit of any high object in this spirit commands from men of wide views that respect which is felt towards all exertion having in view more elevated objects than the pursuit of gain. Accordingly it is very natural to classify scientists, and philosophers with the men who in all ages have sought after learning instead of utility. But there is another aspect of the question which will show the relations of scientific advance to the practical affairs of life in a different light. I make bold to say that the greatest want of the day, from a purely practical point of view, is the more general introduction of the scientific method and the scientific spirit into the discussion of those political and social problems which we encounter on our road to a higher plane of public well being. Far from using methods too refined for practical purposes, what most distinguishes scientific from other thought is the introduction of the methods of practical life into the discussion of abstract general problems. A single instance will illustrate the lesson I wish to enforce.

The question of the tariff is, from a practical point of view, one of the most important with which our legislators will have to deal during the next few years. The widest diversity of opinion exists as to the best policy to be pursued in collecting a revenue from imports. Opposing interests contend against each other without any common basis of fact or principle on which a conclusion can be reached. The opinions of intelligent men differ almost as widely as those of the men who are immediately interested. But all will admit that public action in this direction should be dictated by one guiding principle—that the greatest good of the community is to be sought after. That policy is the best which will most promote this good. Nor is there any serious difference of opinion as to the nature of the good to be had in view; it is in a word the increase of the national wealth and prosperity. The question on which opinions fundamentally differ is that of the effects of a higher or lower rate of duty upon the interests of the public. If it were possible to foresee, with an approach to certainty, what effect a given tariff would have upon the producers and consumers of an article taxed, and, indirectly, upon each member of the community in any way interested in the article, we should then have an exact datum which we do not now possess for reaching a conclusion. If some superhuman authority, speaking with the voice of infallibility, could give us this information, it is evident that a great national want would be supplied. No question in practical life is more important than this: How can this desirable knowledge of the economic effects of a tariff be obtained?

The answer to this question is clear and simple. The subject must be studied in the same spirit, and, to a certain extent, by the same methods which have been so successful in advancing our knowledge of nature. Every one knows that, within the last two centuries, a method of studying the course of nature has been introduced which has been so successful in enabling us to trace the sequence of cause and effect as almost to revolutionize society. The very fact that scientific method has been so successful here leads to the belief that it might be equally successful in other departments of inquiry.

The same remarks will apply to the questions connected with banking and currency; the standard of value; and, indeed, all subjects which have a financial bearing. On every such question we see wide differences of opinion without any common basis to rest upon.

It may be said, in reply, that in these cases there are really no grounds for forming an opinion, and that the contests which arise over them are merely those between conflicting interests. But this claim is not at all consonant with the form which we see the discussion assume. Nearly every one has a decided opinion on these several subjects; whereas, if there were no data for forming an opinion, it would be unreasonable to maintain any whatever. Indeed, it is evident that there must be truth somewhere, and the only question that can be open is that of the mode of discovering it. No man imbued with a scientific spirit can claim that such truth is beyond the power of the human intellect. He may doubt his own ability to grasp it, but cannot doubt that by pursuing the proper method and adopting the best means the problem can be solved. It is, in fact, difficult to show why some exact results could not be as certainly reached in economic questions as in those of physical science. It is true that if we pursue the inquiry far enough we shall find more complex conditions to encounter, because the future course of demand and supply enters as an uncertain element. But a remarkable fact to be considered is that the difference of opinion to which we allude does not depend upon different estimates of the future, but upon different views of the most elementary and general principles of the subject. It is as if men were not agreed whether air were elastic or whether the earth turns on its axis. Why is it that while in all subjects of physical science we find a general agreement through a wide range of subjects, and doubt commences only where certainty is not attained, yet when we turn to economic subjects we do not find the beginning of an agreement?

No two answers can be given. It is because the two classes of subjects are investigated by different instruments and in a different spirit. The physicist has an exact nomenclature; uses methods of research well adapted to the objects he has in view; pursues his investigations without being attacked by those who wish for different results; and, above all, pursues them only for the purpose of discovering the truth. In economical questions the case is entirely different. Only in rare cases are they studied without at least the suspicion that the student has a preconceived theory to support. If results are attained which oppose any powerful interest, this interest can hire a competing investigator to bring out a different result. So far as the public can see, one man's result is as good as another's, and thus the object is as far off as ever. We may be sure that until there is an intelligent and rational public, able to distinguish between the speculations of the charlatan and the researches of the investigator, the present state of things will continue. What we want is so wide a diffusion of scientific ideas that there shall be a class of men engaged in studying economical problems for their own sake, and an intelligent public able to judge what they are doing. There must be an improvement in the objects at which they aim in education, and it is now worth while to inquire what that improvement is.

It is not mere instruction in any branch of technical science that is wanted. No knowledge of chemistry, physics, or biology, however extensive, can give the learner much aid in forming a correct opinion of such a question as that of the currency. If we should claim that political economy ought to be more extensively studied, we would be met by the question, which of several conflicting systems shall we teach? What is wanted is not to teach this system or that, but to give such a training that the student shall be able to decide for himself which system is right.

It seems to me that the true educational want is ignored both by those who advocate a classical and those who advocate a scientific education. What is really wanted is to train the intellectual powers, and the question ought to be, what is the best method of doing this? Perhaps it might be found that both of the conflicting methods could be improved upon. The really distinctive features, which we should desire to see introduced, are two in number: the one the scientific spirit; the other the scientific discipline. Although many details may be classified under each of these heads, yet there is one of pre-eminent importance on which we should insist.

The one feature of the scientific spirit which outweighs all others in importance is the love of knowledge for its own sake. If by our system of education we can inculcate this sentiment we shall do what is, from a public point of view, worth more than any amount of technical knowledge, because we shall lay the foundation of all knowledge. So long as men study only what they think is going to be useful their knowledge will be partial and insufficient. I think it is to the constant inculcation of this fact by experience, rather than to any reasoning, that is due the continued appreciation of a liberal education. Every business man knows that a business-college training is of very little account in enabling one to fight the battle of life, and that college bred men have a great advantage even in fields where mere education is a secondary matter. We are accustomed to seeing ridicule thrown upon the questions sometimes asked of candidates for the civil service because the questions refer to subjects of which a knowledge is not essential. The reply to all criticisms of this kind is that there is no one quality which more certainly assures a man's usefulness to society than the propensity to acquire useless knowledge. Most of our citizens take a wide interest in public affairs, else our form of government would be a failure. But it is desirable that their study of public measures should be more critical and take a wider range. It is especially desirable that the conclusions to which they are led should be unaffected by partisan sympathies. The more strongly the love of mere truth is inculcated in their nature the better this end will be attained.

The scientific discipline to which I ask mainly to call your attention consists in training the scholar to the scientific use of language. Although whole volumes may be written on the logic of science there is one general feature of its method which is of fundamental significance. It is that every term which it uses and every proposition which it enunciates has a precise meaning which can be made evident by proper definitions. This general principle of scientific language is much more easily inculcated by example than subject to exact description ; but I shall ask leave to add one to several attempts I have made to define it. If I should say that when a statement is made in the language of science the speaker knows what he means, and the hearer either knows it or can be made to know it by proper definitions, and that this community of understanding is frequently not reached in other departments of thought, I might be understood as casting a slur on whole departments of inquiry. Without intending any such slur, I may still say that language and statements are worthy of the name scientific as they approach this standard; and, moreover, that a great deal is said and written which does not fulfill the requirement. The fact that words lose their meaning when removed from the connections in which that meaning has been acquired and put to higher uses, is one which, I think, is rarely recognized. There is nothing in the history of philosophical inquiry more curious than the frequency of interminable disputes on subjects where no agreement can be reached because the opposing parties do not use words in the same sense. That the history of science is not free from this reproach is shown by the fact of the long dispute whether the force of a moving body was proportional to the simple velocity or to its square. Neither of the parties to the dispute thought it worth while to define what they meant by the word "force," and it was at length found that if a definition was agreed upon the seeming difference of opinion would vanish. Perhaps the most striking feature of the case, and one peculiar to a scientific dispute, was that the opposing parties did not differ in their solution of a single mechanical problem. I say this is curious, because the very fact of their agreeing upon every concrete question which could have been presented, ought to have made it clear that some fallacy was lacking in the discussion as to the measure of force. The good effect of a scientific spirit is shown by the fact that this discussion is almost unique in the history of science during the past two centuries, and that scientific men themselves were able to see the fallacy involved, and thus to bring the matter to a conclusion.

If we now turn to the discussions of philosophers, we shall find at least one yet more striking example of the same kind. The question of the freedom of the human will has, I believe, raged for centuries. It cannot yet be said that any conclusion has been reached. Indeed I have heard it admitted by men of high intellectual attainments that the question was insoluble. Now a curious feature of this dispute is that none of the combatants, at least on the affirmative side, have made any serious attempt to define what should be meant by the phrase freedom of the will, except by using such terms as require definition equally with the word freedom itself. It can, I conceive, be made quite clear that the assertion, "The will is free," is one without meaning, until we analyze more fully the different meanings to be attached to the word free. Now this word has a perfectly well-defined signification in every day life. We say that anything is free when it is not subject to external constraint. We also know exactly what we mean when we say that a man is free to do a certain act. We mean that if he chooses to do it there is no external constraint acting to prevent him. In all cases a relation of two things is implied in the word, some active agent or power, and the presence or absence of another constraining agent. Now, when we inquire whether the will itself is free, irrespective of external constraints, the word free no longer has a meaning, because one of the elements implied in it is ignored.

To inquire whether the will itself is free is like inquiring whether fire itself is consumed by the burning, or whether clothing is itself clad. It is not, therefore, at all surprising that both parties have been able to dispute without end, but it is a most astonishing phenomenon of the human intellect that the dispute should go on generation after generation without the parties finding out whether there was really any difference of opinion between them on the subject. I venture to say that if there is any such difference, neither party has ever analyzed the meaning of the words used sufficiently far to show it. The daily experience of every man, from his cradle to his grave, shows that human acts are as much the subject of external causal influences as are the phenomena of nature. To dispute this would be little short of the ludicrous. All that the opponents of freedom, as a class, have ever claimed, is the assertion of a causal connection between the acts of the will, and influences independent of the will. True, propositions of this sort can be expressed in a variety of ways connoting an endless number of more or less objectionable ideas, but this is the substance of the matter.

To suppose that the advocates on the other side meant to take issue on this proposition would be to assume that they did not know what they were saying. The conclusion forced upon us is that though men spend their whole lives in the study of the most elevated department of human thought it does not guard them against the danger of using words without meaning. It would be a mark of ignorance, rather than of penetration, to hastily denounce propositions on subjects we are not well acquainted with because we do not understand their meaning. I do not mean to intimate that philosophy itself is subject to this reproach. When we see a philosophical proposition, couched in terms we do not understand, the most modest and charitable view is to assume that this arises from our lack of knowledge. Nothing is easier than for the ignorant to ridicule the propositions of the learned. And yet, with every reserve, I cannot but feel that the disputes to which I have alluded prove the necessity of bringing scientific precision of language into every demand of thought. If the discussion had been confined to a few, and other philosophers had analyzed the subject, and showed the fictitious character of the discussion, or had pointed out where opinions really might differ, there would be nothing derogatory to philosophers. But the most suggestive circumstance is that although a large proportion of the philosophic writers in recent times have devoted more or less attention to the subject, few, or none, have made even this modest contribution. I speak with some little confidence on this subject, because several years ago I wrote to one of the most acute thinkers of the country, asking if he could find in philosophical literature any terms or definitions expressive of the three different senses in which not only the word freedom, but nearly all words implying freedom were used. His search was in vain.

Nothing of this sort occurs in the practical affairs of life. All terms used in business, however general or abstract, have that well-defined meaning which is the first requisite of the scientific language. Now one important lesson which I wish to inculcate is that the language of science in this respect corresponds to that of business; in that each and every term that is employed has a meaning as well defined as the subject of discussion can admit of. It will be an instructive exercise to inquire what this peculiarity of scientific and business language is. It can be shown that a certain requirement should be fulfilled by all language intended for the discovery of truth, which is fulfilled only by the two classes of language which I have described. It is one of the most common errors of discourse to assume that any common expression which we may use always conveys an idea, no matter what the subject of discourse. The true state of the case can, perhaps, best be seen by beginning at the foundation of things, and examining under what conditions language can really convey ideas.

Suppose thrown among us a person of well-developed intellect, but unacquainted with a single language or word that we use. It is absolutely useless to talk to him, because nothing that we say conveys any meaning to his mind. We can supply him no dictionary, because by hypothesis he knows no language to which we have access. How shall we proceed to communicate our ideas to him? Clearly there is but one possible way, namely, through his five senses. Outside of this means of bringing him in contact with us we can have no communication with him. We, therefore, begin by showing him sensible objects, and letting him understand that certain words which we use correspond to those objects. After he has thus acquired a small vocabulary, we make him understand that other terms refer to relations between objects which he can perceive by his senses. Next he learns, by induction, that there are terms which apply not to special objects, but to whole classes of objects. Continuing the same process, he learns that there are certain attributes of objects made known by the manner in which they affect his senses, to which abstract terms are applied. Having learned all this, we can teach him new words by combining words without exhibiting objects already known. Using these words we can proceed yet further, building up, as it were, a complete language. But there is one limit at every step. Every term which we make known to him must depend ultimately upon terms the meaning of which he has learned from their connection with special objects of sense.

To communicate to him a knowledge of words expressive of mental states it is necessary to assume that his own mind is subject to these states as well as our own, and that we can in some way indicate them by our acts. That the former hypothesis is sufficiently well established can be made evident so long as a consistency of different words and ideas is maintained. If no such consistency of meaning on his part were evident, it might indicate that the operations of his mind were so different from ours that no such communication of ideas was possible. Uncertainty in this respect must arise as soon as we go beyond those mental states which communicate themselves to the senses of others.

We now see that in order to communicate to our foreigner a knowledge of language, we must follow rules similar to those necessary for the stability of a building. The foundation of the building must be well laid upon objects knowable by his five senses. Of course the mind, as well as the external object, may be a factor in determining the ideas which the words are intended to express; but this does not in any manner invalidate the conditions which we impose. Whatever theory we may adopt of the relative part played by the knowing subject, and the external object in the acquirement of knowledge, it remains none the less true that no knowledge of the meaning of a word can be acquired except through the senses, and that the meaning is, therefore, limited by the senses. If we transgress the rule of founding each meaning upon meanings below it, and having the whole ultimately resting upon a sensuous foundation, we at once branch off into sound without sense. We may teach him the use of an extended vocabulary, to the terms of which he may apply ideas of his own, more or less vague, but there will be no way of deciding that he attaches the same meaning to these terms that we do.

What we have shown true of an intelligent foreigner is necessarily true of the growing man. We come into the world without a knowledge of the meaning of words, and can acquire such knowledge only by a process which we have found applicable to the intelligent foreigner. But to confine ourselves within these limits in the use of language requires a course of severe mental discipline. The transgression of the rule will naturally seem to the undisciplined mind a mark of intellectual vigor rather than the reverse. In our system of education every temptation is held out to the learner to transgress the rule by the fluent use of language to which it is doubtful if he himself attaches clear notions, and which he can never be certain suggests to his hearer the ideas which he intends. Indeed, we not infrequently see, even among practical educators, expressions of positive antipathy to scientific precision of language so obviously opposed to good sense that they can be attributed only to a failure to comprehend the meaning of the language which they criticise.

Perhaps the most injurious effect in this direction arises from the natural tendency of the mind, when not subject to a scientific discipline, to think of words expressing sensible objects and their relations as connoting certain supersensuous attributes. This is frequently seen in the repugnance of the metaphysical mind to receive a scientific statement about a matter of fact simply as a matter of fact. This repugnance does not generally arise in respect to the every day matters of life. When we say that the earth is round we state a truth which every one is willing to receive as final. If without denying that the earth was round, one should criticise the statement on the ground that it was not necessarily round but might be of some other form, we should simply smile at this use of language. But when we take a more general statement and assert that the laws of nature are inexorable, and that all phenomona, so far as we can show, occur in obedience to their requirements, we are met with a sort of criticism with which all of us are familiar, and which I am unable adequately to describe. No one denies that as a matter of fact, and as far as his experience extends, these laws do appear to be inexorable. I have never heard of any one professing, during the present generation, to describe a natural phenomenon, with the avowed belief that it was not a product of natural law; yet we constantly hear the scientific view criticised on the ground that events may occur without being subject to natural law. The word "may," in this connection, is one to which we can attach no meaning expressive of a sensuous relation.

This is, however, not the most frequent misuse of the word may. In fact, the unscientific use of language to which I refer, is most strongly shown in disquisitions on the freedom of the will. When I say that it is perfectly certain that I will to-morrow perform a certain act unless some cause external to my mind which I do not now foresee occurs to prevent me, I make a statement which is final so far as scientific ideas are concerned. But it will sometimes be maintained that however certain it may be that I shall perform this act, nevertheless I may act otherwise. All I can say to this is that I do not understand the meaning of the statement.

The analogous conflict between the scientific use of language and the use made by some philosophers, is found in connection with the idea of causation. Fundamentally the word cause is used in scientific language in the same sense as in the language of common life. When we discuss with our neighbors the cause of a fit of illness, of a fire, or of cold weather, not the slightest ambiguity attaches to the use of the word, because whatever meaning may be given to it is founded only on an accurate analysis of the ideas involved in it from daily use. No philosopher objects to the common meaning of the word, yet we frequently find men of eminence in the intellectual world who will not tolerate the scientific man in using the word in this way. In every explanation which he can give to its use they detect ambiguity. They insist that in any proper use of the term the idea of power must be connoted. But what meaning is here attached to the word power, and how shall we first reduce it to a sensible form, and then apply its meaning to the operations of nature? That this can be done, I by no means deny. All I maintain is that if we shall do it, we must pass without the domain of scientific statement.

Perhaps the greatest advantage in the use of symbolic and other mathematical language in scientific investigation is that it cannot possibly be made to connote anything except what the speaker means. It adheres to the subject matter of discourse with a tenacity which no criticism can overcome. In consequence, whenever a science is reduced to a mathematical form its conclusions are no longer the subject of philosophical attack. To secure the same desirable quality in all other scientific language it is necessary to give it, so far as possible, the same simplicity of signification which attaches to mathematical symbols. This is not easy, because we are obliged to use words of ordinary language, and it is impossible to divest them of whatever they may connote to ordinary hearers.

I have thus sought to make it clear that the language of science corresponds to that of ordinary life, and especially of business life, in confining its meaning to phenomena. An analogous statement may be made of the method and objects of scientific investigation. I think Professor Clifford was very happy in defining science as organized common sense. The foundation of its widest general creations is laid, not in any artificial theories, but in the natural beliefs and tendencies of the human mind. Its position against those who deny these generalizations is quite analogous to that taken by the Scottish school of philosophy against the skepticism of Hume.

It may be asked, if the methods and language of science correspond to those of practical life,—why is not the every day discipline of that life as good as the discipline of science? The answer is, that the power of transferring the modes of thought of common life to subjects of a higher order of generality is a rare faculty which can be acquired only by scientific discipline. What we want is that in public affairs men shall reason about questions of finance, trade, national wealth, legislation and administration with the same consciousness of the practical side that they reason about their own interests. When this habit is once acquired and appreciated, the scientific method will naturally be applied to the study of questions of social policy. When a scientific interest is taken in such questions, their boundaries will be extended beyond the utilities immediately involved, and then the last condition of unceasing progress will be complied with.

At the conclusion of Mr. Newcomb's address it was moved by Mr. Hilgard that the thanks of the Society are due to Mr. Newcomb for his weighty, instructive, and interesting address.

The motion was carried.

Mr. J. E. Hilgard then made a communication on the subject of

A MODEL OF THE BASIN OF THE GULF OF MEXICO.

He exhibited to the Society a model of the Gulf of Mexico recently constructed under the direction of the Coast Survey Office upon data obtained by a very great number of soundings. Of these many thousands have been made, and the model is believed to be very correct. As constructed, the vertical scale is thirty times as great as the horizontal in order to emphasize and render easily intelligible the most notable features.

The soundings of the waters in the Gulf of Mexico began with the extension thither of the work of the Coast Survey, but they were at first only littoral and tributary to the topographic and hydrographic work of the Bureau. They were interrupted by the civil war, but were resumed at its close. Soundings had also been made off the east coast of Florida to ascertain the nature and dimensions of the outlet of the Gulf stream. This outlet was found to be relatively quite small. Soundings and temperatures had been taken from Florida to Cuba and to Yucatan. Within a few years the work of exploring the general configuration of the Gulf of Mexico has been commenced by Commander Sigsbee, of the Navy, on duty in the Coast and Geodetic Survey. This officer made great improvements in deep-sea sounding apparatus, and, prosecuting the exploration with great energy and ingenuity, has brought the work to a speedy conclusion.

As a result of these investigations, it is found that the continental profiles which descend from every direction beneath the water of the gulf, have, at first, a very gradual slope of a few feet to the mile—until the 100 fathom depth, or thereabout, is reached. They then descend much more rapidly, and, in some places, with singular abruptness to depths exceeding 2,000 fathoms. All around the gulf shores is a marginal belt of varying width and of comparatively shallow water. Within this marginal belt is an area of similar shape to that of the gulf itself, and nearly concentric with its coast, where the depth is comparable to that of mid-ocean. The extent of the deeper area is about 50,000 square miles. It also appears that the continental or peninsular mass of Florida is of much greater area than that portion which exposes its surface above the water, and the same is true of Yucatan. An examination of the portions in the vicinity of the Mississippi river, shows that the delta has very nearly reached the position where the profile begins to drop rapidly down into deep water, and the apprehensions of those who fear that the jetties lately constructed may cause the accumulation of deposits further out may therefore be dispelled or greatly mitigated.

Turning to the channel of the Gulf stream, Mr. Hilgard remarked that its transverse section between Florida and the Bahama Banks, did not exceed twelve square miles. With an average current velocity of only 2½ miles per hour, it appears quite incredible that enough water can be dischaged through this passage to occasion the mild climate of western Europe. The main mass of the great oceanic drift which warms these shores, he thought must be derived from the Caribbean Sea, passing out between the greater Antilles, where the passes are far wider and deeper. Of this greater oceanic drift the efflux through the Florida straits forms but a small part.

Remarks upon this communication were made by Messrs. ALVORD, DUTTON, GILL, HARKNESS and WHITE.

The Society then adjourned.

190TH MEETING. DECEMBER 18, 1880.

The President in the Chair.

Forty-two members present.

The minutes of the last meeting were read and adopted.

A communication was then read by Mr. SWAN M. BURNETT, entitled

COLOR PERCEPTION AND COLOR BLINDNESS.

The speaker first gave the Young-Helmholtz theory, which consists in the assumption of three fibres in the retina corresponding to the so-called fundamental colors, red, green and violet, stating the objections that have been brought against this theory by Mauthner and others, when viewed from the standpoint of color blindness.

He then explained in brief the theory of Prof. Hering, of Prague, according to which there are supposed to be in the retina three chemical substances, which are called the *black-white*, the *red-green*, and the *blue-yellow*. These are acted on by light, by assimilation, and by dissimilation. Dissimilation (D) of the black-white substance produces white, its assimilation (A) black. The D-action on the red-green produces red, the A-action green. The D-action on the blue-yellow substance produces blue, the A-action yellow. When one of the substances is lacking there is an inability to properly perceive the pair of colors peculiar to it. There is therefore red-green blindness, and blue-yellow blindness. The objections to this theory as advanced by Prof. Donders and others were then brought forward.

There are two strong objections to both these theories aside from those mentioned, first, their want of simplicity, and second, the necessity of inventing new tissues and novel reactions of tissues to the affecting agent.

The true theory of colors, when found, we have every right to expect will be simple, and the laws governing it will be in keeping with the action of light on simple substances, and in the opinion of the speaker, they would be found to lie in the direction of the recent discoveries of the action of light on the molecular structure of homogeneous substances, and he accepted as the foundation of his speculations that *variation in sensation would have its basis, not in complexity of tissue, but in the varying action of the affecting agent.*

A theory on this basis would have the retina a substance whose molecular structure would be such as to allow it to respond promptly to each of those undulations of the ether corresponding to the principal colors. The wave length corresponding to red, for example, would produce a molecular change (most probably simply vibratory) which would be carried to the brain centre of vision by the optic nerve, and there transformed into a distinct sensation. The same would hold good probably for the orange, yellow, green, blue and violet. We have an analogy for such reaction in the molecular change produced by light in the metal selenium when in a crystallized state, and in some other substances. The photophone depends for its existence upon this delicate reaction of the molecular structure of selenium to the influence of light. Which are the primary and which the secondary colors—that is those arising from mixed sensations—would have to be determined by experiment.

The speaker would divide color blindness into two classes, *peripheral* and *central*. In the former the retina and optic nerve would be the agents affected, in the latter the cerebral centre of vision. The latter he considered to be the most common form of congenital color blindness, and it was due in his opinion to the fact that this centre had not yet developed the power of properly differentiating the closely allied impressions sent to it. In such cases, the spectrum was not shortened, but was seen dichromic, the line of demarcation being usually at the blue.

As regards the *retinal* form one broad general principle might be laid down, that where there was a lacking color the molecular changes in the retina were such as to incapacitate it from responding promptly to the wave lengths which physically represent that color.

Believing that education had much to do with the development of the color-sense, the speaker had devised a plan for the "systematic education of the color-sense in children," which, if followed out closely, would, he believed, in the course of generations, make color-blindness as rare in the male sex as it now is among females. This plan is published in full in the Archives of Ophthalmology. (G. P. Putnam's Sons, New York, October, 1879.)

The next communication was by Mr. E. M. Gallaudet, entitled—

THE INTERNATIONAL CONVENTION OF THE TEACHERS OF THE DEAF AND DUMB, AT MILAN.

Mr. Gallaudet recited first certain resolutions adopted at that convention, which were as follows:

"The convention, considering the incontestable superiority of speech over signs, 1st, for restoring deaf-mutes to social life, 2d, for giving them greater facility of language, declares that the method of articulation should have the preference over that of signs in the instruction and education of the deaf and dumb.

"Considering that the simultaneous use of signs and speech has the disadvantage of injuring speech and lip reading and precision of ideas, the convention declares that the pure oral method ought to be preferred."

Apropos to these resolutions, Mr. GALLAUDET quoted the comments of the London *Times*, which journal remarks that—

"No more representative body could have been collected than that which at Milan has declared for oral teaching for the deaf and dumb, and for nothing but oral teaching," and also speaks of the action of the convention as expressing a "virtual unanimity of preference for oral teaching, which might seem to overbear all possibility of opposition."

Mr. GALLAUDET then proceeded to explain the composition of the convention, which, he stated, consisted of 164 members, of whom eighty-seven were Italians and fifty-six French, these two nationalities composing seven-eighths of its representation. There were from America five members, while the city of Milan alone furnished forty-six. The president and secretary, both oralists, were from Milan, and seven out of eight other officers were also oralists. The Paris convention, in 1878, had been organized by the Pereire Society, an active propaganda in favor of the exclusive oral method; and the organization of the Milan convention was of a similar nature, and cannot be regarded as representative of the general body of instructors of the deaf and dumb throughout the world, as the preceding statement of its composition must indicate. The American delegates voted in favor of the combined method of teaching, both orally and by signs.

He expressed, in closing, the conviction that teachers of this country are working in the right direction, and that, in due time, the relative importance as well as the proper sphere of the two methods will be fully recognized in the combined system.

191ST MEETING. JANUARY 8, 1881.

Vice-President TAYLOR in the Chair.

Twenty-seven members present.

The minutes of the last meeting were read and adopted.

A communication by Mr. W. F. McK. Ritter was then read, entitled—

ON A SIMPLE METHOD OF DERIVING SOME EQUATIONS USED IN THE THEORY OF THE MOON AND OF THE PLANETS.

The rectangular and polar co-ordinates of a heavenly body are functions of the elements of the orbit and of the time. When the elements are pure constants, as in the case of undisturbed motion, these co-ordinates vary only with the time; but when the effect of the disturbing force is considered, we have variation or perturbation of the elements, and hence, also, the co-ordinates vary both with the time and the elements.

Since the co-ordinates are functions of the elements, as long as the variations of the elements are unknown, the corresponding corrections to the co-ordinates, due to these variations, must be regarded as zero. Hence, in the differentiation, the differentials of the co-ordinates with respect to the elements, alone considered as variable, must be put equal to zero. Hence, also, the velocities of the rectangular and polar co-ordinates are zero, and thus we are furnished with equations of condition, which greatly facilitate the solution of the problem of determining the perturbations of the elements.

In finding what are called the special perturbations, we resolve the disturbing force into three components.

For this purpose, call

R, the component in the direction of the radius-vector,

S, the component perpendicular to the radius-vector, parallel to the plane of the orbit, and positive in the direction of the motion, and

Z, the component perpendicular to the plane of the orbit.

The values of these components, in the form we wish to employ, are

$$R = k^2 (1 + m) \frac{d\Omega}{dr},$$

$$S = k^2 (1 + m) \frac{1}{r} \frac{d\Omega}{dv},$$

$$Z = k^2 (1 + m) \frac{d\Omega}{dz}.$$

Here Ω is the disturbing function, r and v are polar co-ordinates, z the co-ordinate perpendicular to the plane of the orbit, k^2 the

Gaussian constant, and m the relation of the mass of the disturbed body to that of the sun.

By putting the first differential co-efficients of the co-ordinates with respect to the time equal to zero, we derive, with great ease, the expressions for the variations of the elements. This is for the case of special perturbations. These expressions will contain the components R, S, and Z.

If we now substitute the values of these components, wherever they appear, and perform the necessary reductions, we get expressions for the variations of the elements, where, instead of the components of the disturbing force, the force itself appears.

In the case of the mean anomaly, another method has been followed. Its variation can best be found by means of the relation

$$M = \mu\,(t - T),$$

where M represents the mean anomaly, μ the mean daily motion, and T the time of perihelion-passage.

I have thus derived, among others, the equations:

$$\frac{dL}{dt} = k^2(1+m)\frac{d\Omega}{dM}, \quad \frac{dM}{dt} = -k^2(1+m)\frac{d\Omega}{dL}$$

$$\frac{dG}{dt} = k^2(1+m)\frac{d\Omega}{d\omega}, \quad \frac{d\omega}{dt} = -k^2(1+m)\frac{d\Omega}{dG},$$

$$\frac{dH}{dt} = k^2(1+m)\frac{d\Omega}{d\Omega}, \quad \frac{d\Omega}{dt} = -k^2(1+m)\frac{d\Omega}{dH}.$$

From these, by slight changes, we get the equations used by Delaunay in his theory of the moon's motion. Thus by putting $k^2(1+m)\,\Omega = R$, and writing l, g, h, for M, ω, Ω, respectively, we have

$$\frac{dL}{dt} = \frac{dR}{dl}, \quad \frac{dl}{dt} = -\frac{dR}{dL},$$

$$\frac{dG}{dt} = \frac{dR}{dg}, \quad \frac{dg}{dt} = -\frac{dR}{dG},$$

$$\frac{dH}{dt} = \frac{dR}{dh}; \quad \frac{dh}{dt} = -\frac{dR}{dH}.$$

In these equations, according to the notation of Delaunay, $L = \sqrt{a\mu}$, μ being the sum of the masses of the earth and moon, $G = L\sqrt{1-e^2}$, $H = G\cos i$; a, e, and i being the semi-major axis, eccentricity, and inclination respectively; l designates the mean anomaly, g the angular distance of the ascending node from the perigee, and h the longitude of the ascending node.

The equations which Le Verrier uses in his theories of the planets are not as simple in form as those of Delaunay; but there is no difficulty attending their derivation by this method. The method Le Verrier uses in deriving them is long and cumbrous. Delaunay does not stop to derive the equations he uses, but refers, on this head, to a memoir by Benét.

By the method given above I have derived all the fundamental equations used by these authors, and by those who have considered the subject of perturbations from the same standpoint.

I think I have here given enough of the process to enable any one to understand the method. I may add that the method occurred to me seven or eight years ago.

The next communication was by Mr. EDGAR FRISBY

ON THE ORBIT OF SWIFT'S COMET.

This comet was first observed by Prof. Swift of Rochester, October 10, 1880, and was reported by him as moving directly towards the earth. It was observed by Prof. Eastman with the transit circle of the U. S. Naval Observatory on the evenings of October 25, November 7, and November 20, and from the data so obtained the following elements were computed by Prof. Frisby:

Epoch of perihelion passage $7^d.775675$ Washington mean time

$$\left.\begin{aligned} \Omega &= 296^\circ\ 48'\ 19.''9 \\ \pi &= 42^\circ\ 59'\ 15.''8 \\ \vartheta &= 42^\circ\ 26'\ 48.''5 \\ \imath &= 5^\circ\ 30'\ 35.''9 \\ \log a &= 0.517002 \\ \log \mu &= 2.774504 \end{aligned}\right\} \text{Mean Equinox 1880.0.}$$

From these elements it will be inferred that it was moving very nearly towards the earth at the time of discovery, October 10. On November 8, it came very near the earth's orbit, its distance from it then being about 0.069 of the earth's mean distance from the sun. The aphelion lies just beyond Jupiter's orbit so that its perturbations are liable at any time to become immense. The periodic time from the elements is about 2,178 days, or a little less than six years, but Jupiter's position in his orbit is now such that it is not likely to come near the comet for a long period. For a time after the discovery of the comet it was doubtful whether the period was 11 or 5½ years. The latter is undoubtedly the true one, the slight discrepancy being due to insufficient data. It would probably be impossible to see it at every return, for assuming its period to be approximately $5\frac{1}{2}$ years, the earth would at each alternate return be at the opposite side of its orbit, and the sun would then intervene between the earth and the comet. It passed nearest to the earth about the 18th of November.

The logarithms of the radii vectors and distance from the earth on the dates given are:

	log. r	log. Δ
October 25,	0.035328	9.221510
November 7,	0.029018	9.141693
November 20,	0.034557	9.119295

No theory about any periodic time was assumed in these calculations.

At the conclusion of Mr. Frisby's paper the Society adjourned.

192D MEETING. JANUARY 22, 1881.

The President in the Chair.

Thirty-seven members present.

The following communication was read by Mr. J. W. CHICKERING, entitled—

NOTES ON ROAN MOUNTAIN, NORTH CAROLINA.

The great Appalachian chain, with its undulating line of 1,300 miles, from the promontory of Gaspè, on the Gulf of St. Lawrence, to Georgia and Alabama, beginning as a series of simple folds of moderate height, increases in complexity as in altitude from north to south, attaining its greatest elevation in a veritable mountain knot in the Black range. Following it from its commencement to the Hudson, we find the single chain of the Green Mountains, rising to its extreme height in Mount Mansfield, 4,430 feet, with, on the east, the outlying clusters of the White Mountains in New Hampshire, with Mount Washington reaching 6,288 feet, and others exceeding 5,000 feet, and Mount Katahdin in Maine, 100 miles away, about 5,200 feet, and on the west the Adirondack group, rising to 5,379 feet, and the Catskills considerably lower.

From the Hudson to the New River in Virginia, 450 miles, through the States of New Jersey, Pennsylvania, and Virginia, it gradually gains in both width and altitude, consisting of many parallel ranges, with fertile valleys between, of which the great valley of Virginia is the largest and best known. In Pennsylvania the summits vary from 800 to 2,500 feet. Toward the south the chains become more numerous and in Virginia the Peaks of Otter reach 4,000 feet. The extreme eastern range is called the Blue Ridge, the extreme western the Cumberland Mountains, or, more properly, Plateaus, while the high range or ranges between are, in general, called the Alleghanies.

From the New River south the system becomes much more complex. The main chain, hitherto called the Blue Ridge, is deflected to the west, and for 250 to 300 miles, in a circuitous chain, under the names of Iron, Stone, Bald, Great Smoky, and Unaka Mountains, forms the boundary line between North Carolina and Tennessee, rising frequently to heights exceeding 6,000 feet; while the more easterly range, retaining the name of Blue Ridge, and finding its southern terminus at Cæsar's Head, in South Carolina, where it turns abruptly to the northwest, reaches even loftier altitudes, Mitchell's high peak being accredited with 6,717 feet.

In North Carolina these two ranges are more than 50 miles apart, are partially connected by transverse ranges, and, for more than 100 miles, constitute a great central plateau, like that of Colorado on a small scale.

As says Prof. Guyot, "Here then through an extent of more than 150 miles the mean height of the valley from which the mountains rise is more than 2,000 feet. The mountains which reach 6,000 feet are counted by scores, and the loftiest peaks exceed 6,700 feet, while at the north, in the group of the White Mountains, the base is scarcely 1,000 feet, the gaps 2,000 feet, and Mount Washington, the only one which rises above 6,000 feet, is still 400 feet below the Black Dome of the Black Mountains. Here then, in all respects, is the culminating region of the vast Appalachian system."

The eastern chain, or Blue Ridge is still the watershed, and, on the Atlantic slope, gives birth to the Roanoke, Catawba, Broad, Saluda, and Savannah rivers; while on the other side this area of mountains and plateaus is separated by transverse chains into many deep basins, at the bottom of each one of which runs one of those mountain streams, which are compelled to cut their way to the Tennessee through gaps, gorges, and defiles in the very heart of this mighty chain, giving us some of the most picturesque scenery to be found on the continent. Among these, the New, Watauga, Nolichucky, and French Broad are the best known.

In the midst of this region, with all three ranges in sight, stands Roan Mountain, Laurentian in age, the State line crossing it at an altitude of 6,391 feet, as determined by the mean of my barometrical observations—and on and about this mountain it was my good fortune to stay from June 25th to August 30th.

Notes upon some of the peculiarities of the region, as contrasted with the northern Appalachian, will be my apology for asking your attention.

I. The Uniformity of Elevation.

Standing on the summit of Roan, we look into seven different States, and command a horizon of 30 to 80 miles. On the north and west the eye catches the Cumberland range in the horizon, beyond the great Tennessee plateau, which is traversed by the Clinch and a score of other ranges, but all as level as if designed for railroad embankments.

On the south and east there is a wilderness of mountains. Guyot gives 50 to 60 with altitudes exceeding 6,000 feet, and yet the highest is only 6,717 feet, and perhaps 40 of them fall between 6,000 and 6,500, while hundreds of others are above 5,000. The valleys rarely go below 3,000 feet. The railroad after leaving Lynchburg reaches 1,000 feet in a few miles, and from that point for nearly 300 miles never goes below 1,500 feet, its highest summit being at 2,550 feet.

II. Uniformity of Temperature.

During nine weeks the mercury once indicated 75°, seven times 70° +, once 45°, three times 50°, the general daily variation being between 55° and 65°. The spring, a few rods rods from the hotel, has a temperature of 45°. Equally remarkable was the uniformity of atmospheric pressure the highest barometer being 24.19, and the lowest 23.87, or a difference of only 0.32 inches. No wind had a velocity of more than twenty miles an hour, and seldom did it reach ten.

III. Fertility of the Summit.

Instead of the upper 1,000 feet being, as in most of the northern Appalachian peaks reaching an altitude of over 5,000 feet, a pile of barren rocks, with lichens their only vegetation, the summit of Roan, and many other peaks, is a smooth, grassy slope, of the most vivid green, dotted with clumps of *Alnus viridis*, and *Rhododendron catawbiense*, the soil one or two feet in depth, rich and black. How this amount of humus was accumulated on these summits, and what cause destroyed the forests which its existence would seem to indicate as formerly existing, are questions not easily answered.

The valleys are very fertile, and adapted to almost any crop.

At an elevation of 3,000 to 4,000 feet occurs a belt of the most magnificent forest trees I have ever seen—hundreds of chestnuts, sugar maples, lindens, tulip trees, yellow birches, buck-eyes—some from 4 to 7 feet in diameter, and rising 70 to 80 feet without a limb. One chestnut measured 24 feet in circumference, and one black cherry measured 19 feet. Thorn bushes are as large as old apple trees with dwarf buck-eyes and yellow birches, looked like old orchards of vast extent.

IV. Flora.

Ascending the mountain, the vegetation takes on a northern aspect. Hemlocks abound till near the summit, where they are replaced by *Abies Fraseri*, the characteristic species of these summits.

Anemone nemorosa, Oxalis acetosella, Rubus odoratus, Ribes lacustre and *prostratum, Aster acuminatus, Habenaria articulata, Veratrum viride, Lycopodium lucidulum*, and similar species, remind one of the woods of Maine or New Hampshire.

The peculiar flora of the upper 1,000 feet, greatly resembles in habit that of the White Mountains, but very few species are the same. *Paronychia argyrocoma, Lycopodium selago* and *Alnus viridis*, are almost the only plants that occur to me as identical in the two localities, and these in the White Mountains are found in Crawford Notch, while in Roan they are near the summit. *Arenaria grœnlandica* is replaced by *A. glabra, Solidago thyrsoidea* by *S. glomerata; Geum radiatum* of the North is a variety of that found here; the two dwarf *Nabali* of White Mountains are represented by a new species, *N. roanensis, Rhododendron lapponicum* (four inches high) by magnificent *R. catawbiense*, covering the summit with its domes of inflorescence six to eight feet in diameter, *Castilleia pallida* by *C. coccinea*.

So that, in general, the species peculiar to these mountains are hardly sub-alpine, and thus continuous with similar species further north, but are rather apparent instances of local variation, many species being confined to very limited localities.

On Mount Washington, a few rods will often give the same plant in bud, flower, and fruit, as a north or south exposure, a precipice, or a snow-drift may retard or accelerate growth; but on these southern mountains no such difference obtains any more than in the valleys below.

On this communication Mr. J. W. POWELL remarked that the uniformity in the altitudes of the peaks is a feature resulting from the fact that the general mass out of which they have been carved by erosion possesses a plateau structure. The elevation of that region was distributed in its effects with an approach to uniformity over a wide extent of country, and was unaccompanied by those sharp flexings or the protrusions of abrupt mountain cores, which are encountered in some portions of the Appalachians and other mountainous regions. The individual masses and ranges in the Cumberland region are the work of erosion—the general process of land sculpture acting upon a broad platform, excavating broad valleys and narrow gorges, and leaving the peaks and ridges as cameos—mere remnants left in the general degradation of the whole region. Prof. Powell exemplified the process by citing the Uinta Mountains as a broad platform similarly carved by an extensive erosion.

The following paper was read by LESTER F. WARD, entitled—

FIELD AND CLOSET NOTES ON THE FLORA OF WASHINGTON AND VICINITY.

[Abstract.*]

Introductory Remarks.

This paper has resulted from a suggestion made to the writer in the spring of 1880, by a member of the Committee on Publications of this Society, relative to the need that exists for some special

*Mr. Ward's communication presented to the Society only a brief notice of the principal points of a monograph which he had prepared upon the flora of the District of Columbia. In view of the local character of his subject, and of the thorough and commendable manner in which it had been elaborated, the Committee on Communications recommended, and the General Committee authorized, the printing of a very full and copious abstract of the paper, which is given herewith.

treatise on the flora of this vicinity, and for a new and revised catalogue of the plants. While there now exists a provisional catalogue containing most of the species which have been collected or observed by botanists during the past six or seven years, it consists of so many small annual accretions, due to constant new discoveries, and contains withal so many blemishes and imperfections, incident to its hasty compilation and irregular growth, that it has ceased, in great part, to meet the demands of the present time. The elaboration of a systematic catalogue of the local flora was not, however, at the outset at all contemplated, but merely the presentation of certain notes and special observations on particular species, which had been made in the course of some nine years of pretty close attention to the vegetation, and somewhat varied and exhaustive field studies in this locality.

The flowering-time of most species here is much earlier than that given in the manuals, and is, moreover, in many cases, very peculiar and anomalous, rendering it important to collectors as well as interesting to botanists to have it definitely stated for a large proportion of the plants. It being thus necessary to extend the enumeration so far, it was thought that the remainder might as well be added, thus rendering it a complete catalogue of all the vascular plants known to occur here at the present time. To these has been appended the list of *musci* and *hepaticæ* prepared by the late Mr. Rudolph Oldberg for the *Flora Columbiana*, which has been left unchanged except in so far as was required to make it conform strictly to Sullivant's work which has long been the standard for this country. Dr. E. Foreman has also furnished the names of a few of the *Characeæ* collected by himself, and named by Prof. Farlow, of Cambridge, which, in the present unsettled state of the classification of the cryptogams, have, for convenience, been placed at the foot of the series.

In undertaking this compilation I have endeavored to resist the usual temptation of catalogue makers to expand their lists beyond the proportions which are strictly warranted by the concrete facts as revealed by specimens actually collected or species authentically observed; but have been content to set down only such as I can either personally vouch for, or as are vouched for by others who have something more substantial than memory to rely upon; preferring that a few species actually occuring but not yet seen should be omitted and afterwards supplied, rather than that others, supposed to exist, but which cannot be found, should stand in the catalogue to be apologized for to those who would be glad to obtain them. A few species, however, which are positively known to have once occured within our limits, but which have been obliterated within the recollection of persons now living, have been retained, as well as several of which only a single specimen has been found; but in all such cases the facts are fully stated in the notes accompanying each plant.

Range of the Local Flora.

The extent of territory which has of late years been tacitly recognized by botanists here as constituting the area of what has been called the *Flora Columbiana* is limited on the north by the Great Falls of the Potomac, and on the south by the Mount Vernon estate in Virginia, and Marshall's just opposite this on the Maryland side of the river, while it may reach back from the river as far as the divide to the east, and as far westward as the foot of the Blue Ridge, so as not to embrace any of the peculiarly mountain forms. Practically, however, the east and west range is much more restricted and only extends a few miles in either direction.

Comparison of the Flora of 1830 *with that of* 1880.

Washington and its vicinity has long been a field of botanical research. The year 1825 witnessed the dissolution of the *Washington Botanical Society*, which had for many years cultivated the science, and the same year also saw the formation of the *Botanic Club*, which continued the work, and in one respect, at least, excelled the former in usefulness, since it has handed down to us of the present generation a valuable record in the form of a catalogue of the plants then known to exist in this locality. This catalogue, which was fittingly entitled *Floræ Columbianæ Prodromus*, and claimed to exhibit "a list of all the plants which have as yet been collected," though now rare, and long out of print, is still to be found in a few botanical libraries.

I have succeeded in securing a copy of this work, and have been deeply interested in comparing the results then reached with those which we are now able to present. A few of these comparisons are well worth reproducing.

It should be premised that the *Prodromus* is arranged on the artificial system of Linnaeus, so that before the plants could be placed in juxtaposition they required to be re-arranged. This, however, was not the principal difficulty. Such extensive changes have taken place in the names of plants during the fifty years which have elapsed since that work appeared, (1830,) that it is only with the greatest difficulty that they can be identified. After much labor, I have succeeded in identifying the greater part of them, and in thus ascertaining about to what extent the two lists are in unison. This also reveals the extent to which each overlaps the other, and thus affords a sort of rude index to the changes which our flora has undergone in half a century. There are, however, as will be seen, many qualifying considerations which greatly influence these conclusions and diminish the value of the data compared.

The whole number of distinct names (species and varieties) enumerated in the *Prodromus* is 919. Of these 59 are mere synonyms or duplicate names for the same plant, leaving 860 distinct plants. I have succeeded in identifying 708 of these with certainty as among those now found, and six others, not yet clearly identified, should probably be placed in this class. This leaves 146 enumerated in the old catalogue which have not been found in recent investigations. [A classified list of these plants was presented and commented upon somewhat in detail.]

With regard to these 146 species, it must not be hastily concluded that they represent the disappearance from our flora of that number of plants. While they doubtless indicate such a movement to a certain extent, there are ample evidences that many of them can be accounted for in other ways. After careful consideration, I have been able to divide them into four principal classes arising out of—

1st. Errors on the part of those early botanists in assigning to them the wrong names.

2d. The introduction into the catalogue of adventitious and even of mere cultivated species, never belonging to the flora of the place.

3d. The undue extension by those collectors of the range of the local flora so as to make it embrace a portion of the maritime vegetation of the Lower Potomac or the Chesapeake Bay, and also the mountain flora of the Blue Ridge.

4th. The actual extermination and disappearance of indigenous plants during the fifty years that have intervened since they made their researches.

The assignment which I have made of each species to its appropriate class has been of course in great part conjectural and may be incorrect in many cases, while another botanist might have differed considerably in regard to special plants; yet it is not based on a general judgment drawn from my acquaintance with the present flora, but upon several kinds of special evidence, which in numerous instances has reversed my *prima facie* decision.

In the first place, I have carefully compared the range of each species as given in the text books to determine the probabilities for or against its being found here, and in the second place I have compared this list with the corresponding one of the species now found but not enumerated in the *Prodromus.* I have also endeavored to make due allowance on the one hand for the tendency above referred to to swell catalogues beyond their proper limits, and on the other for the well known fact that every flora is at all times undergoing changes.

It must not be forgotten, either, that half a century ago the surface of the entire country here must have presented a very different appearance from that which it presents now. The population of the District of Columbia in 1830, when it included a portion of Virginia, was only 39,834. It is now, exclusive of the Virginian part receded to that State, 177,638. To render the comparison more exact we may add to the latter number the present population of Alexandria county, amounting to 17,545, and we have in the place of 39,834 a population on substantially the same area of 195,183, or about five times as large. The population of Maryland in 1830 was 447,040; in 1880 it was 934,632, or considerably more than twice as large. That of Virginia in 1830 was 1,211,405. Virginia and West Virginia, embracing the same territory, now number 2,131,249 the population not having quite doubled: the retardation, however, as compared with Maryland, is doubtless due entirely to influences affecting the southern counties. There were doubtless large areas of primeval forest then within our limits which are now under cultivation, and a much greater variety of soil and woodland was then open to the researches of the botanist. As a consequence we ought to expect that it would sustain a much richer flora.

The general result at which I arrive by the process adopted may be summed up as follows:

1st. That 43 of these names, or 29 per cent. of them, belong to the first class and constitute errors in naming.

2d. That 12 of these plants, or 8 per cent., belong to the second class, or were simply cultivated species, and never belonged to this flora.

3d. That 10 of them, or 7 per cent., belong to the third class and were collected beyond the reasonable limits of our local flora.

4th. The remaining 81, or 56 per cent., belong to the fourth class, and represent *bona fide* discoveries in 1830 of species which either do not now occur or are so rare as to have escaped the investigations of the present generation of botanists.

With regard to the first of these classes, the large number of errors in naming cannot be considered any derogation from the ability or fidelity of the compilers of the *Prodromus* or their immediate predecessors, when we remember the very unsettled state that American botany was in at that time. Both names and authorities were badly confused, and errors were committed even by the most experienced botanists. For example, their *Corydalis glauca* as probably also their *C. aurea*, meant *C. flavula* which is now abundant, but omitted by them. Their *Arabis stricta* might have been *A. hirsuta* or *A. patens*, which are both now rare, though it was more probably a form of *A. laevigata*, as they seemed to be specially fond of drawing nice distinctions and expressing them by synonyms. Varieties, however, were scarcely recognized by them, the trinomial theory being then in its infancy. I might thus proceed to discuss all their supposed errors, but this is not necessary.

The second and third classes, amounting together to 16 per cent. of the alleged excess over the present flora, consist also of errors, but errors which it is much less easy to palliate. It is natural to wish to make as large a showing as possible, and the temptation to insert into a catalogue everything which by any construction can be claimed to belong there is rarely resisted. To show that this propensity still exists, it may be remarked that of the 1054 species enumerated in the preliminary catalogue of plants of this vicinity, published by the Potomac Side Naturalist's Club in 1876, 89, or about 8½ per cent. are now admitted by all not to have been seen here at that time, and have never been found by any one since, although nearly three hundred other species have since been added to the flora. This is certainly not a scientific method to proceed upon, and as already remarked, the present effort aims to eliminate to a great extent this source of error.

The 81 species constituting the fourth class remain, therefore, the only ones to which any special interest attaches and for the determination of which the present somewhat laborious analysis of this ancient document has been undertaken. For these, the botanists of our times should make diligent search and perchance a few of them may still be found. Assuming that they no longer exist, they do not represent the whole number of plants that have disappeared from our flora during the interval of fifty years. This could be only on the assumption that the *Prodromus* was a complete record of the flora at the time. This it certainly is not. The aggregate number, exclusive of synonyms or duplicated names, which it contained was, as we saw, 860, which includes one cellular plant, viz: *Achara*. We now identify, counting as was then done, species and varieties, 1249 distinct vascular plants. While no doubt many of these have been freshly appearing while others have been disappearing, still, from the considerations above set forth, it is highly probable that the indigenous flora of 1830 was considerably larger than that of 1880, and may have reached 1400 or 1500 vascular plants. It would appear, therefore, that only a little over half the plants actually existing were discovered by the botanists of that day, and enumerated in their catalogue. If the proportion of disappearances could be assumed to be the same for species not discovered as for those discovered by them, this would raise the aggregate number to considerably above one hundred, and perhaps to one hundred and twenty-five.

The great number of present known species not enumerated in the *Prodromus*, some of them among our commonest plants and amounting in the aggregate to 535 species, is another point of interest, since, after due allowance has been made for mistakes in naming them, it remains clear on the one hand that these researches must have been, compared with recent ones, very superficial; and on the other, that, not to speak of fresh introductions, many plants now common must have then been very rare, otherwise they would have proved too obtrusive to be thus overlooked.

Localities of Special Interest to the Botanist.

The flora of a wild region is always more uniform than that of one long subjected to human influences. The diversity in the former is a natural consequence of the corresponding diversity in the surface and other physical features. In the latter it is due to conditions arbitrarily imposed by man. A primeval flora is usually more rich in indigenous species, but the artificial changes caused by cultivation often offset this to a great extent by the introduction of foreign ones. This, however, greatly reduces its botanical interest.

In many respects the botanist looks at the world from a point of view precisely the reverse of that of other people. Rich fields of corn are to him waste lands; cities are his abhorrence, and great areas under high cultivation he calls "poor country;" while on the other hand the impenetrable forest delights his gaze, the rocky cliff charms him, thin-soiled barrens, boggy fens, and unreclaimable swamps and morasses are for him the finest lands in a State. He takes no delight in the "march of civilization;" the ax and the plow are to him symbols of barbarism, and the reclaiming of waste lands and opening up of his favorite haunts to cultivation he instinctively denounces as acts of vandalism. In him more than in any other class of mankind the poet's injunction—

"Woodman, spare that tree,"

touches a responsive cord. While all this may seem as absurd to some as does the withholding from tillage of great pleasure grounds in the form of hunting parks for the landed sporting gentry of Northern and Western Europe, still, when these parts of the world are compared with the artificially made deserts of Southeastern Europe and Western Asia, caused by the absence of such sentiments, there may, perhaps, be dimly recognized a "soul of good in things evil," if not a soul of wisdom in things ridiculous.

After the protracted subjection of a country to the conditions of civilization it gradually comes about that while the greater part of the surface falls under cultivation, more or less thorough, and the botanist is ultimately excluded from it, there will remain a few favored spots, which, from one cause or another, will escape and continue to form his favorite haunts. In the vicinity of large rivers, giving greater variety to the surface, or of rugged hills or mountains, this will be especially the case. As a country grows old large estates in the vicinity of cities fall into the possession of heirs who are engaged in mercantile or professional business, and neglect them, or they come into litigation lasting for years, and are thus happily abandoned to nature. These and other causes have operated in an especial manner in the surroundings of Washington, and there thus exist a large number of these green oases, as it were, interspersed over the otherwise botanical desert.

In consequence of this fact it requires experience in order to improve the facilities which the place affords. A botanist unacquainted with the proper localities for successful collection might spend a month almost in vain, and depart with the conviction that there was nothing here to be found. It may not be wholly peculiar, but these favored localities are here often of very limited extent, and in situations which from a distance afford no attraction to the collector. Civilization is, however, very perceptibly encroaching upon many of them, and it is feared that in another half century little will be left but a few bare rocks or inaccessible marshes.

In naming localities the principal authorities relied upon are: 1. A recent *Atlas of fifteen miles around Washington, including the County of Montgomery, Md., Compiled, Drawn, and Published from Actual Surveys, by G. M. Hopkins, C. E:* Philadelphia, 1879; and, 2, a military map of Northeastern Virginia, published in the work of General J. G. Barnard, on the *Defences of Washington,* 1821.

From the former the names of many roads, streams, estates, &c., have been obtained, while from the latter those of forts, batteries, &c., are often employed as more convenient. In this respect, however, much remains to be desired. While the military map is antiquated, the other is frequently defective in omitting what is required and incorrect in erroneously locating streams and other objects well known to the writer. In his extensive rambles he has learned many local names not found on the map, and in a few cases of special botanical interest, where names are wholly wanting, he has long been in the habit of designating the localities by names of his own christening, and for which he offers no apology.

The following are a few of the principal places of botanical interest which will be found to recur most frequently in the notes, and for this reason brief descriptions of them are appended.

1. *The Rock Creek Region.*—Rock Creek which forms the boundary line between Washington and Georgetown (West Washington), has escaped to a remarkable degree the inroads of agriculture and population. For the greater part of its length within the District of Columbia its banks are still finely wooded for some distance back, and afford a rich and varied field for botanical exploration. The character of the surface along Rock Creek is most beautiful and picturesque, often rocky and hilly with frequent deep ravines

coming down into the usually narrow bottom through which the creek flows. The stream itself is full of the most charming curves and the whole region is an ideal park. No one can see it without thinking how admirably it is adapted for a National Park. Such a park might be made to extend from Oak Hill Cemetery to the Military Road opposite Brightwood, having a width of a mile or a mile and a half. Not only every botanist but every lover of Art and Nature must sigh at the prospect, now not far distant, of beholding this region devastated by the ax and the plow. The citizens of Washington should speedily unite and strenuously urge upon Congress the importance of early rescuing this ready-made National Park from such on unfortunate fate.*

The Rock Creek Region is divided, so far as the designation of localities is concerned, into six sections. The first embracing the series of groves from Georgetown to Woodley Park on the right bank of the creek, is called Woodley. This section embraces several interesting ravines and in it are found many plants rare elsewhere, such as *Chamæ lirium, Carolinianum, Cypripedium pubescens, Hesperis matronalis* and *Liparis Lœselii.* In it is also a grove of the Hercules club (*Aralia spinosa.*) On the left bank of the creek lie the Kalorama Heights and some open woodland.

The *Woodley Park* section extends to the ravine which comes down opposite the old brick mill-ruin known as the Adams Mill. The timber here has been thinned out recently by the proprietors but not cleared off, and the vegetation has undergone a marked change. Several interesting plants have been found in Woodley Park, including the rare *Obolaria Virginica,* and the beautiful *Spiraea aruncus.* Above this the timber is heaviest on the left bank and some very fine ravines occur, at the head of one of which is a magnolia and sphagnum swamp where *Veratrum viride* and *Symplocarpus fœtidus* keep company with *Gonolibus obliquus* and *Pyrus*

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* It is remarkable that when committees of Congress have been appointed, as has several times been done, to consider a site for a National Park, they have usually looked in other directions and have seemed to ignore the existence of this region, which is certainly the only one that possesses any natural claims. A mere carriage ride through such parts as are traversed by roads is wholly insufficient to afford an adequate idea of its merits from this point of view. For the greater part of the distance mentioned above this region is accessible only to footmen.

arbutifolia. Here, too, though well up towards the ford, has been found *Polemonium reptans*, not seen elsewhere.

This third section terminates at Piney Branch, and from here to Pierce's mill, and as far above as the mouth of Brood Branch, the fourth section extends. This section is well wooded on both sides and includes the enchanting Cascade run which leaps down over the most romantic rocks. Near Pierce's mill are many trees and shrubs, planted there years before, but now well naturalized. Among these are *Aralia spinosa*, *Xanthoxylum Americanum*, *Acer saccharinum*, *Pinus strobus*, and *Carya alba.* Below the mill on the creek bottom is a long-abandoned nursery of *Populus alba* and *Acer dasycarpum*, from which many of the trees of the city may have been supplied.

From Broad branch to the Military road is the fifth and perhaps most interesting section of the Rock Creek Region. On the left bank lie the once noted Crystal Springs, and though the buildings are removed, the springs remain unchanged. Here have been found *Ophioglossum vulgatum*, *Anychia dichotoma*, and *Perilla ocimoides*, as well as *Tipularia discolor.* On the right bank and above Blagden's mill is a bold bluff in a short bend of the creek forming a sort of promontory upon which there grows *Gaultheria procumbens*, the winter-green or checkerberry, this being its only known locality within our limits. Half a mile farther up and back upon the wooded slope is the spot on which stand a dozen or more fine trees of the Table Mountain Pine, (*P. pungens.*) Here also was first found *Pycnanthenum Torreyi.*

To these there must be added a sixth section extending from the Brightwood road to the north corner of the District of Columbia which lies near Rock Creek. For the first mile there is little of interest, the cultivated land approaching the creek and the low hills near its banks being covered with a short second growth of scrub pine and black-jack. But above the Claggett estate on the right bank, and to some extent on both sides, lies the largest forest within our limits. This wood belongs, I learn, to the Carroll estate and is so designated in this catalogue. In it have been found very many most interesting plants. It was the first extensive tract found for the crowfoot (*Lycopodium complanatum*) and still constitutes the most reliable and abundant source known of this plant. Its present fame, however, rests upon its hybrid oaks, of which some most interesting forms have been found there. [See Field and Forest, October and November, 1875; Botanical Gazette, October, 1880, p. 123.] Here also grows very sparingly *Microstylis ophioglossoides*, and quite abundantly *Pyrola elliptica* and *P. secunda*. It is also a rich locality for many other species rare elsewhere.

2. *The Upper Potomac Region.*—The flora of the left bank of the Potomac is, in many respects, very unlike that of any other locality within our limits. A mile above Georgetown, and commencing from the recently constructed outlet lock of the Chesapeake and Ohio canal, there exists a broad and low strip of country formerly known by the name of Carberry Meadows, lying between the canal and the river, and extending to the feeder of the canal, a distance of about three and a half miles. This interval is relieved by two convenient landmarks, viz., one mile above the outlet lock, a grist-mill and guano factory, popularly known as Eads' mill; and a mile further, the celebrated Chain Bridge. Little Falls, proper, begin a hundred yards above the bridge, and extend half a mile or more. The region above the bridge will, therefore, be designated as Little Falls. The flats terminate in a remarkable knoll or small hillock of very regular outline and abrupt sides, which, from the combined effects of the feeder on one side, and large overflows from it below, becomes practically an island, and is well known to all as High Island. These river flats are, in most places, covered with large boulders of the characteristic gneiss rock of the country. In some parts the surface is very rough, and numerous pools or small ponds of water occur. Overflows and leakages from the canal cause large sloughs and quagmires, while annual ice-gorges crush down the aspiring fruticose vegetation. All these circumstances lend variety to the locality, and, as might be expected, the flora partakes largely of this characteristic. It would prolong this sketch unduly to enumerate all the rare and interesting plants which this region has contributed to our vegetable treasures, but conspicuous among them are *Polygonum amphibium*, var. *terrestre*, *Isanthus cœruleus*, *Herpestis nigrescens*, *Brasenia peltata*, *Cyperus virens*, and *Nesæa verticillata*, all of which recur below Ead's mill; *Ammannia humilis*, a remarkable variety of *Salix nigra*, (*S. nigra* var. *Wardi*, *Bebb.*) *Salix cordata*, and *S. longifolia*; as also *Spiranthes latifolia*, and *Samolus valerandi* var. *Americanus*, *Vitis vulpina* and *Panicum pauciflorum*, which may be found between this point and the bridge, while at the Little Falls we are favored with *Paronychia dichotoma*, *Œnothera fruticosa*, var. *lineare*

(very distinct from the type) and *Ceonothus ovatus*: also *Ranunculus pusillus* and *Utricularia gibba*. But rich and varied as are these lower flats, they are excelled by High Island, the flora of which is by far the most exuberant of all within the knowledge of botanists. Here we find *Jeffersonia diphylla, Caulophyllum thalictroides, Erigenia bulbosa, Silene nivea, Valeriana pauciflora, Erythronium albidum, Iris cristata,* and a great number of others of our most highly prized plants, many of which are found nowhere else.

Above the feeder is a series of islands in the river lying for the most part near the Maryland shore, and to which the maps, so far as I can learn, assign no names. The first of these lies well out in the river, and has been made to form a part of the feeder-dam. It is low and frequently overflowed, and has not, as yet, furnished many rare plants, though here *Arabis dentata* and some others have been found. It has been designated *Feeder-dam Island*. The second is half or three-quarters of a mile above, lies higher, and is covered with a very dense and luxuriant herbaceous vegetation and fine trees, chiefly of Box Elder, *Negundo aceroides*, from which circumstance and the peculiar impression which the long gracefully pendent staminate flower of these trees produced on the occasion of its first discovery by a botanical party it received the name of *Box Elder Island*. The third island is a short distance above the last, has a more elevated central portion and a similar vegetation. Here was found, on our first visit, and also on subsequent ones, *Delphinium tricorne*, and for this contribution to the Flora Columbiana it was christened *Larkspur Island*. The fourth of these islands is, in many respects, similar to the two last described, and upon it stands the only indigenous specimen of *Acer saccharinum* yet found here. It has, therefore, been appropriately named *Sugar-maple Island*. *Erythronium albidum, Trillium sessile, Jeffersonia diphylla* and similar species abound on all these islands, while on the Larkspur Island, besides the *Delphinium*, has also been found *Phacelia Purshii*. The beauty of these natural flower-gardens in the months of April and May is unequaled in my experience. The light and rich alluvial soil causes the vegetation to shoot up with magic rapidity at the first genial rays of the vernal sun, and often the harbinger of spring, *Erigenia bulbosa*, true to its name, will greet the delighted rambler in late February or early March.

The opposite, or Virginia side of the Upper Potomac, consists entirely of bold bluffs, interrupted by deep ravines, often containing wild torrents and dashing cascades. Here the flora, though less rich and varied, is also characteristic and interesting, and embraces, among other rare things, *Rhododendron maximum, Iris cristata, Scutellaria saxatilis, Pycnanthemum Torreyi, Solidago rupestris* and *S. virga-aurea*, var. *humilis.* On the Maryland side and a mile above the uppermost point thus far mentioned, is the Cabin John run, which the botanist celebrates more for its walking fern (*Camptosorus rhizophyllus*) than for the world-renowned arch that spans it.

The next most prolific source of interesting plants is the region of the Great Falls. The collecting grounds begin a mile or more below at Broad Water. On both sides of the canal the country is excellent, rocky and wooded, with stagnant pools and sandy hillocks. On these rocks grow *Sedum telephoides* and near Sandy Landing are found *Vitis vulpina, Arabis patens, A. hirsuta* and *Triosteum angustifolium.* In the pools have been found *Carex decomposita, Potamogeton hybridus* and *P. pauciflorus*, while on a rocky headland a large "water-pocket" has yielded my only specimen of the white water lily (*Nymphœa odorata*). *Cratœgus parvifolia, Rumex verticillatus Steironema lanceolatum*, and last but not least, *Nasturium lacustre*, have also rewarded my researches in this singular and rather weird region.

On the opposite side of the river the site of the ancient canal around the Falls has proved very fertile in botanical trophies. *Polygala ambigua* is found near the boat landing, while by climbing the cliffs below this point the native of more northern climes may gaze once more upon his familiar Hemlock Spruce, *Tsuga Canadensis.* Difficult Run, a mile farther down, though indeed difficult of approach, repays the effort with *Podostemon ceratophyllus, Smilacina stellata, Potamogeton Claytonii*, and numerous other herbal treasures.

3. *The Lower Potomac Region.*

Passing next to the lower Potomac, the localities of special interest are, 1. Custis Spring, opposite the Arlington estate, with the extensive marsh below, where *Sagittaria pusilla, Discopleura capillacea, Cyperus erythrorhizus*, and other rare species are alone known to grow. 2. The point and bay below Jackson City, known as Roach's run, where are found, among others, *Scrophularia nodosa,* *Tripsacum dactyloides* and *Pycnanthemum lanceolatum.* 3. Four Mile run, half way to Alexandria, not yet sufficiently explored, including the vicinity of Fort Scott to the northwest, where *Clematis ochroleuca* and *Asclepias quadrifolia* may be collected; and, 4. Hunting creek, a large estuary below Alexandria, including Cameron run, the stream which debouches into it, with its tributaries, Back Lick run and Holmes run, which unite to form it. Here have been found, at various points, *Clematis ochroleuca, Gonolobus hirsutus, Itea Virginica, Geranium columbinum, Micranthemum Nuttallii, Habenaria virescens, Quercus macrocarpa, Carex gracillima, Geum strictum, Galium asprellum,* and very many other rare plants.

On the left bank of the lower Potomac the chief locality of interest is a large wooded area below the Government Hospital for the Insane. This has proved a rich hunting ground for the botanist, and has yielded *Carex pallescens, Carex Woodii, Gonolobus hirsutus, Silene armeria, Parietaria Pennsylvanica, Myosotis arvensis, Scutellaria nervosa,* &c., &c. *Asplenium angustifolium* is known only at Marshall Hall, where it has been reported by Mr. O. M. Bryan, while opposite Fort Foote Mr. Zumbrock has found *Myriophyllum spicatum,* and opposite Alexandria Professor Comstock and Miss Willets have discovered *Plantago cordata.*

4. *The Terra Cotta Region.*

This embraces some low grounds and undulating barrens near the terra cotta works, at Terra Cotta Station, on the Metropolitan Branch of the Baltimore and Ohio railroad, three miles from the city, and also a small swamp a quarter of a mile beyond, and to the eastward. Here on the dry ground have been found *Onosmodium Virginianum, Lespedeza Stuvei, Clitoria Mariana,* and *Habenaria lacera;* and in the swamp *Aster æstivus, Solida stricta, Woodwardia Virginica, Asclepias rubra, Poterium Canadense,* and numerous other plants rare or absent in other localities.

5. *The Reform School Region.*

This locality is very limited in extent, but has proved one of the most fertile in botanical rarities. Its nucleus consists of a little swampy spot a short distance to the south of the National Reform School, in which is located a beautiful spring; but the woody tract of country surrounding this and stretching southward and eastward some distance has also proved very fruitful. In the different portions of this region have been discovered *Phlox maculata, Melanthium Virginicum, Bartonia tenella, Lespedeza Stuvei, Desmodium Marilandicum* and *D. cilare, Buchnera Americana, Fimbristylis capillaris, Quercus prinoides, Carex bullata,* and *Gentiana ochroleuca,* most of which do not occur at all elsewhere.

6. *The Holmead Swamp Region.*

Like the last, this locality is quite circumscribed in area, but like it, too, it is rich in interesting plants. It occupies a ravine leading to Piney Branch from the east at the point where the continuation of Fourteenth street crosses that stream. The road connecting the last named with the Rock Creek Church road, and which is called Spring street, follows this valley. The collecting grounds are on the south side of this road and in the springy meadow along the rill. The timber has long been cut off, but the boggy character of the ground has thus far protected it from cultivation. The pasturing of animals on it during a portion of the year has latterly become a serious detriment to the growth of plants. Mr. Holmead, who owns it and lives near by, has kindly permitted botanists to investigate it for their purposes. Here have been found *Ludwigia hirsuta, Drosera rotundifolia Asclepias rubra, Xyris flexuosa, Fuirena squarrosa, Rhinchospora alba, Coreopsis discoidea* and the beautiful *Calopogon pulchellus* the most showy of our orchids.

In addition to these specially fertile tracts there are many other localities of great interest where valuable accessions to our flora have been made, and which will be particularly designated under the names of these species. It will suffice here to mention a wet meadow between the National Driving Park and Bladensburg, where, in a very diminutive spot, *Sarracenia purpurea, Viola lanceolata,* and *Carex bullata,* the two first wholly unknown elsewhere, have been discovered; a marsh a mile from Bladensburg, near the millrace, where only the majestic *Stenanthium robustum* has been seen; a little swamp near the Sligo creek, between the Riggs and Blair roads, where the Hartford fern (*Lygodium palmatum*) grows sparingly; and another between Bladensburg and the Maryland Agricultural College, where *Solidago elliptica, Ascyrum stans,* and *Lycopodium complanatum,* var. *Sabinæfolium,* have been found. The

Eastern branch region is not specially rich in floral treasures, but on its banks and marshes some good things appear. *Habenaria virescens*, *Steironema laceolatum*, *Eleocharis quadrangulata*, *Scirpus fluviatilis*, *Ranunculus ambigens*, and *Salix Russelliana* are among these, though some of them are found elsewhere.

Flowering time of Plants.

It has already been remarked that most species flower at Washington much earlier than at points farther north or the dates given in the manuals. In consequence of this, a botanist unacquainted with this fact, and accustomed to those climates and to relying upon the books, would be likely to be behind the season throughout the year, and fail to get the greater part of the plants he desired. With all my efforts to make allowance for this fact, I have frequently been sorely disappointed and was at last driven to making a careful record, preserving and correcting it from year to year, of the *flowering time* of plants in this locality. The notes on this subject appended to nearly every species enumerated in the list embody the general results of these observations and may in the main be relied upon. The expressions used are not loose conjectures, but are in the nature of compilations from recorded data. In most cases an allowance of two weeks may be made for the difference in seasons though rarely more and often less. Certain plants, as for example, *Tipularia discolor*, flower at almost exactly the same time every year. Occasionally, however, one will vary a month or more in a quite unaccountable way. But any one who has watched the periodical changes of the general vegetation for a series of years and recorded his observations, will more and more realize the exactness even of these complex biological phenomena which depend so absolutely upon uniform astronomical events.

From this point of view the season which presents the greatest variation and also, for this and other reasons, the greatest interest is the spring. There are a few plants which may sometimes be found in flower here in January, such as *Stellaria media*, *Taraxacum dens-leonis or Acer dasycarpum* (collected Jan. 17, 1876, in the city) in favored places, but these will bloom at any time when a few days of mild weather with sunshine can come to revive them. There are, however, several strictly vernal species which bloom quite regularly in the latter part of February, such as *Symplocarpus fœ-*

tidus, *Chrysosplenium Americanum*, and often *Anemone hepatica*. The number regularly found in flower in March is quite large and in special years very large. It was of course impossible to make observations every day of any year, but taking a number of years my observations cover nearly every day of the spring season. As showing the number of these early vernal species and also how widely the seasons may differ, the following facts are presented:

In the year 1878 seventeen species had actually been seen in flower and noted up to March 24th. I did not go out again that year until April 7, when I enumerated forty-six additional species, making sixty-three in all up to that date. This was an exceptionally early season. The next spring, that of 1879, was a backward one, as is shown by the fact that while I had visited the same localities, and taken notes with equal care only thirty-three species had been seen in flower up to April 13th: twenty-nine species which had been seen in flower on April 7th, 1878, were not yet in flower in the same localities on April 13th, 1879. There appeared to be about three week's difference in these two seasons. The last season, 1880, was again an early one, though less so than 1878. It was, however, near enough to the average to render the facts observed of great value. The following are a few of them: On February 29th, seven species were seen in flower in the Rock Creek region. On April 4th, thirty were enumerated on the Virginia side of the Potomac, above the Aqueduct Bridge. On April 11th, eleven were seen in addition to those previously enumerated in the Eastern Branch region: and on the 18th of April, High Island was visited, and twenty-nine added to all previously recorded, three of which were then in fruit. The total to this date was therefore seventy species. This season I concluded was a week or ten days later than that of 1878, and as much earlier than that of 1879.*

* Since the above was written the present season (1881) has passed its vernal period. It has proved still more backward than 1879 and the latest spring thus far observed. On April 3d, I made my first excursion and visited the Virginia side of the Potomac above Rosslyn. Only 7 species were seen in flower including *Alnus serrulata* which doubtless can be obtained much earlier in ordinary years, but has been overlooked. Besides *Draba verna*, a January species, and *Anemone hepatica*, a February one, the only herbaceous flower found was *Sanguinaria Canadensis*. On April 10th, High Island was visited, but only 8 species could be added to the above 7, and several of these, as *Jeffersonia diphylla*, *Dicentra cucullaria*, *Saxifraga Virginiensis*, *Erythronium Americanum*, and *Stellaria pu-*

We may now inquire what some of these early plants are. The following have been observed in flower in February:

Chrysosplenium Americanum, February 17, 1878.
Anemone Hepatica, February 20, 1876.
Salix Babylonica, February 22, 1874.
Populus alba, February 22, 1874.
Draba verna, February 24, 1878.
Acer dasycarpum, February 24, 1878,
Stellaria media, February 29, 1880.
Cerastium viscosum, February 29, 1880.
Claytonia Virginica, February 29, 1880.
Acer rubrum, February 29, 1880.
Symplocarpus fœtidus, February 29, 1880.

To these should, perhaps, be added *Equisetum hyemale*, which was found February 17, 1878, near the receiving reservoir with the spikes well advanced, quite contrary to the books which make it fruit in summer.

In addition to the above, which may often also be seen later, the the following have been noted flowering in March:

Populus alba, March 3, 1874,
Viola pedata, March 5, 1876.
Houstonia cœrulea, March 5, 1876.
Obolaria Virginica, March 5, 1876.
Dentaria heterophylla, March 8, 1874.
Poa brevifolia, March 8, 1874.
Capsella Bursa-pastoris, March 10, 1878.
Lamium amplexicaule, March 10, 1878.
Lindera Benzoin, March 10, 1878.
Epigaea repens, March 15, 1874.
Ulmus fulva, March 15, 1874.
Luzula campestris, March 15, 1874.
Saxifraga Virginiensis, March 16, 1879.
Sanguinaria Canadensis, March 17, 1878.
Sisymbrium Thaliana, March 17, 1878.

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bera, were very sparingly out. Cold weather continued to the end of the third week in April, and on April 24th, when High Island was again visited and a thorough canvas made, only 22 additional plants could be found there, and the whole number seen to that date was 46. The conclusion was that up to that time the season was about three weeks later than that of 1880.

Salix tristis, March 17, 1877.
Populus grandidentata, March 21, 1880.
Corydalis flavula, March 22, 1874.
Thalictrum anemonoides, March 24, 1878.
Dentaria laciniata, March 24, 1878.
Antennaria plantaginifolia, March 24, 1878.
Erodium cicutarium, March 27, 1874.
Erigenia bulbosa, March 28, 1875.
Cardamine hirsuta, March 30, 1879.

It is about the first of April, especially in early years, that the vegetation seems to receive the greatest impetus. This is well shown by the following list of species seen in flower during the first week in April:

Ulmus Americana, April 1, 1873.
Jeffersonia diphylla, April 2, 1876.
Cardamine rhomboidea, April 2, 1876.
Stellaria pubera, April 2, 1876.
Thaspium aureum, April 2, 1876.
Euphorbia commutata, April 2, 1876.
Alnus serrulata, April 3, 1881.
Ranunculus abortivus, April 4, 1880.
Dicentra Cucullaria, April 4, 1880.
Arabis laevigata, April 4, 1880.
Viola tricolor. var. arvensis, April 4, 1880.
Vicia Caroliniana, April 4, 1880.
Amelanchier Canadensis, April 4, 1880.
Nepeta Glechoma, April 4, 1880.
Sassafras officinale, April 4, 1880.
Carpinus Americana, April 4, 1880.
Ostrya Virginica, April 4, 1880.
Erythroneum Americanum, April 4, 1880.
Barbarea vulgaris, April 5, 1874.
Pedicularis Canadensis, April 5, 1874.
Mertensia Virginica, April 5, 1874.
Ranunculus abortivus, var. micranthus, April 7, 1878.
Ranunculus repens, April 7, 1878.
Asimina triloba, April 7, 1878.
Caulophyllum thalictroides, April 7, 1878.
Arabis dentata, April 7, 1878.

Barbarea praecox, April 7, 1874.
Sisymbrium Alliaria, April 7, 1878.
Viola cucullata, April 7, 1878.
Viola striata, April 7, 1878.
Viola glabella, April 7, 1878.
Ionidium concolor, April 7, 1878.
Silene, Pennsylvanica, April 7, 1878.
Cerastium vulgatum, April 7, 1878.
Cerastium oblongifolium, April 7, 1878.
Geranium, maculatum, April 7, 1878.
Oxalis corniculata, April 7, 1878.
Cercis Canadensis, April 7, 1878.
Potentilla Canadensis, April 7, 1878.
Thaspium trifoliatum, April 7, 1878.
Cornus florida, April 7, 1878.
Chrysogonum, Virginianum, April 7, 1878.
Senecio aureus, April 7, 1878.
Fraxinus viridis, April 7, 1878.
Phlox divaricata, April 7, 1878.
Lithospermum arvense, April 7, 1878.
Betula nigra, April 7, 1878.
Populus monilifera, April 7, 1878.
Arisaema triphyllum, April 7, 1878.
Erythronium albidum, April 7, 1878.
Trillium sessile, April 7, 1878.

My special observations on the vernal flowering time of plants extend about two weeks later or to the end of the third week in April, after which the great number of plants in bloom, including the amentaceous trees, render it difficult to pursue the investigation, while at the same time the facts become less valuable. The results for the second and third weeks of April, always excluding all previously enumerated, are as follows:

Arabis lyrata, April 9, 1876.
Fraxinus pubescens, April 11, 1880.
Salix cordata, April 11, 1880.
Salix purpurea, April 11, 1880.
Vaccinium corymbosum, April 12, 1880.
Carex platyphylla, April 12, 1880.
Poa annua, April 12, 1874.

Thalictrum dioicum, April 14, 1876.
Rhus aromatica, April 14, 1878.
Phlox subulata, April 14, 1878.
Arabis patens, April 18, 1880.
Cardamine hirsuta, var sylvatica, April 18, 1880.
Negundo aceroides, April 18, 1880.
Erigeron bellidifolius, April 18, 1880.
Krigia Virginica, April 18, 1880.
Sisyrinchium Bermudiana, April 18, 1880.
Carex laxiflora, April 18, 1880.
Carex Emmonsii, April 18, 1880.
Melica mutica, April 18, 1880.
Anemone nemorosa, April 19, 1874.
Viola cucullata, var. cordata, April 19, 1874.
Dirca palustris, April 19, 1874.
Carex Pennsylvanica, April 19, 1874.
Lathyrus venosus, April 21, 1878.
Ribes rotundifolia, April 21, 1878.
Salix nigra, var. Wardi, April 21, 1878.

We thus see that a single collector has in the course of eight year's operations actually observed and noted eleven species in bloom in February, 24 more in March, 51 additional in the first week of April, and 26 others during the second and third weeks of April or 112 up to April 21.

It should be remarked that there is no doubt that if the same localities in which the large numbers were observed on April 2 1876, April 4, 1880, and April 7, 1878 had been visited in the last days of March of those years quite a number of these plants would have been found sufficiently advanced to demand a place in the lists, and thus the month of March would have been credited with so many here set down for the first week in April. Probably, all things considered, not less than fifty species in certain favored seasons either reach or pass by their flowering-time by the end of March.

In arranging the above lists the order of dates has of course taken precedence, but where several are enumerated under one date the natural order is followed.

It is scarcely necessary to suggest a caution to collectors against relying upon these dates in making collections. They represent the earliest observations and not the average. In most cases an allowance of at least one week should be made for the full blooming of all the individuals of any given species. In all cases, however, one or more individuals were actually seen in flower and sufficiently advanced for collection, otherwise no note was taken. The *Carices* of course had not advanced to developed perigynia, and many plants whose inflorescence is centrifugal or centripetal, or which develop fruit while retaining their flowers, should be looked for at a later stage.

Autumnal Flowering.

One of the most interesting peculiarities of the flora of this vicinity is that of the second-blooming of vernal species, which in most cases takes place quite late in the fall. [See *Field and Forest*, April–June, 1878, Vol. III, p. 172.] In addition to the seven species observed and published in 1878, I have noted more than as many others manifesting this habit, and it is probable that still others will yet be added. The following is a list of those thus far recorded with the dates at which they were observed and which may be compared with those of their regular vernal period:

Ranunculus abortivus, var. micranthus, November 28, 1875.
Cardamine hirsuta, October 3, 1880.
Viola pedata, var. bicolor, September 22, and December 8, 1878
Viola striata, September 10, 1876.
Fragaria Virginiana, September 22, 1878.
Rubus villosus, September 22, and October 27, 1878.
Lonicera Japonica, October 13, 1878.
Houstonia purpurea, October 13, 1878.
Houstonia purpurea, var. angustifolia, September 12, 1880.
Houstonia cærulea, September 7, 1879.
Vaccinium stamineum, October 13, 1878.
Rhododendron nudiflorum, October 13, 1878.
Sabbatia angularis, October 27, 1878.
Phlox divaricata, October 16, 1873.
Echium vulgare, October 8, 1880.
Veronica officinalis, October 8, 1873.
Agrostis scabra, November 12, 1876.

To this list of seventeen should perhaps be added *Stellaria pubera*, which instead of a vernal and autumnal period, has two vernal periods as described under that species in the systematic notes.

Salix longifolia has this year (1881,) flowered twice ; once in April and again in June.

Autumnal blooming, in so far as it is peculiar to this climate, may be chiefly attributed to the tolerably regular occurrence here of a hot and dry season in midsummer. This usually begins towards the end of June and ends about the middle of August. During this period, in some seasons, the ground and vegetation become parched and dried up, so that vegetal processes in many plants cease almost as completely as in the opposite season of cold. From this dormant state, the warm and often copious rains of the latter part of August revive them, as do the showers of spring, and they begin anew their regular course of changes. The frosts of October usually cut their career short before maturity is reached, but in some cases two crops of seed are produced. In addition to this, there frequently also occurs a very warm term in November, often extending far into December, and of this certain species take advantage and push forth their buds and flowers.

Albinos.

Well defined albinos have been collected of the following species

Desmodium nudiflorum.
Liatris graminifolia.
Rhododendron nudiflorum.
Vinca minor.
Mertensia Virginica.
Sabbatia angularis.
Pontederia cordata.

The green flowered variety of *Trillium sessile* is also common, and *Gonolobus obliquus* exhibits on High Island this same anomalous feature. *Carex tentaculata* having the spikes perfectly white, as if etiolated, was found June 14 of this year, (1881,) on the Eastern Branch marsh. This last phenomenon was certainly due neither to maturity or disease, but was a mere *lusus naturæ.*

Double Flowers, &c.

Thalictrum anemonoides, Ranunculus bullosus, Claytonia Virginica, and *Rubrus Canadensis,* have been found with the flowers much doubled as in cultivation.

Hydrangea arborescens occasionally has the outer circle of petals expanded as in cultivation.

Rudbeckia fulgida has been found with all its rays tubular but of the usual length.

Statistical View of the Flora.

In order to present a clear view of the general character of the vegetation of the District of Columbia and the adjacent country, I have made a somewhat careful analysis of the large groups and families, and comparison of them not only with each other, but with the same groups and families in larger areas and other local floras. The general results are presented below.

It is important to remark that in all enumerations, it is not simply the number of *species*, as at present recognized, but the number of *different plants*, (species and varieties,) that is employed. The reason for doing this is that in very many cases, well marked varieties are eventually made species, and if two plants really differ there is little probability that they will ever be merged into one species without that difference being indicated by some difference of name. The aim has therefore been to take account of the number of plants without regard to the manner in which they are named.

The whole number of vascular plants now known to this flora, as catalogued in the list appended to this paper, is 1249, and these belong to 527 different genera, or about 2⅓ species to each genus. These are distributed among the several systematic series, classes, and divisions, as follows:

Groups.	Genera.	Species and varieties.
Polyptelæ	174	356
Gamopetalæ	169	389
Total Dichlamydeæ	343	745
Monochlamydeæ (Apetalæ)	47	124
Total Dicotyledons	390	869
Monocotyledons	112	331
Gymnospermæ (Coniferæ)	4	7
Total Phænogamia	506	1,207
Cryptogamia	21	42
Total vascular plants	527	1,249

The percentages of the total are as follows:

Polypetalæ	33	29
Gamopetalæ	32	31
Total Dichlamydeæ	65	60
Monochlamydeæ (Apetalæ)	9	10
Total Dicotyledons	74	70
Monocotyledons	21	26
Gymnospermæ (Coniferæ)	1	1
Total Phænogamia	96	97
Cryptogamia	4	3

Large Orders.

The sixteen largest orders arranged according to the number of species, are as follows:

	Genera.	Species and varieties.
1. Compositæ	53	149
2. Gramineæ	43	110
3. Cyperaceæ	10	108
4. Leguminosæ	24	57
5. Rosaceæ	15	46
6. Labiatæ	23	42
7. Cruciferæ	16	33
8. Scrophulariaceæ	15	32
9. Filices	16	30
10. Ranunculaceæ	7	27
11. Ericaceæ	11	26
12. Cupuliferæ	7	26
13. Orchidaceæ	12	24
14. Liliaceæ	18	24
15. Polygonaceæ	3	23
16. Umbelliferæ	17	22

The whole number of systematic orders represented in our District is 116, of which sixteen, or 14 per cent. furnish 55 per cent. of the genera and 62 per cent. of the species.

Large Genera.

The fifteen large genera arranged according to the number of plants are the following:

		Species and varieties.
1.	Carex	70
2.	Aster	21
3.	Panicum	19
4.	Solidago	18
5.	Quercus	18
6.	Polygonum	16
7.	Desmodium	14
8.	Salix	14
9.	Juncus	14
10.	Viola	13
11.	Cyperus	12
12.	Ranunculus	11
13.	Eupatorium	11
14.	Helianthus	10
15.	Asclepias	10

Thus fifteen, or less than three per cent., of the genera furnish 271, or nearly 22 per cent. of the species.

Introduced Species.

The whole number of introduced plants enumerated in the subjoined catalogue is 193, of which 15 are supposed or known to be indigenous to other parts of the United States.* These are distributed through the several larger groups as follows:

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* These are the following:

Xanthoxylum Americanum.
Trifolium repens.
Prunus Chicasa.
Rosa setigera.
Philadelphus inodorus.
Ribes rotundifolium.
Ribes rubrum.
Passiflora incarnata.
Symphoricarpus racemosus.
Symphoricarpus vulgaris.
Catalpa bignonioides.
Maclura aurantiaca.
Populus grandidentata.
Poa annua.
Pinus Strobus.

	Old World.	United States.	Total.
Polypetalous	65	8	73
Gamopetalous	54	3	57
Apetalous	28	2	30
Monocotyledonous	31	1	32
Coniferæ	--	1	1
Total	178	15	193

It will be seen that the introduced plants amount to 15.5 per cent. of the total flora.

The several orders to which these belong, are shown in the summary.

Shrubby Species.

Of the 342 "Forest Trees" enumerated in Sargent's preliminary catalogue of 1880, this flora embraces 85, or 24.8 per cent., of which 65 are large enough to have the dignity of timber trees. Of these 85, 25 are in the Polypetalous Division, but only 12 of this latter number are large; 9 are in the Monopetalous Division, all but 2 of which are large; 44 are in the Apetalous Division, 39 of which are large; and the remaining 7 are Coniferous, all full-sized trees.

The whole number of species which are shrubby or woody above ground is 194, which is 15.5 per cent. of the whole; they are distributed as follows:

Polypetalous	83
Gamopetalous	36
Apetalous (Monochlamydeous)	64
Monocotyledonous (Endogenous)	4
Gymnospermous (Coniferous)	7
Total	194

For further particulars the reader can consult the Summary at the end of the catalogue.

Comparisons with other Floras.

While these facts are of great interest in affording a clear conception of the character of our flora, they do not aid us in determining in what respects it is peculiar or marks a departure from those of other portions of the country, or from that of the country at large. To institute comparisons with other local floras would of course carry me much too far for the general purpose of this paper, but it is both more interesting and more practicable to confront a few of the above results with similar ones, drawn from a consideration of a large part of the United States. For this purpose, as not only most convenient but as least liable to embrace facts calculated to vitiate the comparisons, I have chosen that portion of the United States situated east of the Mississippi river, and for the most part well covered by *Gray's Manual of Botany* for the Northern portion and *Chapman's Flora of the Southern States* for the Southern. The plants described in these works are conveniently collected into one series by the second edition of *Mann's Catalogue*, published under the supervision of the authorities at Cambridge, in 1872. Many changes have since been made in the names, &c., and a few new species added, but these are not sufficient to affect the general conclusions to be drawn from the following comparative tables.

Comparison of Species and Varieties.

The number of species and varieties of vascular plants enumerated in the work above referred to is 4,034, of which the 1,249 of the flora of Washington, by groups, is as follows:

	Species and varieties in the		Per Cent.
	Eastern U. S.	Flora Columbiana.	
Polypetalæ	1,115	356	32
Gamopetalæ	1,314	389	30
Total Dichlamydeæ	2,429	745	31
Monochlamydeæ (Apetalæ)	349	124	36
Total Dicotyledons	2,778	869	31
Monocotyledons (Endogens)	1,034	331	32
Gymnospermæ	28	7	25
Total Phænogamia	3,840	1,207	31
Cryptogamia	194	42	22
Total vascular plants	4,034	1,249	31

Comparison of Genera.

The whole number of genera in the flora of the Eastern United States is 1065. That of the Flora Columbiana, as already stated is 527. This is over 49 per cent., a much larger proportion than was shown by a comparison of the species. A comparison of the genera by classes, gives the following results:

	Genera represented in the		Per Cent.
	Eastern U. S.	Flora Columbiana.	
Polypetalæ	340	174	51
Gamopetalæ	379	169	45
Total Dichlamydeæ	719	343	48
Monochlamydeæ (Apetalæ)	97	47	48
Total Dicotyledons	816	390	48
Monocotyledons	198	112	57
Gymnospermæ	12	4	33
Total Phænogamia	1,026	506	49
Cryptogamia	39	21	54
Total vascular plants	1,065	527	49

The percentages here range from 33 in the Gymnosperms to 57 in the Monocotyledons, averaging between 49 and 50, whereas in the similar comparisons for species they ranged from 22 in the Cryptogams to 36 in the *Monochlamydeæ.* This result was to be expected since as the groups increase, the number represented in any local flora should be proportionally larger. For example, 116 orders out of the 156 are represented here, which is upwards of 74 per cent.

Comparison of Large Orders.

It will be interesting to compare in a manner similar to the foregoing, the number of species in several of the largest orders. For this purpose we may use the same orders mentioned a few pages back as the richest in species of any belonging to this flora. The comparison may then be shown as follows:

Orders.	Eastern U. S.	Flora Col.	Per Cent.
1. Compositæ	497	149	30
2. Gramineæ	297	110	37
3. Cyperaceæ	357	108	30
4. Leguminosæ	208	57	27
5. Rosaceæ	104	46	44
6. Labiatæ	121	42	35
7. Cruciferæ	76	33	43
8. Scrophulariaceæ	97	32	33
9. Filices	134	30	22
10. Ranunculaceæ	80	27	34
11. Ericaceæ	89	26	29
12. Cupuliferæ	45	26	58
13. Orchidaceæ	71	24	34
14. Liliaceæ	82	24	29
15. Polygonaceæ	56	23	41
16. Umbelliferæ	63	22	35

This table exhibits better perhaps than any other the special charateristics of the flora. The normal percentage being about 31, we see that in all but five of these sixteen largest orders our flora is in excess of that standard, while it is richest proportionally in the *Cupuliferæ*, *Rosaceæ*, and *Cruciferæ*, and poorest in the *Filices*, and *Leguminosæ*.

Comparison of Large Genera.

In like manner we may compare the fifteen large genera given in a preceding table.

Genera.	Eastern U. S.	Flora Col.	Per Cent.
1. Carex	180	70	39
2. Aster	63	21	33
3. Panicum	36	19	53
4. Solidago	61	18	30
5. Quercus	38	18	47
6. Polygonum	27	16	59
7. Desmodium	24	14	58
8. Salix	23	14	61
9. Juncus	38	14	37
10. Viola	24	13	54
11. Cyperus	41	12	29
12. Ranunculus	27	11	41
13. Eupatorium	24	11	46
14. Helianthus	27	10	37
15. Asclepias	22	10	45

This table shows that in all the large genera except Solidago and Cyperus, the District of Columbia has more than its full proportion. The genus *Salix* is the one proportionally best represented, while *Polygonum, Desmodium, Panicum* and *Viola*, each exceed 50 per cent. *Quercus, Eupatorium* and *Asclepias* are also well filled out.

As already remarked, it would carry us too far to undertake the systematic comparison of our flora with those of other special localities, even were the data at hand. Few local catalogues are condensed and summarized for this purpose and the labor of doing this is very great. The recently published *Flora of Essex County Massachusetts*, prepared by Mr. John Robinson, however, forms something of an exception to this, and we may directly compare the larger classes and also the orders. The following tables will give an idea of the differences between that flora and our own:

Series, Classes, and Divisions.	Number of Orders.		Number of Genera.		Number of Species and Varieties.	
	Essex County.	Washington.	Essex Connty.	Washington.	Essex County.	Washington.
Polypetalæ	42	45	155	174	360	356
Gamopetalæ	25	27	158	169	358	389
Total Dichlamydeæ	67	72	313	343	718	745
Monochlamydeæ	18	19	44	47	132	124
Total Dicotyledons	85	91	357	390	850	869
Monocotyledons	17	20	120	112	392	331
Gymnospermæ (Coniferæ)	1	1	7	4	17	7
Total Phæenogamia	103	112	484	506	1,259	1,207
Cryptogamia	5	4	20	21	65	42
Total vascular plants	108	116	504	527	1,324	1,249

The sixteen large orders enumerated on page 89 may also be compared with profit:

Large Orders.	Number of Genera.		Number of Species and Varieties.	
	Essex County.	Washington.	Essex County.	Washington.
1. Compositæ	43	53	136	149
2. Gramineæ	50	43	128	110
3. Cyperaceæ	9	10	120	108
4. Leguminosæ	17	24	39	57
5. Rosaceæ	12	15	55	46
6. Labiatæ	22	23	35	42
7. Cruciferæ	14	16	29	33
8. Scrophulariaceæ	14	15	29	32
9. Filices	13	16	40	30
10. Ranunculaceæ	9	7	30	27
11. Ericaceæ	18	11	37	26
12. Cupuliferæ	6	7	16	26
13. Orchidaceæ	13	12	32	24
14. Liliaceæ	18	18	27	24
15. Polygonaceæ	3	3	27	23
16. Umbelliferæ	16	17	20	22

In the flora of Essex County, the orders *Umbelliferæ* (20) and *Cupuliferæ* (16) fall below the lowest of the sixteen for the flora of Washington, (*Umbelliferæ* 22,) while on the other hand the *Caryphyllaceæ* (27,) *Salicaceæ* (23,) and *Naiadaceæ* (28,) not in the list, rise above that number. These orders in the flora of Washington are represented respectively by 19, 19, and 9 species and varieties. With reference to the last named of these orders, however, it may be remarked that the genus *Potamogeton*, which constitutes the greater part of it, has been imperfectly studied here, and will certainly be largely increased when thoroughly known.

The orders in which this flora falls below that of Essex county are: the *Gramineæ*, *Cyperaceæ*, *Rosaceæ*, *Filices*, *Ranunculaceæ*, *Ericaceæ*, *Liliaceæ*, *Orchidaceæ*, and *Polygonaceæ*, nine in all. In the remaining seven orders there is a greater number of species here than there. It is noteworthy that our flora exceeds that of Essex county most in the *Compositæ*, *Leguminosæ*, and *Cupuliferæ*, and next to these in the *Scrophulariaceæ*, *Labiatæ* and *Cruciferæ*. Our comparatively poorest orders are the *Cyperaceæ*, *Rosaceæ*, *Ericaceæ* and *Filices*. Comparing in like manner the fifteen large genera enumerated on page 90 we are able to see still more definitely wherein the two floras differ.

Large Genera.	Number of Species and Varieties.	
	Essex County.	Washington.
1. Carex	71	70
2. Aster	25	21
3. Panicum	14	19
4. Solidago	19	18
5. Quercus	10	18
6. Polygonum	21	16
7. Desmodium	7	14
8. Salix	18	14
9. Juncus	14	14
10. Viola	11	13
11. Cyperus	11	12
12. Ranunculus	13	11
13. Eupatorium	7	11
14. Helianthus	5	10
15. Asclepias	7	10

The total number of species and varieties represented by these fifteen genera is thus considerably larger in the Washington flora (271,) than in that of Essex county, (253;) but whereas they are absolutely the largest genera here, this is not the case there. The genus *Potamogeton* numbers 23 in Mr. Robinson's Catalogue, and the genus *Scirpus* 14, while several others probably exceed ten. Those in the above list falling below ten, the lowest on the Washington list, are *Desmodium* (7,) *Eupatorium* (7,) *Asclepias* (7,) and *Helianthus* (5.) Those in which the Essex flora exceeds the Washington flora are *Carex*, *Aster*, *Solidago*, *Polygonum*, *Salix* and *Ranunculus*, though *Carex*, *Solidago* and *Cyperus* may be regarded as equal in the two floras, and *Juncus* is exactly equal. In *Quercus*, *Desmodium*, *Eupatorium*, *Helianthus* and *Asclepias*, the Essex flora is poor, only amounting in the second and fourth named, to half the number found here.

Relative to the above comparisons in general, it may be remarked first, that the flora of Essex county, Massachusetts, is much more thoroughly and exhaustively elaborated than that of the District of Columbia, lying as it does in the immediate center of botanical activity in this country. This alone is probably sufficient to account for all the difference in the number of species in the two localities, and it will probably be ultimately found that the two floras are very nearly equal. In the second place, if it should be thought that from its intermediate location between the southern and the northern sections of the country, our flora should naturally be the more rich in species, it may be satisfactorily urged on the other hand, that while we have only an inland territory, Essex county has both an inland and a maritime territory. Could our range be extended to embrace even a small extent of sea coast, the number would thereby be very largely increased.

As a final statistical exhibit, more comprehensive in its scope, and from a different point of view, I give below a table in which our local flora is compared not only with the floras above named, but with several others in America. As these several floras not only overlap to a considerable extent, but also differ widely in the total number of plants embraced by each, it is evident a numerical comparison would convey a very imperfect idea of the variety in their essential characteristics. It is therefore necessary to reduce them to a common standard of comparison, which has been done by disregarding the actual numbers and employing only the percentage which each group compared bears to the total for each respective flora. The relations of the several groups to the total vegetation of each flora is thus brought out, and a comparison of the percentages of the same group in the different areas displays in the clearest manner possible the predominance or scantiness of the groups in each flora. Upon this must depend, in so far as botanical statistics can indicate it, the *facies* of each flora, its peculiarities and characteristics. As in previous comparisons, the table is restricted to Phenogamous and vascular Cryptogamous plants, and the same groups are employed, except that the large genera are omitted, while the number of orders is increased to the 23 largest of this flora, which is taken as the basis of comparison, and they are arranged in the order of rank with reference to it.

The several floras compared with the total number of plants embraced in each, are as follows:

1. Flora of Washington and vicinity	1,249
2. Flora of Essex county, Massachusetts	1,324
3. Flora of the State of Illinois	1,542
4. Flora of Northeastern United States	2,365
5. Flora of Southeastern United States	2,696
6. Flora of Eastern United States (= 4 + 5)	4,034
7. Plants collected by the Fortieth Parallel Survey	1,254
8. Plants collected by Lieut. Wheeler's Survey	1,535

For the flora of Illinois, (No. 3,) and also for that of the Northern United States, east of the Mississippi, (No. 4,) I have used, without verification, the figures of the *Catalogue of the Plants of Illinois*, 1876, prepared by Mr. Harry N. Patterson, as summarized in the preface. In the former case, the introduced species are included, but the varieties seem to be excluded. In the latter case, as stated by Mr. Patterson, the introduced species are excluded, as are also doubtless the varieties.

For the flora of the Southern United States, east of Mississippi, (No. 5,) which I have compiled from Dr. Chapman's *Flora of the Southern States*, indigenous species are alone taken, in order to make it conform as nearly as possible to the flora of the Northeastern United States, (No. 4.)

The plants collected by the Fortieth Parallel Survey, (No. 7,) and those collected on Lieut. Wheeler's Survey, (No. 8,) are introduced rather as a means of contrasting the Eastern with the Western portions of the continent, than as a proper part of the comparative botanical statistics of this vicinity. The former of these collections was very thoroughly and carefully made by an energetic and experienced botanist, Mr. Sereno Watson, and derives its chief value from this fact. It embraces, however, a territory having a somewhat special character from a botanical point of view, viz: in general terms, the Great Basin between the Rocky Mountains and the Sierra Nevada, and the High Plateaus and mountains immediately adjacent, (Wasatch, Uintas, Sierras,) with a restricted range north and south. The data are taken from the summary of the work prepared by Mr. Watson, and found on page XIV of the Report. The collections embraced in the Report of Lieut. Wheeler's Survey, on the other hand, were made by numerous collectors, some of them amateurs, and were scattered over a very wide extent of western territory, including Colorado, New-Mexico, Utah, Arizona and Nevada, and continued through five years of exploration. They may be taken therefore to represent, with some correctness, the general character of our Western Flora, exclusive of the Pacific Coast. The facts given are derived from the "Table of Orders" on page 379. In both cases varieties are excluded.

For the remaining floras compared in the table, (Nos. 1, 2, and 6,) to avoid re-compilation, the data previously used are repeated, species and varieties, including also introduced plants, being employed. As already intimated, however, this difference in the basis of compilation of different floras, applying as it does to the several groups and to the aggregate alike, cannot materially affect the percentages as computed.

The following is the Table of Percentages:

Groups.	Flora of Washington and Vicinity.	Flora of Essex County, Massachusetts.	Flora of the State of Illinois.	Flora of the Northeastern United States.	Flora of the Southeastern United States.	Flora of the total Eastern United States.	Plants collected by the 40th Parallel Survey.	Plants collected by Lieut. Wheeler's Survey.
Polypetalæ	28.5	27.2	28.5	26.8	28.9	27.6	35.1	31.9
Gamopetalæ	31.1	27.0	32.2	31.6	34.7	32.6	36.0	35.8
Total Dichlamydeæ	59.6	54.2	60.7	58.4	63.6	60.2	71.1	67.7
Monochlamydeæ	9.9	10.0	9.8	7.9	8.8	8.7	9.8	10.6
Total Dicotyledons	69.5	64.2	70.5	66.3	72.4	68.9	80.9	78.3
Monocotyledons	26.5	27.6	25.5	29.0	24.3	25.6	16.4	15.7
Gymnosperms	0.6	1.3	0.7	0.9	0.7	0.7	1.2	1.3
Total Phænogamia	96.6	95.1	96.7	96.2	97.4	95.2	98.5	95.3
Cryptogamia	3.4	4.9	3.3	3.8	2.6	4.8	1.5	4.7
Total vascular plants	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0

Orders.	Flora of Washington and Vicinity.	Flora of Essex County, Massachusetts.	Flora of the State of Illinois.	Flora of the Northern United States.	Flora of the Southern United States.	Flora of the total Eastern United States.	Plants collected by the 40th Parallel Survey.	Plants collected by Lieut. Wheeler's Survey.
1. Compositæ	11.9	10.3	13.0	12.2	13.7	12.3	16.5	16.6
2. Gramineæ	8.9	9.7	7.8	7.5	7.2	7.4	5.4	7.8
3. Cyperaceæ	8.6	9.1	8.5	10.5	8.0	8.9	4.4	3.8
4. Leguminosæ	4.6	2.9	4.7	4.3	6.1	5.2	7.2	8.2
5. Rosaceæ	3.7	4.2	3.2	3.0	2.2	2.6	3.4	2.9
6. Labiatæ	3.4	2.6	2.8	2.2	2.8	3.0	0.9	2.2
7. Cruciferæ	2.6	2.2	2.1	2.0	1.4	1.9	4.4	2.8
8. Scrophulariaceæ	2.6	2.2	2.7	2.3	2.5	2.4	4.5	4.8
9. Filices	2.4	3.0	2.3	2.4	2.1	3.3	1.0	4.3
10. Ranunculaceæ	2.2	2.3	2.7	2.3	1.9	2.0	3.0	2.3
11. Ericaceæ	2.1	2.8	0.9	2.9	2.0	2.2	1.3	0.9
12. Cupuliferæ*	2.1	1.8	1.4	1.5	1.3	1.4	0.4	0.9
13. Liliaceæ	1.9	2.0	2.1	2.4	2.1	2.0	3.0	1.5
14. Orchidaceæ	1.9	2.4	1.8	2.4	1.9	1.7	0.6	0.5
15. Polygonaceæ	1.8	2.0	1.9	1.1	1.5	1.4	4.0	3.2
16. Umbelliferæ	1.8	1.5	1.8	1.7	1.6	1.6	2.4	1.2
17. Caryophyllaceæ	1.5	2.0	1.4	1.5	1.5	1.5	2.2	1.6
18. Salicaceæ	1.5	1.7	1.2	0.8	0.3	0.7	0.9	0.8
19. Onagraceæ	0.9	1.1	1.2	1.2	1.3	1.1	2.3	2.4
20. Saxifragaceæ	0.7	1.0	0.8	1.5	0.9	1.1	2.1	1.4
21. Chenopodiaceæ	0.7	1.3	0.7	0.5	0.5	0.6	2.1	1.5
22. Naiadaceæ	0.7	2.1	1.2	1.2	0.4	1.0	0.7	0.3
23. Polemoniaceæ	0.5	0.1	0.5	0.3	0.5	0.4	3.3	1.8

* Including the Betulaceæ.

Comparisons have already been made of our local flora with that of Essex county, Massachusetts, which contains so nearly the same number of plants. In examining the percentages in the above table, these distinctions are equally manfest. In both divisions of the *Dichlamydeæ*, and also in the Dicotyledons, and the total *Phænogamia*, our flora is richer than that of Essex county, while in the *Monochlamydeæ*, the Monocotyledons, the Gymnosperms, and the Cryptogams, it falls below. In the *Compositæ*, *Leguminosæ*, *Labiatæ*, *Cruciferæ*, *Scrophulariceæ*, *Cupuliferæ*, and a few other orders it is in excess, while in the *Gramineæ*, *Cyperaceæ*, *Rosaceæ*, *Filices*, *&c.*, the Essex flora leads.

In the comparison with the flora of the State of Illinois, one is struck by the marked similarity in the position of the groups, notwithstanding the well known differences in the actual species. In the *Gamopetalæ*, and total *Dichlamydeæ*, as also in the *Monochlamydeæ* the difference is very slight, while in the *Polypetalæ* it disappears entirely. The Dicotyledons are therefore nearly the same, and we find this true also of the Monocotyledons, and the Gymnosperms. Whatever slight variations occur in the above named groups, they are so adjusted as nearly to balance each other, so that when we reach the total *Phænogomia*, we again have substantial unison, which of course is maintained in the *Cryptogamia*.

This harmony is less pronounced in the larger orders, the *Compositæ* being richer, and the *Gramineæ* poorer there than here. In the *Cyperaceæ*, *Leguminosæ*, *Scrophulariaceæ*, and *Filices*, the difference is not great, but in the *Rosaceæ*, *Labiatæ*, *Cruciferæ*, and *Cupuliferæ*, the Washington flora is decidedly in advance, and in the *Ericaceæ* it is of course in very marked contrast. In the *Orchidaceæ*, *Polygonaceæ*, *Umbelliferæ*, *Caryophyllaceæ*, and *Polemoniaceæ*, there is substantial, or exact identity. In the *Ranunculaceæ*, *Onagraceæ*, *Naiadaceæ*, and *Liliaceæ*, besides the *Compositæ* already mentioned, the Illinois flora leads that of Washington. On the whole there is a remarkable similarity in the facies of these two floras, which may be due to their inland situation, with fluriatile areas, and similar position as to latitude. Considering, however, the marked specific peculiarities of the flora of the flat prairies of the West, we would have naturally looked for a corresponding distinctness in the larger groups and orders.

The comparisons of our flora, from this point of view, with those of the Northern and Southern States, east of the Mississippi river, and with these two combined, as represented in the next three columns, proves of the highest interest, and will repay somewhat close inspection. It has often been asked, to what extent the flora of Washington is affected by influences of a peculiarly southern character, and while it has generally been conceded that it belongs clearly to the northern section of the country, many facts, such as those previously set forth, relative to autumnal flowering and early flowering, as well as to the number of species, which exhibit more or less green foliage throughout the winter, combine to give it a decidedly southern aspect. In so far as the method of testing such questions which has been here adopted can be relied upon, this southern leaning on the part of the Washington flora is clearly exhibited in this table. In letting the eye follow columns four and five, the differences are well marked in nearly all the groups, and in most of the large orders. These are what express statistically the essential characteristics of the northern as contrasted with the southern flora. It is also obvious that the figures in column six will, in most cases, express the mean between these two extremes. To obtain the true position of our flora, it is necessary to observe toward which of these extremes it most nearly approaches, and whether it falls on the northern or southern side of the mean established by column six. In instituting this comparison, we perceive at the outset, that in the Polypetalous division, it falls so far on the southern side as to come within four tenths of one per cent. of being identical with the flora of the Southern States. In the *Gamopetalæ,* however, it agrees quite closely with the flora of Northern States, so that in the *Dichlamydeæ* as a whole, it coincides very well with the mean for both sections. The *Monochlamydeæ* agree better with those of the Southern States and the total Dicotyledons fall largely on the southern side of the mean. The Monocotyledons also fall somewhat on the southern side, while the Gymnosperms are below the mean which here corresponds with the southern flora. This leaves the total Phænogams, occupying an intermediate position. The Cryptogams are also very nearly intermediate, though approaching the northern side.

Considering next the relations of the large orders, we find that in the *Compositæ* our flora is northern in aspect. In the *Gramineæ* it is very exceptionally rich, surpassing all the larger areas and approaching that of Essex county, Massachusetts. In the *Cyperaceæ,* which are peculiarly typical for the purpose, on account of being indigenous in all the floras, it does not correspond at all, either with the northern section or with the average of both sections, but does agree very closely with the exceptionally meager representation of the southern flora. The *Leguminosæ* are here northern in aspect, the *Rosaceæ,* like the *Gramineæ,* exceptionally rich, far exceeding either section, as is also the case with the *Labiatæ* and the *Cruciferæ.* The ferns are northern in their degree of representation, as are the *Ranunculaceæ* while the *Ericaceæ* and *Scrophulariaceæ* are southern. The *Cupuliferæ* again are anomalous and tower above all other floras. The *Liliaceæ* are southern, as are also the *Orchidaceæ.* The *Polygonaceæ* are in excess, and in so far southern in aspect, while the *Umbelliferæ,* also in excess, denote a northern inclination. The *Caryophyllaceæ* are remarkable for showing the same percentage in all of the four floras now under comparison. The *Salicaceæ* are largely in excess of every flora compared in the table, except that of Essex county, Massachusetts, while *Onagraceæ* and *Saxifragaceæ* both fall below the normal, the latter, however, showing a southern tendency. The *Naiadaceæ* are southern, as are also the *Polemoniaceæ*, while the *Chenopodiaceæ* are slightly in excess in their degree of representation.

Now, as this locality has been classed as northern, we should not expect to find it occupying an intermediate position, which would place it on the boundary line between the northern and the southern flora, but we should expect to find it agreeing closely with the northern flora, or at least lying midway statistically, as it does geographically, between the dividing line or medium, represented by the total eastern flora and the northern flora. So far is this from being the case, however, that we actually find it occupying a position considerably below the medium line, and between this and the line of the southern flora; a position which would be geographically represented by the latitude of Nashville or Raleigh, or even by Memphis or Chattanooga.

This result is very remarkable, and while the proofs from statistics are, perhaps, not alone to be relied upon, it serves to confirm many facts recorded which have puzzled the observers of the phenomena of the vegetable kingdom in this locality.

The results of the careful comparison of the two remaining columns need not be here summed up, as the reader will readily perceive their general import, and he will not be likely to stop with considering the relations of the local flora with those of the far West, but will probably seek for more general laws governing the vegetation of the eastern and western sections, as we have already done to some extent for the northern and southern sections.

Abundant Species.

It was Humboldt who remarked that of the three great Kingdoms of Nature, the Mineral, the Vegetable, and the Animal, it is the Vegetable which contributes most to give character to a landscape. This is very true, and it is also true, that botanists rarely take account of this fact. The latter are always interested in the relative numbers of species belonging to different Classes, Families, and Genera, rather than to the mere superficial aspect of the vegetation. It is, however, not the number of species, but individuals which give any particular flora its distinguishing characteristics to all but systematic botanists, and it is upon this, that in the main depends the commercial and industrial value of the plant-life of every region of the globe. It is often the omnipresence of a few, or even of a single, abundant species that stamps its peculiar character upon the landscape of a locality. This is to a far greater extent true of many other regions, especially in the far West, than it is of this; the vegetation of the rural surroundings of Washington is of a highly varied character, as much so perhaps as that of any part of the United States. And yet there are comparatively few species, which from their abundance chiefly lend character to the landscape, and really constitute the great bulk of the vegetation. The most prominent, if not actually the most numerous of these, are of course, certain trees and notably several species of oak. Probably the most abundant tree here, as in nearly all parts of the country, is *Quercus alba*, the white oak; but *Q. prunus*, the chestnut oak, *Q. coccinea*, the scarlet oak, *Q. palustris*, the swamp oak, and *Q. falcata*, the Spanish oak, are exceedingly common. The most abundant hickory is *Carya tomentosa*, the mockernut. *Liriodendron tulipifera*, the tulip-tree, often improperly called white poplar, besides being one of the commonest trees, is the true monarch of our forests, often attaining immense size. It is a truly beautiful tree whose ample foliage well warrants the recent apparently successful experiments in introducing it as a shade tree for the streets of the city. Among other common trees may be mentioned the chestnut, (*Castanea vulgaris, Lam, var. Americana*, A. D. C., the beech, (*Fagus ferruginea*,) the red maple, (*Acer rubrum*,) the sycamore, (*Platanus occidentalis*,) the red or river birch, (*Betula nigra*,) the white elm, (*Ulmus Americana*,) the sour gum, (*Nyssa multiflora*,) the sweet gum, (*Liquid-amber Styraciflua*,) the scrub pine, (*Pinus inops*,) the pitch pine, (*P. rigida*,) and the yellow pine, (*P. mitis*.)

Of the smaller trees, *Cornus florida*, the flowering dogwood and *Cercis Canadensis*, the red-bud or Judas tree are very abundant, and chiefly conspicuous in the spring from the profusion of their showy blossoms; all three species of sumac are common. *Hamamelis Virginica*, the witch-hazel, and *Virburnum prunifolium* the black haw abound; *Sassafras officinale*, sassafras, *Castania pumila*, the chinquapin and *Juniperus Virginiana*, the red cedar also belong to this class.

Of the smaller shrubby vegetation, we may safely claim as abundant *Cornus sericea*, and *C. alternifolia*, the silky, and the alternate-leaved normal *Viburnum acerifolium*, *V. dentatum*, and *V. nudum*, arrow-woods, *Gaylussacia resinosa*, the high-bush huckleberry, *Vaccinium stamineum*, the deer berry, *V. vacillans* and *V. corymbosum* the blueberries, *Leucothoë racemosa*, *Andromeda Mariana*, the stagger bush, *Kalmia latifolia*, the American laurel, or calico-bush, *Rhododendron nudiflorum*, the purple azalea flower, *Lindera Benzoin*, the spice bush.

Of vines besides three species of grape which are abundant, we have *Ampelopsis Virginiana*, the Virginian creeper or American woodbine, *Rhus toxicodendron*, the poison ivy, and *Tecoma radicans*, the trumpet vine, which give great beauty and variety to the scenery.

The most richly represented herbaceous species may be enumerated somewhat in their systematic order. Of *Polypetalæ*, may be mentioned *Ranunculus repens*, *Cimicifuga racemosa*, *Dentaria laciniata*, *Viola cucullata*, *Viola pedata*, *var. bicolor*, and *V. tricolor*, *var. arvensis*; *Stellaria pubera*, *Cerastium oblongifolium*, *Geranium maculatum*, *Impatiens pallida*, and *I. fulva*; *Desmodium nudiflorum*, *D. acuminatum*, and *D. Dillenii*; *Vicia Caroliniana*, *Potentilla Canadensis*, *Geum album*, *Saxifraga Virginiensis*, *Oenothera fruticosa*, and *Thaspium barbinode*. In the *Gamopetalæ* before *Compositæ*, we have *Galium aparine*, *Mitchella repens*, *Houstonia purpurea*, and *H. cœrulea*. In the *Compositæ*, the most conspicuous are; *Vernonia Noveboracense*, *Eupatorium purpureum*, *Liatris graminifolia*, *Aster patens*, *A. ericoides*, *A. simplex* and *A. miser*, *Solidago nemoralis*, *S. Canadensis*, *S. altissima*, and *S. ulmifolia*; *Chrysopsis Mariana*, *Ambrosia trifida*, and *A. artemisiæfolia*, (these behaving like introduced weeds;) *Helianthus divaricatus*, *Actinomeris squarrosa*, *Rudbeckia laciniata*, and *R. fulgida*; *Coreopsis verticillata*, *Bidens cernua*, *Verbesina Siegesbeckia*, *Gnaphalium polycephalum*, *Antennaria plantaginifolia*, *Hieracium venosum*, and *H. Gronovii*; *Nabalus albus*, and *N. Traseri*, *Lactuca Canadensis*.

The remaining *Gamopetalæ* furnish as abundant species: *Lobelia spicata*, *Chimaphila umbellata*, and *C. maculata*; *Veronica officinalis*, and *V. Virginica*, *Gerardia flava*, *Verbena hastata*, and *V. urticifolia*; *Pycnanthemum incanum*, and *P. linifolium*, *Collinsonia Canadensis*, *Salvia lyrata, Monarda fistulosa*, and *M. punctata; Nepeta glechoma, Brunella vulgaris, Mertensia Virginica, Flox paniculata*, and *P. divaricata; Solanum Carolinense*, and *Asclepias cornuti.*

Of herbaceous *Monochlamydeæ* may be named *Polygonum Virginianum, P. sagittatum*, and *P. dumetorum; Laportea Canadensis, Pilea pumila*, and *Bœmehria cylindrica.*

The *Monocotyledons* give us *Arisœma triphyllum*, the Indian turnip, *Sagittaria variabilis, Aplectrum hyemale, Erythronium Americanum, Luzula campestris, Juncus effusus, Juncus marginatus*, and *Juncus tenuis, Pontederia cordata.*

Of the *Cyperi, C. phymatodes, C. strigosus* and *C. ovularis* are the most common. *Eleocharis obtusa* and *E. palustris; Scirpus pungens, S. atrovirens, S. polyphyllus*, and *S. eriophorum*, are very conspicuous. Of *Carices, C. crinata, C. intumescens*, the various forms of *C. laxiflora, C. platyphylla, C. rosea, C. scoparia, C. squarrosa, C. straminea, C. stricta, C. tentaculata, C. virescens* and *C. vulpinoides*, are the most obtrusive. In the *Gramineæ*, those which most uniformly strike the eye are *Agrostis scabra, Muhlenbergia Mexicana*, and *M. sylvatica, Tricuspis seslerioides, Eatonia Pennsylvanica, Poa pratensis, Poa sylvestris*, and *P. brevifolia,; Eragrostis pectenacea, Festuca nutans, Bromus ciliatus, Elymus Virginicus, Danthonia spicata, Anthoxanthum odoratum, Panicum virgatum, P. latifolium, P. dichotomum*, (with a multitude of forms,) and *P. depauperatum; Andropogon Virginicus*, and *A. scoparius.*

Of ferns *Polypodium vulgare, Pteris aquilina, Adiantum pedatum, Asplenium ebeneum*, and *A. Filix-fœmina; Phegopteris hexagonoptera, Aspidium acrostichoides, A. marginale* and *A. Noveboracense; Osmunda regalis, O. Claytoniana*, and *O. cinnamonea*, are the most constantly met with.

Lycopodium lucidulum is quite common, and *L. complanatum* is very abundant in certain localities.

Besides the above, which are all indigenous to our flora, there are many introduced species in the vicinity of the city, and of cultivation everywhere which manifest here as elsewhere, their characteristic tendency to crowd out other plants and monopolize the soil.

Such are the most general features which the traveler accustomed to observe the vegetable characteristics of localities visited, may expect to see when he pays his respects to the Potomac valley. To some even this imperfect description might furnish a fair idea of our vegetable scenery without actually seeing it.

Classification Adopted.

In endeavoring to conform to the latest authoritative decisions relative to the most natural system of classification, I have followed, with one exception, the arrangement of the *Genera Plantarum* of Bentham and Hooker so far as this goes, and the accepted authorities of Europe and America for the remainder. For the *Gamopetalæ* after *Compositæ*, however, covered by Prof. Gray's *Synoptical Flora of North America*, I have followed that work which is substantially in harmony with the *Genera Plantarum*. In the arrangement of the orders, too, for the *Polypetalæ*, Mr. Sereno Watson's *Botanical Index* has in all cases been conformed to, as also not materially deviating from the order adopted by Bentham and Hooker. In the genera there are numerous discrepancies between the works last named, and in the majority of these cases the American authorities have been followed. For example, Bentham and Hooker have thrown *Dentaria* into *Cardamine*, *Elodes* into *Hypericum*, and *Ampelopsis* into *Vitis*, and *Pastinaca* and *Archemora* into *Peucedanum*. The change of *Spergularia* to *Lepigonum* is adopted, as well as a few alterations in orthography where the etymology seemed to demand them, as Pyrus to Pirus and Zanthoxylum to Xanthoxylum. I have also declined to follow Bentham and Hooker in the changes which they have made in the terminations of many ordinal names. The termination *aceæ* is doubtless quite arbitrary in many cases, and, perhaps, cannot be defended on etymological grounds but as a strictly ordinal ending it has done good service in placing botanical nomenclature on a more scientific footing. It is also true that the old system does not always employ it, as in some of the largest orders, *e. g. Cruciferæ*, *Leguminosæ*, *Compositæ*, *Labiatæ;* but whatever changes are made should rather be in the direction of making it universal than less general. Bentham and Hooker do not adopt a universal termination, neither do they abolish the prevailing one, and they retain it in the majority of cases; but in certain cases, for which they doubtless have special reasons, they substitute a different one, and one which is often far less euphonious. The following are the orders represented in this catalogue in which the termination *aceæ* is retained by American and altered by English authorities.

American.	*English.*
Berberidaceæ.	Berberideæ.
Cistaceæ.	Cistineæ.
Violaceæ.	Violarieæ.
Polygalaceæ.	Polygaleæ.
Caryophyllaceæ.	Caryophylleæ.
Portulacaceæ.	Portulaceæ.
Hypericaceæ.	Hypericineæ.
Celastraceæ.	Celastrineæ.
Vitaceæ.	Ampelideæ.
Saxifragaceæ.	Saxifrageæ.
Hamamelaceæ.	Hamamelideæ.
Lythraceæ.	Lythrarieæ.
Onagraceæ.	Onagrariæ.
Passifloraceæ.	Passifloreæ.
Cactaceæ.	Casteæ.
Valerianaceæ.	Valerianeæ.
Asclepiadaceæ.	Asclepiadeæ.
Gentianaceæ.	Gentianeæ.
Borraginaceæ.	Borragineæ.
Scrophulariaceæ.	Scrophularineæ.
Lentibulaceæ.	Lentibulariceæ.
Plantaginaceæ.	Plantagineæ.
Nyctaginaceæ.	Nyctagineæ.
Lauraceæ.	Laurineæ.
Juglandaceæ.	Juglandeæ.
Salicaceæ.	Salicineæ.
Ceratophyllaceæ.	Ceratophylleæ.

On the other hand, the British authorities are followed in uniting the *Saururaceæ* with the *Piperaceæ*, and also in placing the *Paronychieæ*, reduced to a sub-order under the *Illecebraceæ;* but from the certain relationship of this order with the *Caryophyllaceæ*, it is deemed unnatural to separate these two orders by putting the former into the *Monochlamydeous* division. [See American Naturalist, November, 1878, p. 726.] On the same ground of apparently close relationship, I have followed Bentham and Hooker in abolishing the *Callitrichaceæ*, and placing *Callitriche* in the *Halorageæ*. On the other hand I have followed Gray in retaining the *Lobeliaceæ*, as also in keeping the *Ericaceæ* intact, and not slicing off the *Vacciniaceæ* from one end, and the *Monotropeæ* from the other, as is done in the *Genera Plantarum.*

In the *Gamopetalæ*, before and including *Compositæ*, in the *Monochlamydeæ*, and throughout the *Monocotyledons*, serious difficulties occur in consequence of a want of recent systematic works from the American point of view. In nearly all cases the names as well as the arrangement of Gray's Manual, 5th edition, have here been adopted. I have, however, been able to avail myself of a number of recent revisions of genera made by Gray, Watson, and Engelman* and published in various forms, chiefly in the Proceedings of the American Academy of Arts and Sciences. I have also derived many useful hints from the *Flora of California*, from the botanical reports of the various Western Surveys, from Sargent's Catalogue of the Forest Trees of North America, and from the Flora of Essex county, Massachusetts.

Mr. M. S. Bebb, of Rockford, Illinois, has shown great kindness not only in determining all the uncertain *Salices*, but in generously drawing up a list of them in the order of their nearest natural relationship, which is followed implicitly in the catalogue.

For the Ferns, the magnificent work of Prof. Eaton has furnished everything that could be desired, and is unswervingly adhered to.

The following genera in the *Compositæ* have been changed by Bentham and Hooker, but the new names cannot be adopted until the species have been worked up by American botanists. The old ones are therefore retained with a simple indication of the recent disposition.

Maruta has been made Anthemis.
Leucanthemum has been made Chrysanthemum.
Cacalia has been made Senecio.
Lappa has been made Arctium.
Cynthia has been made Krigia.
Mulgedium has been made Lactuca.
Nabalus has been made Prenanthes.

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* While I have gladly adopted the arrangement of the species of *Quercus* decided upon by Dr. Engelman after so careful a study, I cannot do so without recording a gentle protest against the position to which he assigns *Q. palustris.* viz: between *Q. falcata*, and *Q. nigra*, and far removed from *Q. rubra*. Not only the shallow, finely scaled cup, but especially its light colored buds and thin early leaves, as also a special *facies* belonging to its aments and foliage ally this species with *Q. rubra*, and distinguish these two species as a group from all others found in this flora.

Several of these cases are a return to the older names, and whether they will be adopted by American authorities it is impossible to say.

It remains to consider the one deviation above referred to from the prevailing system of botanical classification, which it has been thought proper to make in the subjoined list of plants. This consists in placing the *Gymnosperms,* here represented only by the single order *Coniferæ,* after the *Monocotyledons* and next to the *Cryptogams.*

It is not the proper place here to state the already well known grounds upon which this position of the Gymnosperms has been defended. [See American Naturalist, June, 1878, pp. 359 to 378.] It is sufficient to point out that the correctness of this arrangement was recognized by Adrien de Jussieu, and has been repeatedly maintained by later botanists of eminence. The object in adopting it here, however, is not simply because it seems fully justified by the present known characters of plants, for consistently to do this would also require that the *Polypetalæ* be placed before the *Monochlamydeæ* (in the descending series,) and that numerous other changes be made. So wide a departure from the existing system would seriously detract from the convenience of the work as a practical aid to the local botanist, and aside from the labyrinth of nice and critical points into which it must inevitably lead, it would not be advisable in the present state of botanical literature. But as the position of the *Gymnosperms* is the most glaringly inconsistent of all the defects of the present so-called Natural System, and as the *Coniferæ* are represented here by only four genera and seven species, it is evident that no serious objection could arise on the ground of inconvenience, while at the same time it may serve some useful purpose in directing the minds of botanists who may look over the work to the obvious rationality of this classification, and contribute its mite towards awakening them to the recognition of a truth which, I cannot doubt, must sooner or later find expression in all accepted versions of the true order of nature with respect to the vegetable kingdom.

Common Names.

I am well aware that in recent times it has become more and more the practice among botanists to eschew all common or popular names of plants. This sentiment I share to a great extent and will therefore remark at the outset that the best common name for a plant is always its systematic name, and this should be made a substitute for other popular names wherever and whenever it can be done. In most cases the names of the genera can be employed with entire convenience and safety; and in many cases they are to be defended on the ground of euphony. How much better, for example, the name *Brunella* sounds than either Self-heal, or Heal-all, both of which latter, so far as their meaning goes, express an utter falsehood. Some works professing to give common names frequently repeat the generic name, as such. This has seemed to me both unnecessary and calculated to mislead. It is not done where other accepted common names exist, and thus the implication is that in such cases it is incorrect to use the Latin name. Again it is only done for the commoner species, leaving it to be inferred that there is no popular way of designating the rarer ones. The plan here followed is to regard the genus as the best name to use in all cases, and as *ex officio* the proper common name of every plant, and, therefore, not in need of being repeated in different type as such in any case. But in addition it has been deemed best to give such appropriate or well established common names as can be found. Some scientific men seem disposed to forget that it is the things rather than the names that constitute the objects of scientific study. There is a vast amount of true scientific observation made by mere school-girls and rustics, who do not know the name of the branch of science they are pursuing. A knowledge of a plant by whatever name or by no name at all is scientific knowledge, and the devotees of science should care less for the means than the end which they have in view. Individuals differ in their constitution and character. The sound or sight of a Latin word is sometimes sufficient, in consequence of ineradicable, constitutional or acquired idiosyncrasies, to repel a promising young man, or woman, from the pursuit of a science for which genuine aptitude and fondness exist. For such and other classes, common English names have a true scientific value. The object should be to inspire a love for plants in all who can be made to take an interest in them, and to this end to render the science of Botany attractive by every legitimate means available. In so far, therefore, as English names of plants can be made conducive to this end, they should be employed. Their inadequacy to the true needs of the science in its later stages cannot fail to impress itself upon all who pursue it to any considerable extent.

Finally common names are not wholly without their scientific uses. A few of them have proved more persistent than any of the systematic names, as I have had occasion to observe in examining the *Prodromus Floræ Columbianæ* of 1838, in which difficult work, I must confess, they frequently rendered me efficient aid in determining the identity of plants, which the Latin names used did not reveal.

In appending common names to the plants of this vicinity *The Native Wild Flowers and Ferns of the United States,* by Prof. Thomas Meehan, has been followed in most cases, so far as this work goes, but this of course embraces but a fraction of the entire flora. Most of the remaining names are taken from Gray's Manual of Botany, and from his Synoptical Flora of the United States. In many cases some of the names given which do not seem appropriate are omitted, and in a few cases those given have been slightly changed. A small number of local names given, not found in any book, but in themselves very expressive, have been given, as "curly head" for *Clematis ochroleuca,* &c.; and in a few other cases, names have been assigned to abundant species on the analogy of those given for allied genera or species.

Concluding Remarks.

The foregoing remarks on the value of common names naturally suggest a few general reflections with which our introduction will conclude.

The popularization of science is now a leading theme of scientific men. To accomplish this, certain branches of science must first become a part of liberal culture. The pursuit of fashion, which is usually regarded as productive solely of evil, may be made an agency of good. If it could become as much of a disgrace to be found ignorant of the flora or fauna of one's native place as it now is to be found ignorant of the rules of etiquette or the contents of the last new novel, devotees of Botany and natural history would immediately become legion, and the woods and fields would be incessantly scoured for specimens and objects of scientific interest. It should be the acknowledged work of educationalists to make science fashionable and call to their aid these powerful social sentiments in demanding the recognition of its legitimate claims.

Of all the natural sciences, that of Botany is the most easily converted into a branch of culture. Its objects appeal directly to the highest esthetic faculties. It naturally allies itself with the arts of drawing, painting, and sketching, and the deeper the insight into its mysteries the stronger does it appeal to the imagination. Its pursuit, besides being the best possible restorer of lost, and preserver of good health, is a perpetual source of the purest and liveliest pleasure. The companionship of plants, which those who do not know them cannot have, is scarcely secoud to that of human friends. The botanist is never alone. Wherever he goes he is surrounded by these interesting companions. A source of pure delight even where they are familiarly known to him, unlike those of his own kind, they grow in interest as their acquaintance grows less intimate, and in all his travels they multiply immensely his resources of enjoyment.

The man of science wonders what the unscientific can find to render travel a pleasure, and it must be confessed that a great many tourists of both sexes go at the behest of fashion, and care little more for nature when crossing the Alps than did Julius Cæsar, who could only complain of the bad roads and while away the hours in writing his grammatical treatise, *De Analogia.* While all forms of natural science, so far from paralyzing the esthetic faculties, tend powerfully to quicken them, that of Natural History and especially of Botany awakens such an interest in Nature and her beautiful objects, that those who have once tasted pleasure of this class may well consider other pleasures insipid.

But notwithstanding these attractions which Botany possesses above other sciences, there exists among a small class of scientific men a disposition to look down upon it as lacking scientific dignity, as mere pastime for school-girls or fanatical specialists. This feeling is most obvious among zoölogists, some of whom affect to disdain the more humble forms of life and the simplicity of the tame and stationary plant.

This sentiment, though now happily rare, is natural and really constitutes what there is left of that proud spirit with which man has ever approached the problems of Nature. His first studies disdained even so complicated an organism as man himself, and spent themselves in the pursuit of spiritual entities wholly beyond the sphere of science. Later he deigned to study *mind* detached from body and from matter, still later he attacked some of the higher manifestations of *life*. Ethics came next, and social organizations; then anthropological questions were opened, and next those of physiology and anatomy, and at last comparative anatomy and structural zoölogy. Phytology brought up the rear and was long confined to the most superficial aspects. It is only in recent times that plants and all the other lowly organisms have begun to receive proper attention, and only since this has been done has there been made any real progress in solving the problem of Biology.

It is a paradox in science that its most complicated forms must first be studied and its simplest forms last, while only through an acquaintance with the latter can a fundamental knowledge be obtained. The history of biological science furnishes many striking illustrations of this truth, the most interesting of which is perhaps to be found in the labors of the two great French savants, Cuvier and Lamarck. The former spent his life and powers in the study of vertebrate zoölogy amid the most complex living organisms. The latter devoted his energies to Botany and to Invertebrate Zoölogy, including the protozoan and protistan kingdoms. The former founded his great theory of types, and his cosmology of successive annihilation and reconstructions of the life of the globe. The latter promulgated his theory of unbroken descent with modification. The conclusions of the former were accepted in his day, and are rejected in ours, those of the latter were rejected in his own lifetime, but now form the very warp of scientific opinion.

Let no botanist, therefore, or person contemplating the study of Botany be deterred by the humble nature of the objects he would cultivate. The humblest flower or coarsest weed may contain lessons of wisdom more profound than can be drawn from the most complicated conditions of life or of mind.

The city of Washington is becoming more and more a center, not only of scientific learning and research, but also of art and every form of liberal culture. Already the public schools have reached out and taken Botany into their curriculum, and we have seen that as a field for the pursuit of this branch of science the environs of the National Capital are in a high degree adapted. Science and culture must go hand in hand. Culture must become more scientific, and science more cultured. Botany has an important part to perform in this work of reconciliation, and there is no good reason why Washington may not become one of the foci from which these influences are to radiate. It has been such reflections as these, aside from the practical needs for such a work, that have encouraged me to persevere in this humble, indeed, but not the less laborious task, and if it shall be found useful to however slight a degree, in promoting these worthy objects, no regrets will arise at having undertaken it.

SUMMARY.

No.	ORDERS.	Genera.	Species.	Varieties.	Species and Varieties.	Introduced Plants.	Woody Plants.	Trees.
1	Ranunculaceæ	7	23	4	27	3	...	...
2	Magnoliaceæ	2	2	...	2	...	2	2
3	Anonaceæ	1	1	...	1	...	1	1
4	Menispermaceæ	1	1	...	1	...	1	...
5	Berberidaceæ	4	4	...	4	1	1	...
6	Nymphæaceæ	3	3	...	3	...	...	...
7	Sarraceniaceæ	1	1	...	1	...	...	...
8	Papaveraceæ	3	3	...	3	2	...	...
9	Fumariaceæ	3	3	...	3	1	...	...
10	Cruciferæ	16	32	1	33	15	...	...
11	Cistaceæ	2	2	...	2	...	...	...
12	Violaceæ	2	9	5	14	...	...	...
13	Polygalaceæ	1	7	...	7	...	...	...
14	Caryophyllaceæ	9	19	...	19	8	...	...
15	Illecebraceæ	2	2	1	3	...	...	...
16	Portulacaceæ	2	2	...	2	1	...	...
17	Hypericaceæ	3	9	...	9	1	1	...
18	Malvaceæ	4	7	...	7	5	...	...
19	Tiliaceæ	1	1	...	1	...	1	1
20	Linaceæ	1	3	...	3	1	...	...
21	Geraniaceæ	4	9	...	9	3	...	...
22	Rutaceæ	2	2	...	2	1	2	...
23	Ilicineæ	1	4	...	4	...	4	1
24	Celastraceæ	2	3	1	4	...	4	...
25	Rhamnaceæ	1	2	...	2	...	2	...
26	Vitaceæ	2	6	...	6	...	6	...
27	Sapindaceæ	3	5	...	5	...	5	4
28	Anacardiaceæ	1	6	...	6	...	6	1
29	Leguminosæ	24	55	2	57	13	4	3
30	Rosaceæ	15	43	3	46	12	30	8
31	Saxifragaceæ	8	9	...	9	3	5	...
32	Crassulaceæ	2	3	...	3	...	...	...
33	Droseraceæ	1	1	...	1	...	...	...
34	Hamamelaceæ	2	2	...	2	...	2	1
35	Halorageæ	3	3	...	3	...	...	...
36	Melastomaceæ	1	1	...	1	...	...	...
37	Lythraceæ	4	4	...	4	...	...	...
38	Onagraceæ	6	10	1	11	...	...	...
39	Passifloraceæ	1	2	...	2	1	...	...

SUMMARY.—*Continued.*

No.	ORDERS.	Genera.	Species.	Varieties.	Species and Varieties.	Introduced Plants.	Woody Plants.	Trees.
40	Cucurbitaceæ	1	1	...	1	...	...	...
41	Cactaceæ	1	1	...	1	...	...	...
42	Ficoideæ	1	1	...	1	...	...	...
43	Umbelliferæ	17	22	...	22	2		...
44	Araliaceæ	1	4	...	4	...	1	1
45	Cornaceæ	2	5	...	5	...	5	2
46	Caprifoliaceæ	5	12	...	12	3	10	1
47	Rubiaceæ	5	12	1	13	...	1	...
48	Valerianaceæ	2	4	...	4	1	...	...
49	Dipsaceæ	1	1	...	1	1	...	...
50	Compositæ	53	138	11	149	17	1	...
51	Lobeliaceæ	1	5	...	5	...	...	...
52	Campanulaceæ	2	2	...	2	...	...	...
53	Ericaceæ	11	24	2	26	...	17	2
54	Primulaceæ	5	8	2	10	2	...	...
55	Ebenaceæ	1	1	...	1	...	1	1
56	Oleaceæ	2	4	...	4	...	4	4
57	Apocynaceæ	2	2	1	3	1	...	...
58	Asclepiadaceæ	4	13	1	14	...	...	...
59	Gentianaceæ	4	6	...	6	...	...	...
60	Polemoniaceæ	2	6	...	6	...	...	...
61	Hydrophyllaceæ	3	4	...	4	...	...	...
62	Borraginaceæ	7	12	...	12	3	...	...
63	Convolvulaceæ	3	11	...	11	4	...	...
64	Solanaceæ	5	8	...	8	5	...	...
65	Scrophulariaceæ	15	32	...	32	5	...	...
66	Orobanchaceæ	4	4	...	4	1	...	...
67	Lentibulaceæ	1	2	...	2	...	...	...
68	Bignoniaceæ	2	2	...	2	1	2	1
69	Acanthaceæ	2	3	1	4	...	...	...
70	Verbenaceæ	3	6	...	6	1	...	...
71	Labiatæ	23	41	1	42	10	...	...
72	Plantaginaceæ	1	5	1	6	2	...	...
73	Amarantaceæ	2	5	...	5	4	...	...
74	Chenopodiaceæ	3	7	2	9	7	...	...
75	Phytolaccaceæ	1	1	...	1	...	...	...
76	Polygonaceæ	3	21	2	23	7	...	...
77	Podostemaceæ	1	1	...	1	...	...	...
78	Aristolochiaceæ	2	2	...	2	...	...	...
79	Piperaceæ	1	1	...	1	...	...	...
80	Lauraceæ	2	2	...	2	...	2	1
81	Thymelaceæ	1	1	...	1	...	1	...
82	Santalaceæ	1	1	...	1	...	...	...
83	Loranthaceæ	1	1	...	1	...	1	...
84	Euphorbiaceæ	4	9	...	9	1	...	...
85	Urticaceæ	11	13	...	13	4	6	6
86	Platanaceæ	1	1	...	1	...	1	1
87	Juglandaceæ	2	7	...	7	...	7	7
88	Myricaceæ	1	1	...	1	...	1	...

SUMMARY.—*Continued.*

No.	ORDERS.	Genera.	Species.	Varieties.	Species and Varieties.	Introduced Plants.	Woody Plants.	Trees.
89	Cupuliferæ	7	25	1	26	...	26	23
90	Salicaceæ	2	14	5	19	7	19	6
91	Ceratophyllaceæ	1	1	...	1	...	...	...
92	Araceæ	5	6	...	6	...	...	...
93	Lemnaceæ	1	1	...	1	...	...	...
94	Typhaceæ	2	3	1	4	...	...	...
95	Naiadaceæ	2	9	...	9	...	...	...
96	Alismaceæ	2	3	2	5	...	...	...
97	Hydrocharidaceæ	2	2	...	2	...	...	...
98	Orchidaceæ	12	23	1	24	...	...	...
99	Amaryllidaceæ	1	1	...	1	...	...	...
100	Haemodoraceæ	1	1	...	1	...	...	...
101	Iridaceæ	2	6	...	6	1	...	...
102	Dioscoreaceæ	1	1	...	1	...	...	...
103	Smilaceæ	1	6	...	6	...	4	...
104	Liliaceæ	18	24	...	24	5	...	...
105	Juncaceæ	2	8	7	15	...	...	...
106	Pontederiaceæ	3	3	...	3	...	...	...
107	Commelynaceæ	2	3	...	3	...	...	...
108	Xyridaceæ	1	1	...	1	...	...	...
109	Eriocaulonaceæ	1	1	...	1	...	...	...
110	Cyperaceæ	10	94	14	108	...	...	...
111	Gramineæ	43	104	6	110	26	...	...
112	Coniferæ	4	7	...	7	1	7	7
113	Equisetaceæ	1	2	...	2	...	...	...
114	Filices	16	29	1	30	...	...	...
115	Ophioglossaceæ	2	2	2	4	...	...	...
116	Lycopodiaceæ	2	5	1	6	...	...	...
117	Musci	42	98	...	98	...	...	...
118	Hepaticæ	23	29	...	29	...	...	...
119	Characeæ	2	4	...	4	...	...	...

RECAPITULATION.

Groups.	Orders.	Genera.	Species.	Varieties.	Species and Varieties.	Introduced Plants.	Woody Plants.	Trees.
Polypetalæ	45	174	338	18	356	73	83	25
Gamopetalæ	27	169	368	21	389	57	36	9
Dichlamydeæ	72	343	706	39	745	130	119	34
Monochlamydeæ	19	47	114	10	124	30	64	44
Dicotyledones	91	390	820	49	869	160	183	78
Monocotyledones	20	112	300	31	331	32	4	----
Gymnospermæ	1	4	7	----	7	1	7	7
Phænogamia	112	506	1,127	89	1,207	193	194	85
Vascular Cryptogamia	4	21	38	4	42	----	----	----
Vascular Plants	116	527	1,165	84	1,249	193	194	85
Cellular Cryptogamia	3	67	131	----	131	----	----	----
Total Flora	119	594	1,296	84	1,380	193	194	85

On this communication, Mr C. A. White remarked that he hoped Mr. Ward would be able to furnish some further information concerning the influence exerted upon a flora by the character of the country rocks. It is well known that the constitution of the strata, influencing as it does the character of the soils which cover them, had a further effect upon the native plants growing above them. Thus the granite localities of the east were more favorable to the growth of certain genera, for example, the Ericaceæ than the magnesian limestones of the Mississippi valley. He hoped that Mr. Ward might be able to ascertain how far these influences affected other families of plants.

Mr. Powell inquired what were the characters or character of plants that had apparently disappeared from the local flora in the comparison of the field results of the present time with those obtained forty or fifty years ago.

Mr. Ward replied that the missing species in the present lists were not confined to any particular family, but were diffused considerably among the several classes.

The Society then adjourned.

193d Meeting. February 5th, 1881.

Vice President Welling in the Chair.

Thirty-eight members present.

The minutes of the last meeting were read and adopted.

A communication was then read by Mr. C. E. Dutton, on

THE SCENERY OF THE GRAND CANON DISTRICT.

The communication was reserved by the author.

Remarks upon this communication were made by Mr. J. W. Powell, at the conclusion of which, the Society adjourned.

194th Meeting. February 19th, 1881.

Vice President Taylor in the Chair.

Thirty-one members present.

The minutes of the last meeting were read and adopted.

The President announced to the Society the death of Dr. George A. Otis. It was moved and carried, that a committee be appointed to prepare suitable resolutions for the action of the Society, relative to the death of Dr. Otis, and the Chair appointed a committee consisting of Messrs. Antisell, Billings, and Mew.

The first communication for the evening was by Mr. J. E. Todd, of Iowa who had been invited by the General Committee to read a communication on the

QUARTERNARY DEPOSITS OF WESTERN IOWA AND EASTERN NEBRASKA.

Mr. Todd gave first an account of the three members which compose the Quarternary deposits of the regions in questions. The lowest is in Iowa, and is the boulder-clay consisting of the hard compact clay usually occurring in this formation, with its included rocky glaciated fragments. In central and western Nebraska this clay is wanting. Upon it rests the red clay, a formation of varying thickness, but usually quite thin, rarely exceeding 20 feet. Upon this rests the *loess* which constitutes a subject of special interest. One peculiarity of it is found in the fact, that it overlies the inequalities of the country which existed prior to its disposition; being found upon the old hill tops and slopes, as well as in the valley bottoms, and exhibiting a general "unconformity by erosion." It is composed of exceedingly fine matter without any fragments of rock of notable size, such as pebbles or stones. It contains, however, bands of calcareous concretions in lines which are usually horizontal, and these concretions are often elongated with their longer dimensions vertical. It also holds those calcareous fibres which Richthofen observed in the *loess* deposits of China, and which he believed to be casts of roots of plants. Another interesting occurrence is that of charcoal, which is found in several places in the midst of the deposits in thin bands. The fossils of the loess are the shells of geophilous mollusca.

Mr. TODD held the view that the loess is a post-pliocene lacustrine deposit, and that the region in discussion was in post-glacial time covered with a very large fresh-water lake.

Prof. T. C. CHAMBERLAIN, of Wisconsin, being present, and invited to take part in the discussion, remarked that while Mr. Todd had presented in a very able and clear manner the reasons for attributing the loess to the deposit of silt in a lake bottom, he was of opinion that the objections to the acceptance of that view were very great. If such a lake existed over the region in question during quarternary time, it must have been of immense extent. According to the observations of Dr. C. A. White, these deposits extend to the borders of the region which drains immediately into the Mississippi river in Iowa, and they are found nearly as far west as the Rocky Mountains. Their north and south extensions are not accurately known, but they are believed to be very great. Independently of these deposits no evidences of such a lake are now known. Its boundaries are not marked by any known barriers on the east where the configuration of the country is now such that no barriers could have existed, unless the region which they should have occupied has undergone remarkable changes of which the nature cannot be specified, and of which no traces exist. To produce such a lake basin very great depressions would be necessay, and there is no evidence known to him which warrants a belief in a former depressed condition of that region sufficient to account for it. Further research may indeed relieve us of some of these difficulties or all of them, but at present they are very great. Prof. Chamberlain could not but commend, however, the earnest and scientific spirit in which Mr. Todd had pursued his valuable investigations.

Mr. O. T. Mason inquired whether the occurrences of charcoal were frequent and bore evidence of human agency.

Mr. Todd replied that charcoal was often met with, and suggested as a possible, though not probable, explanation, that the fragments may have come from some of the recent volcanic regions of the west.

Mr. C. E. Dutton suggested that there would be little difficulty in finding a natural cause for the occurrence of charcoal, if the surface had been above water at the time it was deposited. There can be little doubt that fires are frequently started in the woods and on the plains of the west by lightning, and it is not at all incredible that they may sometimes arise from spontaneous ignition. Many of the frequent fires in the western mountains occur under circumstances which render it incredible that human agency was involved.

Mr. C. A. White spoke of the great areas over which loess deposits are found. They occur not only in the upper Mississippi valley, but also in the regions of the lower Mississippi. They also occupy a great range of altitudes, some being only a few hundred feet above the level of the sea, others several thousand feet above it. They all seem to be of similar character and constitution. The absence of any barriers is one powerful argument against the existence of a lake, and the great changes of level which would be demanded to establish this hypothesis is another.

The next communication was read by Mr. C. E. Dutton, on

THE VERMILION CLIFFS AND VALLEY OF THE VIRGEN, IN SOUTHERN UTAH.

The paper was reserved by the author.

At its conclusion the Society adjourned.

195th Meeting. March 5th, 1881.

Vice-President Taylor in the Chair.

Twenty-two members present.

The minutes of the last meeting were read and adopted.

The Chair announced the election of Mr. Peter Winfield Lauver to membership in the Society.

The first communication was by Mr. THEODORE GILL on the

PRINCIPLES OF MORPHOLOGY.

Mr. Gill's paper may be föund substantially in Johnson's Encyclopœdia, under the title Morphōlogy, which article was written by him.

The second communication was by Mr. MARCUS BAKER on the

BOUNDARY LINE BETWEEN ALASKA AND SIBERIA.

The present boundaries of the territory of Alaska were defined in the treaty of March 30, 1867, whereby Russian America was ceded to the United States. In that treaty the western boundary, or rather so much of it as is here considered, was defined as follows:

"The western limit, within which the territories and dominion conveyed are contained, passes through a point in Behring's Straits on the parallel of sixty-five degrees thirty minutes north latitude, at its intersection by the meridian which passes midway between the island of Krusenstern or Ignalook, and the island of Ratmanoff or Noonarbook, and proceeds due north without limitation into the same Frozen Ocean."

The longitude of this meridian was very properly left out of the treaty on account of its uncertainty. In order to show our knowledge of the subject at the time of the framing of the treaty the following table has been prepared from all known authorities upon the subject down to the present time.

The last three determinations entered in the table, it must be borne in mind, have been made since the treaty was drawn up.

Date.	Longitude.		Authority.
	°	′	
1761	155		Map published by the Imp. Acad. of Sc. of St. Petersb.
1778	169	52	Cook's Atlas.
1802	168	48	Billings.
1822	168	59	Kotzebue.
1827	168	55	Beechey. Br. Adm. Ch. No. 593.
1828	168	54	Lütke's Atlas.
1849	168	57.5	Tebenkoff's Atlas.*
1852	168	54	Russian Hydr. Ch. No. 1455.
1855	168	48	Rogers. U. S. Hyd. Ch. No. 68.
1874	169	04	Russ. Hyd. Ch. No. —.*
1878	168	58	Onatsevich.
1880	168	58	U. S. C. and G. S.

In the case of the two determinations marked with a * the two Diomede Islands are so represented on the chart that the boundary line is tangent to each island.

During the past summer an attempt was made by the party on board the U. S. C. and G. S. Schooner Yukon to make a more careful determination of the longitude of this meridian than had been attempted hitherto. For longitude purposes the party had one pocket and six box chronometers. For determining time the sextant was used, recourse being had to equal altitudes whenever possible.

Plover Bay in Eastern Siberia is about 150 miles to the southward and westward from the Diomede Islands in Behring's Strait. This bay was visited by Prof. Asaph Hall of the U. S. Naval observatory in 1869 for the purpose of observing the total solar eclipse of that year, and, in connection with the eclipse work, Prof. Hall made a careful determination of the longitude of his station. After a careful examination of all the longitude determinations known to exist, and because the facilities for determining the longitude of this place by the Yukon party were not sufficient to improve upon the determination by Prof. Hall, his results have been adopted, and the longitude of the boundary meridian made to depend upon his determination. Before proceeding to give an account of our longitude observations, when near the boundary line, a complete *resumê* of observations for position at Plover Bay, with discussion will be given, this being rendered necessary by the fact that the longitude of the boundary line as well as that of all other points along the Arctic coast and northern part of Behring Sea have been made by us to depend upon Plover Bay.

Previous to 1848 Plover Bay, though an extensive arm of the sea running inland some 20 to 25 miles, appears not to have been known. It is not shown upon any map before 1850. In the period from 1845 to 1848 it seems to have been visited by the whalers. The first information touching it upon which we can lay our hands is the report of Commander Moore to the Admiralty, published in the Nautical Magazine March, 1850. From this it appears that Commander Moore first anchored in Plover Bay, October 17, 1848 Later he moved his vessel, the Plover, farther in, and wintered in the harbor named by him Emma Harbor. He remained in Emma Harbor until June 23, 1849. Concerning the scientific or surveying work accomplished in this period of eight months, he says; "At intervals Mr. Martin, assisted by Mr. Hooper, made a survey of the place in which I had secured the ship for the winter; which, connected with Mr. Martin's and my own observations on the coast to the westward, will, I hope, give a tolerably correct representation of these shores, and when associated with magnetic observations on every attainable point, will, I trust meet their Lordships' approbation."

The results foreshadowed by this report have not come to light. No map or plan of Emma Harbor, or Plover Bay, has been published by the British Admiralty Office, and no statement or account of the observations at Plover Bay, if any were made. General Sabine in his contributions to Terrestial Magnetism No. XIII gives some results which he credits to a MS in the Magnetic Office by Commander Moore, but no *magnetic declination* or *intensities* are given; whence we conclude that no observations, or at least no satisfactory observations, therefor, were taken. A few results for *dip* are given. The geographical position of the station where the dip observations were taken is given by General Sabine, and this position, if due to Commander Moore, is the earliest determination on record of a position for Plover Bay. The position given probably refers to some point near the northern shore of Emma Harbor and is

Latitude,	64°	26′	N.
Longitude,	173	07	W. Gr.

and the observed dip was 75° 10′. From the best existing chart of Plover Bay that we have, it is found that this station is four minutes north, and nine minutes east of the station occupied by the Coast Survey. Whence we find the Coast Survey Astronomical Station to be, according to Commander Moore, approximately in

Latitude,	64°	22′	N.
Longitude,	173	16	W. Gr.

A rough sketch of Plover Bay was made in 1866, by the exploring parties of the Western Union Telegraph Company, and this sketch was published in 1869 by the Coast Survey. The observations were made by Lieut. J. Davison, of the U. S. Revenue Marine Service, and the resulting position is stated to depend upon nine observations referred by a crude triangulation to the mountain Bald Head. The position given by Lieut. Davison for Bald Head is

Latitude,	64°	24′	N.
Longitude,	173	15	W. Gr.

From the best chart extant of Plover Bay, which has been referred to above, and which is one published in 1877 by the Russian Hydrographic Office from surveys by Lieut. Onatsevich, we find Bald Head to be one and a half minutes south and one minute east of the Coast Survey Astronomical Station. Hence, according to Lieut. Davison, the Coast Survey Astronomical Station is in

Latitude, 64° 25.′5 N.
Longitude, 173 16 W. Gr.

As the observations were made, *not* on the mountain, but on the vessel at anchor in the harbor, it seems probable that in transferring the position of the vessel to the mountain some mistake occurred, for the resulting latitude is certainly considerably in error.

The next determination of position at Plover Bay was by Prof. Hall, in 1869, during his visit to this place to observe the total solar eclipse of that year. The latitude was determined with a Pistor and Martin's sextant from observations upon August 3, 4, and 5, by Prof. Hall and Mr. J. A. Rogers. The following table gives the results:

Date.	Latitude. ° ′ ″ ″	Observer.	No. of Observations.
1869, August 3	64 22 22 ± 1.3	Rogers	15
" 3	22 ± 1.9	Rogers	14
" 4	33 ± 1.9	Hall	17
" 5	27 ± 1.9	Hall	12
" 5	20 ± 2.7	Hall	12
Mean adopted	64 22 25		70

For determining the longitude Prof. Hall had ten chronometers whose corrections to Greenwich time were determined at the Astronomical Station in the Navy Yard on Mare Island, California, before setting out and returning from Plover Bay. The dates of the time determinations at Mare Island, are June 17–20, and September 18–19, 1869, the interval being 102 days. The time was determined with a small portable transit instrument. With these means Prof. Hall obtained the following results for the longitude of his station in Plover Bay, west from the station at Mare Island.

h.	m.	s.
3	24	21.3
		19.1
		21.3
		21.0
		22.7
		22.2
		22.5
		15.9
		23.0
		21.1

These are the results by each chronometer, and when combined by weights indicated by their probable errors, the resulting longitude is

$$3^{h}\ 24^{m}\ 21^{s}.1 \pm 0^{s}.36$$

Since these results were published, the longitude of San Francisco has been determined by telegraph, and the station upon Mare Island occupied by Prof. Hall geodetically connected with this determination. The resulting longitude of the Mare Island station is, according to Assistant Schott of the Coast Survey,

$$122^{\circ}\ 16'\ 16'' \pm 2''.2$$

or, in time,

$$8^{h}\ 09^{m}\ 05^{s}.07 \pm 0^{s}.15$$

whence we have for the longitude of Prof. Hall's station, at Plover Bay

$$11^{h}\ 33^{m}\ 26^{s}.2 \pm 0^{s}.4.$$

For Prof. Hall's station, therefore, we adopt

Latitude,	64°	22′	25″	N.
Longitude,	173	21	33 ± 6″	W. Gr.

Before leaving Washington we were furnished by Prof. Hall with a memorandum, describing his station from which it appears that no permanent station mark could be left by him, the character of the soil and natives preventing this. We were, therefore, unable to locate the exact spot, but had no difficulty in finding the general locality, and fixing upon a place that must have been within a few metres of Prof. Hall's station. Here we erected a pile of boulders as a beacon, and by means of the telemeter staff, and a small triangulation connected with our azimuth line, we found this beacon to bear N. 1° 42′ 26″ E. from our astronomical station, and 462.9 metres distant, or in round numbers 460 metres N. 1° 42′ E. of ours; in arc this is 1″ E. and 15″ N. of ours. Applying these reductions to the position already adopted, we have as the position of our station, according to Prof. Hall

Latitude,	64°	22′	10″	N.
Longitude,	173	21	32 ± 6″	W. Gr.

In 1876 the bay was visited by Lieut. M. L. Onatsevich, of the Russian Navy in the "*Vsadnik*," and a rough survey made of the bay with a somewhat detailed survey of the anchorages. At the same time astronomical and magnetic observations were made.

In 1877, the Russian Hydrographic Office published several charts embodying the results of Onatsevich's observations, and among them, a chart of Port Providence, or "Plover Bay," as it is usually called by the whalemen. On this chart it is stated that the astronomical station of Lieut. Onatsevich is, according to his observations in

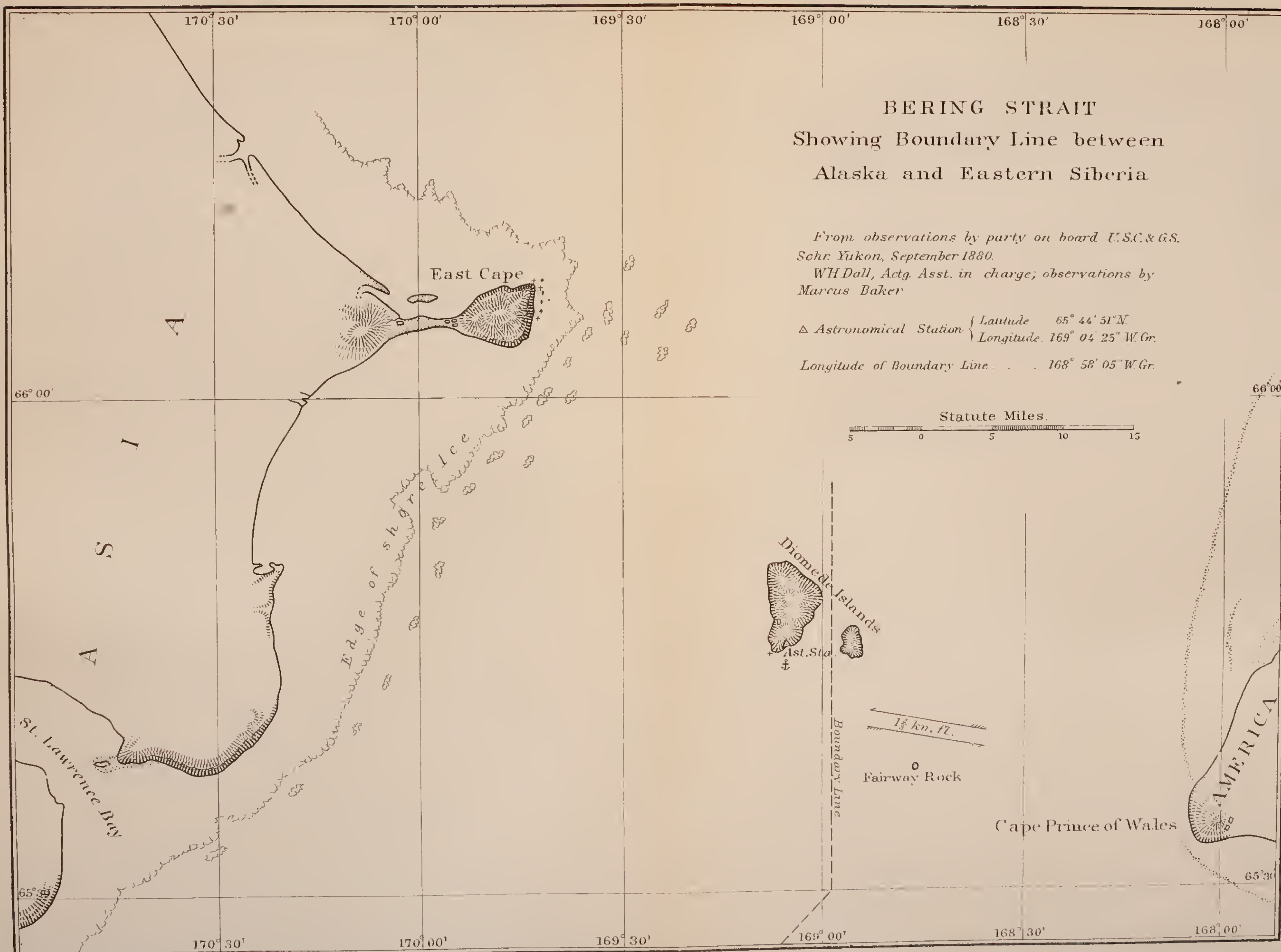
Latitude,	64°	21′	37″	N.
Longitude,	173	18	30	W. Gr.

In the following year, however, 1878, Lieut. Onatsevich's report was published, and in this report the position of the astronomical station is stated to be

Latitude,	64°	21′	55″	N.
Longitude,	173	23	54	W. Gr.

the longitude depending upon that of Petropavlovsk, which latter is taken as 10h. 34m. 37s. or 158° 39′ 15″ E. from Greenwich. This last result appears to be the finally corrected one, and is adopted as Onatsevich's determination.

The station occupied by Lieut. Onatsevich is clearly marked upon his chart, and as we had this chart with us the place was quite closely identified, probably within a few feet. The attempt was made to have our station identical with his, and consequently no reduction is necessary.



BERING STRAIT
Showing Boundary Line between
Alaska and Eastern Siberia
From observations by party on board U.S.C.&G.S. Schr. Yukon, September 1880.
W.H. Dall, Actg. Asst. in charge; observations by Marcus Baker
△ Astronomical Station { Latitude 65° 44' 51" N. Longitude 169° 04' 25" W. Gr.
Longitude of Boundary Line . . . 168° 58' 05" W. Gr.
Statute Miles.
5 0 5 10 15
170° 30'
170° 00'
169° 30'
169° 00'
168° 30'
168° 00'
66° 00'
65° 30'
ASIA
East Cape
Edge of shore Ice
St. Lawrence Bay
Diomede Islands
Ast. Sta.
Boundary Line
1½ kn. fl.
Fairway Rock
Cape Prince of Wales
AMERICA

Recapitulating, therefore, we have the following results for the position of the Coast Survey Astronomical Station at Plover Bay:

Date.	Latitude.			Longitude.			Authority.
	°	′	″	°	′	″	
1848–9	64	22		173	16		Com'r T. E. L. Moore. (?)
1866		25.5			16		Lieut. J. Davison.
Aug., 1869		22	10		21	32	Prof. A. Hall.
July, 1876		21	55		23	54	Lieut. M. L. Onatsevich.
Sept., 1880		21	54	----			U. S. C. and G. S., by M. Baker.

Discussion of foregoing Table.

It is very doubtful whether the results credited to Commodore Moore were really obtained by him, or whether General Sabine took these values from other sources; while the results by Lieut. Davison are known to have been of only a very approximate character. The three remaining results for latitude, when we consider that they were made at different times, by different observers, at different stations, and with different instruments and the instruments of a secondary character, show a satisfactory agreement, and we adopt the simple mean for the latitude determination, which is 64° 22′ 00″ and would assign an arbitrary probable error of 6″.

Neglecting the longitude results by Moore and Davison as being of an inferior character, we have the two remaining by Hall and Onatsevich. The determination by Onatsevich is a chronometric one from Petropavlovsk. How the longitude of Petropavlovsk was obtained we are not informed, but we know it was not determined by telegraph. Moreover the longitude adopted by Onatsevich for Petropavlovsk differs by as much as four miles, (4′ 11.7″ = 16.8*s*) from that adopted by the Russian Hydrographic Office, in 1850, as the basis for their charts of this region, and which determination was the mean of nine different determinations extending from 1779 to 1827. The longitude of Plover Bay based upon Onatsevich's observations and that longitude of Petropavlovsk is 173° 19′ 22″ W. Gr.

It has, therefore seemed best to adopt without change the result of Prof. Hall's observations, not combining it with anything else, viz: 173° 21′ 32″ ± 6″ W. Gr.

Our adopted value, therefore, of the geographical position of the Astronomical Station of the U. S. Coast and Geodetic Survey at Plover Bay, Eastern Siberia, is

Latitude,	64°	22′	00″ ± 6″		N.
Longitude,	173	21	32 ± 6		W. Gr.
	h.	m.	s.	s.	
	11	33	26.1 ± 0.4		

One station was marked by driving a piece of whale's rib into the ground and piling rocks around it. Being identical with the station of Lieut. Onatsevich, any one visiting the place will by the aid of that chart readily identify it.

Having completed our investigation of the geographical position of Plover Bay, we proceed to detail our observations for the longitude of the boundary.

The Yukon arrived at Plover Bay at ten in the evening of August 11, 1880. The following day was cloudy in the morning, afterward rained, and later partially cleared up so that we obtained two pairs of equal altitudes of the sun for time, the interval being about three hours. During the afternoon we succeeded in getting four sets of six each of double altitudes of the sun for time. From the equal altitudes the time of local mean noon by the chronometer, was 11h. 18m. 13.9s, and from the double altitude it was 11h. 18m. 14.2s., a very satisfactory agreement. By means of the intervals the probable errors of each of these determinations have been made out. For the equal altitudes it is ± 1.7s, and for the double altitudes it is ± 0.30s, values which may be taken as fairly representative of the different conditions under which the observations were made. From these observations the corrections of our chronometers to Greenwich mean time on August 12 were determined.

On August 14, we sailed from Plover Bay to the eastward and northward, cruising along the Arctic coast as far as Point Belcher, and returning thence passed through Behring Strait to Port Clarence, and afterwards returning to Behring Strait made a landing on the southeastern shore of Ratmanoff, or the Big Diomede Island, on September 10. We came to anchor at seven in the morning, about a mile off shore, and sailed away about three in the afternoon. During our stay observations were made for latitude and time, and all the magnetic elements, declination, dip and intensity. Of time observations three sets of six each of double altitudes of the sun were obtained with sextant and artificial horizon. These three sets give as the correction of our "hack," or observing chronometer, to local mean time

h.	*m.*	*s.*	*s.*
+ 1	03	26.9	± 0.35,

this probable error resulting from computing the eighteen observations singly and treating in the usual way. The sky was nearly covered with cumulus clouds, the wind fresh, raw and chilly, and thermometer 39° F. Near noon the sun appeared again for a short time, and nine pointings were obtained for latitude, giving the following results, each depending upon a single observation.

	65°	44′	54″
			50
			38
			54
			44
			52
			53
			60
			65
Mean latitude,	65°	44′	51 ± 1.″5 N.

Leaving the Diomedes on the afternoon of September 10, we sailed directly for Plover Bay. That night we were stopped by ice, the next day delayed by calms, but on the following day, September 12, we reached our anchorage in Plover Bay a little before noon, just in time to get a good series—39 observations of circummeridian altitudes of the sun for latitude. In the afternoon we obtained a good series of time observations, but the following morning was cloudy. We succeeded, however, in getting four altitudes corresponding to those of the preceding day, thus enabling our time determination to hang upon four pairs of equal altitudes, the epoch being local mean midnight September 12 and 13. The times of local apparent midnight from these four pairs by our "hack" were

h.	*m.*	*s.*
11	09	0.2
		1.2
		0.3
		0.7

from which the probable error is found to be ± 0.15*s*.

For the longitude of our station upon the Big Diomede Island we have, therefore, as follows:

Plover Bay	1880, Aug. 12, noon	Chron'r corr'n determined,		± 1.7 *s.*
Big Diomede Id.,	" Sept. 10, 8.9 *h.* a. m.,	"	"	± 0.35
Plover Bay	" " 12, midnight	"	"	± 0.15

By means of the time determinations of August 12 and September 12, the rates of the chronometers are determined and then the Greenwich time determination at Big Diomede Island, September 10, is made to depend upon the determination at Plover Bay, September 12, and the rates of all the chronometers carried back to September 10, a period of 2.64 days.

The resulting longitude by each chronometer is shown in the following table:

Chron'r.	*h.*	*m.*	*s.*
214	11	16	18.3
866			17.9
1131			18.0
1713			19.0
2535			14.7
311			16.6

Chronometer No. 2535 was our "hack," and 311 a sidereal chronometer used in making comparisons. Each had rather large rates, that of 2535 exceeding *nine* seconds, and that of 311 *five* seconds per day. The indiscriminate mean of all is 11*h.* 16*m.* 17.4*s.* Assigning only half weight to chronometer 2535, the longitude resulting is

h.	*m.*	*s.*
11	16	17.7

The probable error of the Greenwich time at the Diomedes, based upon the agreement of the chronometer is ± 0.36*s.*

For the probable error of the longitude, therefore, we have

Probable error of longitude of Plover Bay	= ± 0.39 *s.*
Probable error local time determination, Plover Bay, Sept. 12	= ± 0.15
Probable error local time determination, Diomedes, Sept. 10	= ± 0.35
Probable error Greenwich time determination, Diomedes, Sept. 10	= ± 0.36

	h.	*m.*	*s.*	*s.*
Resulting longitude adopted,	11	16	17.7	± 0.65.

The astronomical station of the United States Coast and Geodetic

Survey at the mouth of the ravine, on the southeastern shore of the Big Diomede Island, in Behring Strait, is, therefore, in

Latitude,	65°	44′	51″		N.
Longitude,	169	04	25	± 10″	W. Gr.

From bearings and angles taken from the astronomical station and from the schooner at anchor, using the distance of the schooner from the station as a base line, together with other bearings taken while in the vicinity of the islands, a sketch of the two islands has been prepared from which it appears that the meridian tangent to the extreme eastern edge of the larger island is 2.1 nautical miles, and the meridian tangent to the extreme western edge of the smaller island is 3.1 nautical miles, east of the astronomical station. The boundary line is to pass midway between these meridians, *i. e.* the meridian which forms the boundary is 2.6 nautical miles east of the astronomical station.

In latitude 65° 45′, the latitude of the astronomical station, 2.6 nautical miles is equal to 6′ 20″ of longitude, and, deducting this from the longitude of the astronomical station, the longitude of the boundary line is found to be

168° 58′ 05″ W. Gr.

If we assume an uncertainty of one quarter of a nautical mile, equal in this latitude to 37″ of longitude, in thus transferring the position of the station to the boundary line, and this seems to be quite large enough, we have finally as the longitude of the boundary line between Alaska and Eastern Siberia

°	′	″	″
168	58	05	± 38

or, in time,

h.	*m.*	*s.*	*s.*	
11	15	52.3	± 2.5	W. Gr.

Errata.

Page 126, line 6 from bottom, *for* "and returning" *read* "and after returning."

For "Behring," *read* "Bering," throughout this article.

196TH MEETING. MARCH 19, 1881.

Vice-President TAYLOR in the Chair.

Thirty members and visitors present.

The minutes of the last meeting were read and adopted.

The communication for the evening was by Mr. J. W. POWELL, on

LIMITATIONS TO THE USE OF SOME ANTHROPOLOGIC DATA.

This paper is published in full in the "Abstract of Transactions of the Anthropological Society of Washington, D. C., for the first year ending January 20, 1880, and the second year ending January 18, 1881."

Remarks upon this communication were made by Messrs. GILL, HARKNESS, WARD, NEWCOMB, and ALVORD.

At the conclusion of the discussion the Society adjourned.

197TH MEETING. APRIL 2D, 1881.

Vice-President TAYLOR in the Chair.

Thirty-nine members and visitors present.

The consideration of the minutes of the last meeting was postponed, the recorder being absent.

Dr. ANTISELL, on behalf of the committee appointed at the last meeting of the Society, reported the following resolution in commemoration of the late Dr. GEORGE A. OTIS:

Resolved, That this Society has heard with profound regret of the untimely death, on the 23d of February last, of Dr. GEORGE A. OTIS, U. S. Army, one of its original founders.

Resolved, That while we deplore the loss of so highly valued an associate and friend, there is some compensation to be found in the reflection that his long and incessant suffering has at last terminated, and that it is gratifying to remember that he was not cut off before his services to science, in his chosen field, had received, as well in Europe as in America, the high appreciation which they so richly merited.

Resolved, That the medical literature, not only of this country but of the world, has sustained by this calamity a loss which can with difficulty be replaced.

A communication was then read by Mr. A. B. Johnson on

THE HISTORY OF THE LIGHT HOUSE ESTABLISHMENT OF THE UNITED STATES.

Mr. Johnson read from a paper he had prepared for publication elsewhere, on the History of the Light-house Establishment of the United States, tracing its rise and progress from the first beacon which was erected on Point Allerton, entrance to Boston Harbor, in 1673, to the present time. He gave some account of the eight light-houses built by the Colonies; then of twelve built by the General Government prior to 1812, then of the progress of the establishment, under the charge of Mr. Pleasanton, an Auditor of the U. S. Treasury and the Acting Superintendent of the Lights, when the number increased to some three hundred and twenty-five; then of the causes which led to the creation of the provisional Light-House Board, and then of the erection of the permanent Light-House Board, and of the improvements the Board had since made, in all the arts and sciences connected with the erection of the light-houses and the establishment of cognate aids to navigation. Mr. Johnson then gave some account of light-house construction and of the different kinds of light-towers, material and style of the structures used, and of the problems solved in deciding on the various subaqueous foundations required. He illustrated his subject by the exhibition of large photographs of such stone light-houses as that on Spectacle Reef, Michigan, of such harbor lights as that on Thimble Shoal, entrance to Hampton Roads, Virginia, such skeleton iron houses on driven piles as that on Fowey Rocks, Florida Reef, and the tripod erected on Paris Island, Port Royal Sound, S. C., and of the remarkable stone light-house recently built on the summit of Tillamook Rock off the coast of Oregon.

Some account was given of the fog-signals used in this country, and a large crayon of the syren, the most powerful fog signal known, was shown.

Mr. Johnson spoke of the fact thus noted by Professor Henry: "It frequently happens on a vessel leaving a station that the sound is suddenly lost at a point in its course, and after remaining inaudible some time, is heard again at a greater distance, and is then gradually lost as the distance is further increased." In connection with this he exhibited a chart showing the site of Beaver Tail Light-House on the south point of Conanicut Island, between the two entrances to Narragansett Bay, with Bonnet Point, on which the steamer Rhode Island was wrecked in the fall of 1880, one and one-half miles to the northwest, with Fort Adams three and one-quarter miles to the northeast, and distant one and one-half miles to the southeast. On this chart was indicated the route of a sail boat which had been run to Bonnet Point, thence southerly to near Whale Rock; thence easterly close to Beaver Tail; thence north-easterly to Fort Adams, and thence southeasterly to Newport. On the route followed by the boat, he had indicated by half inch circles, the audibility of the fog-signal in full blast at Beaver Tail, as heard in the boat; the degrees being shown by the various shades; full audibility being indicated by darkening the whole surface of the circle, and complete inaudibility being shown by lack of shading in the circle. In this way it was shown that the observer, an officer of the Navy, found the sound of the fog-signal faint at half a mile from the signal, fainter at three-fourths of a mile off, much louder at a mile, less loud at one and one-eighth miles; he lost the sound entirely at one and one-fourth miles; at one and three-sixteenths miles he heard it faintly, and right under Bonnet Point, one and one-half miles distant, he heard it stronger than he did at one-half mile from the signal. In the run of about one mile from Bonnet Point toward Whale Rock he did not hear the fog-signal at all, and then he heard it faintly, and as he then ran almost toward the signal he lost its sound entirely; when about a half a mile west of the signal he heard its sound quite faintly, and then lost it, not hearing it again till within one-fourth of a mile when he suddenly heard it at its full power and continued to do so on his run to Newport until three-fourths of a mile away, when the sound diminished one-half, and continued so at one mile off and one and one-fourth miles off. At one and one-half miles distance the sound had diminished to about one-fourth of its power; at two miles off he lost it; he did not hear a trace of it at two and one-fourth, two and a half, or two and three-fourths miles distances; but he caught it faintly as he rounded Fort Adams at three miles away, and when he had run another one-fourth of a mile into Newport Harbor he heard it at almost its full power and continued to do so for another quarter of a mile, when he lost it all together.

Mr. Johnson called attention to the fact that in the run of this boat, the sound of the fog-signal had ranged from audibility to to inaudibility, and back again, several times; and that while it was lost at a distance of about a mile, it was distinctly, though faintly heard at Bonnet Point, distant one and one-half miles, and that while it was lost completely at two miles off, on the run to Newport, it was picked up at Fort Adams, three miles off, and heard almost at its full power at three and one-fourth and three and one-half miles away. These records were made by Lieut. Com. F. E. Chadwick, U. S. N., Assistant Light-House Inspector, to ascertain the facts, bearing on the statement that the fog-signal stopped from time to time, made by those who had noticed these intermissions of audibility; and the fact that the fog-signal was in continuous full blast, was noted by his assistant, who remained at Beaver Tail for the purpose.

Mr. Johnson stated that this ricocheting of sound, these intervals of audibility, ought to be recognized by the mariner, who should now understand that in sailing toward or from a fog-signal in full blast, he might lose and pick up its sound several times though no apparent object might intervene. And the mariner now needed that science should deduce the law of this variation in audibility and bring out some instrument which should be to the ears what the maiiner's compass is now to the eyes, and also that variations of this instrument yet to be invented, be provided for and corrected as now are the variations of the mariner's compass. The speaker referred to the benefit the mariner had derived from the promulgation of Professor Henry's theory of the tilting of the sound wave up or down by adverse or favorable winds, and said that by this the sailor had been led to go aloft in the one case and to get as near as possible to the surface of the water in the other, when trying to pick up the sound of a fog-signal.

In this connection Mr. Johnson read the following extract from an article entitled *Signaling by Means of Sound, by E. Price-Edwards*, from the *[English] Journal of the Society of Arts*:

"In one respect, however, the late Professor Henry, who was at the time chairman of the United States Light-House Board, differred from Dr. Tyndall, viz: in regard to the theory of acoustic clouds, and their resultant aërial echoes. Professor Henry's explanation of the obstruction of sound in clear weather, and the echoes, is founded upon the asserted existence of upper and lower currents of air, the tilting up of the sound wave, and the reflection of the sounds from the surface of the sea, or the crests of the wave. From this last explanation, Professor Henry seems to have receded before his death."

Mr. JOHNSON said that he called attention to this statement, as he was satisfied that Mr. Price-Edwards had permitted himself to fall into some inaccuracy as to Prof. Henry's action in this matter. It was within Mr. Johnson's personal knowledge that Prof. Henry, up to the last, had considered the theory of the tilting of the sound wave, under certain conditions, as a good working hypothesis. The Professor had it in contemplation when he was called from his labors to attempt the solution of certain of the questions connected with this subject by stationing observers in steamers, around a vessel anchored far enough from shore to be out of reach of land echoes, on which a powerful fog-signal should be in operation, and these observers should be aided by others in captive balloons, who should note simultaneously with them, upon charts and tables previously prepared, not only the audibility of the signal, but all the other data which could be obtained from the action of the thermometer, the hygrometer, and the anemometer, as to the then condition of the atmosphere. When all this information should be tabulated, Professor Henry hoped to deduce something more of the law of the movement of the sound wave under given conditions, and to formulate it for the benefit of the mariner. This was a work which Professor Henry had left to his successors and which the speaker believed they would not neglect.

Mr. JOHNSON then took up an article in the *Annales des Ponts et Chaussés for October*, 1880, *by M. Emile Allard, Inspecteur General des Ponts et Chaussés*, entitled *Comparison de Quelques Depenses Relative au Service des Phares en France, aux Etats-Unis et en Angleterre,* and called attention to that portion of it in which it was stated in effect, that the lighted coast of the United States measured about 7,500 nautical miles, and that the estimate of the Light-House Board of the expense of maintaining the Light-House Service for the year ending June 30, 1880, was $2,046,500, and that hence the cost to the United States for lighting each nautical mile of its coast was 1,293 francs, while that of France which had twenty-five lights to the one hundred nautical miles [the United States having but about nine lights to that distance] was but 1,155 francs.

Mr. Johnson then showed that the length of the lighted coasts of the United States, except those of the Mississippi, Missouri, and Ohio rivers, measured on a ten-mile chord, was 9,959 miles, giving, as his authority, recent statements made on this point by the United States Coast and Geodetic Survey and of the office of the Chief of Engineers of the United States Army; the one as to the length of the ocean, gulf, sound, and bay coast, and of the lighted rivers beside those above named, and the other as to the length of the lighted lake coasts. He then pointed out the natural mistake of M. Allard, in supposing that the amount of the Board's estimates (*Le Budget Annuel du Bureau des Phares*) had been appropriated by Congress for its support; and he showed instead that the appropriations were much less than the estimates, and that, owing to various causes, the appropriations even had not all been expended, so that the actual expenses of maintaining the United States Light-House Establishment for the year ending June 30, 1880, were but $1,943,600 instead of $2,046,500, as M. Allard had inferred. Hence, it followed that, while it costs France 1,155 francs to light each nautical mile of her coast, it costs but 922.7 francs to light each nautical mile of United States coast, instead of 1,293 francs as has been erroneously inferred by M. Allard.

Mr. Johnson closed by stating that the Light-House Establishment of the United States had been largely modeled on that of France; that the Light-House Board, while it still hoped to reach the French standard in many things, hardly expected to attain to certain of its economies; that he should not have thought of comparing the cost of the maintenance of the two establishments, but as this comparison had been made in the official French journal, he had thought it well, and due to the science of pharology, to correct the errors which had crept into the calculations of this high officer in the French Light-House Service.

The paper from which Mr. Johnson read, and on which he based his remarks, may be found in full in the Annual Appendix for 1880, to be published by the Appletons as Volume XX of the New American Cyclopedia.

Remarks on this paper were made by Messrs. Hilgard and Thornton A. Jenkins. The latter gave some interesting reminiscences of his early connection with the light-house service.

Mr. Taylor said that he wished to emphasize a single point in Mr. Johnson's communication, namely, that referring to Mr. Price-Edwards' statement in regard to the supposed change of view by Prof. Henry as to the explanation of acoustic disturbances, or, at least, as to the source of the ocean echo. The only thing which could give the slightest color to such a supposition was a purely incidental and wholly unimportant suggestion thrown out by Prof. Henry on this subject. Discarding the proposed explanation of the echo by the presence of a hygroscopic flocculence, or invisible acoustic clouds in the air, as quite insufficient in character, as too indefinite in limits, and as too mutable and evanescent in duration, in a mobile atmosphere, to account for so pronounced, distinct, and uniform a phenomenon, Prof. Henry thought, in the absence of any other sufficient surface, that, in view of the large amount of curvature in ordinary sound beams, acoustic waves might be reflected back to the ear from the ocean itself,—probably from the sloping sides of the waves. On having his attention drawn by Prof. Tyndall to the circumstance that the echoes were frequently distinct over a perfectly smooth sea, he admitted that this would invalidate the suggestion of wave crests being concerned in the effect; but he still believed that, with sounds sufficiently powerful to reach considerable distances, it was quite possible for some of the upper sound-beams to be so curved as to be reflected upward from a perfectly level floor, and still to reach an observer's ear placed near the origin of sound. He had also shown that *visible* clouds were quite incompetent to return any sensible echo to the loudest sounds.

So far from receding from his views in regard to the occasions of irregularity in the audibility of sound, in his last Report of the Light-House Board—that for 1877, published but a short time before his death—he announced his previous conclusions as only more confirmed by his later observations; and a summary of these conclusions was also published in the Smithsonian Report for 1877.

The ideas of sound transmission promulgated in popular books and lectures, as derived from class-room experiments, are very inaccurate and misleading when applied to any considerable range of sound travel. Were the medium of sound propagation—the atmosphere—perfectly homogeneous in density, in temperature, and in movement, the beams would indeed travel in sensibly straight lines, but still with a large amount of lateral diffusion bearing no analogy to the diffraction of light. But in distances of several miles—

say from one to ten, as involved in fog-signaling,—it may be said that such conditions of aërial uniformity are *never* present; or in other words that sound beams are never transmitted for any great distance in sensibly straight lines. And hence it is, that after every allowance for lateral deflection, there frequently remain under peculiar circumstances, intermediate points of acoustic darkness, or belts and regions of insulated silence.

The next communication was by Mr. E. B. ELLIOTT, who read from a cablegram from Berlin relative to the Monetary Conference about to meet at Paris, that a fixed legal ratio of value of gold to silver of 15½ to 1, and the unrestricted coinage of both metals at this fixed ratio of value, were to be presented to the Convention as the leading subjects for discussion, and prospective adoption.

The present market ratio is about 18 to 1, the proposed ratio 15½ to 1. Now one ounce of gold and eighteen ounces of silver are equivalents for debt-contracting and debt-paying purposes, but the proposition is that the nations enact that one ounce of gold and 15½ ounces of silver shall be legal equivalents for debt-paying purposes, the option of deciding in which of the two metals the payment shall be reckoned and paid, to be with the person making the payment, or debtor. It is a proposition then to allow the debtor to scale down his debt from 18 to 15½, to scale down his payments 14 per cent. from the existing standard;—a proposition that the nations in the payment of their public debts may diminish their payments 14 per cent. and also, that the people in their several countries may liquidate their debts, public and private at the same reduced rate, 14 per cent.

The adoption of this scheme of partial repudiation by our own or any other nation would of necessity prove disastrous to its credit.

The ability of our own country to pay its indebtedness is believed to be unsurpassed by any on the face of the globe, but its willingness is questioned, and the sending of a Commission to Europe, and inviting a conference of nations to favorably consider the subject of scaling down the value of the monetary unit of account, must tend to the depression of that credit.

If, with that doubt impending as to our *willingness* to make full payment of our indebtedness, our nation can borrow at the low rate of 3½ or 3¼ per cent. per annum, there is reason to believe that, with that doubt dispelled, our bonds can readily be placed on the world's market at the greatly improved rate of 3 per cent. per annum.

To this end it is desirable: (1), that the forced coinage of our legal tender silver dollar (of 412½ grains silver 9-10 fine) be discontinued; (2), that on all future coins and on bullion, be stamped their weight in grammes, and their fineness 9-10; and (3) that an international commission be created whose duty it shall be to periodically (annually or oftener) proclaim, based on the market quotations of the few months immediately preceding the date of the proclamation, the value in gold of an equal weight of silver; and (4) that the metric-stamped coin and bullion at the proclaimed ratio of value, shall each be equally legal tender of payment in unlimited amount, until the issuing of the next periodical proclamation.

This would be true bi-metallism. The adoption of the proposed ratio, 15½, would be silver mono-metallism under the misnomer of bi-metallism.

By the adoption of the true bi-metallic method proposed—*i. e.*, frequent periodical publication of the true market ratio, instead of a single arbitrary proclamation to last for all time—we should stand before the world with our willingness to pay undoubted, and our ability to pay unsurpassed and paramount among the nations, and our national debt could be placed on the market on more favorable terms than that of any other commercial country.

At the conclusion of Mr. Elliott's remarks, the Society adjourned.

198TH MEETING. APRIL 16, 1881.

The President in the Chair.

Fifty-four members and visitors present.

The minutes of the 196th and 197th meetings were read and adopted.

The Chair announced to the Society the election to membership of Mr. WILLIAM A. DECAINDRY.

The first communication of the evening was by Mr. ALEXANDER GRAHAM BELL, announcing to the Society, the discovery of

THE SPECTROPHONE.

In a paper read before the American Association for the Advancement of science, last August, I described certain experiments made by Mr. Sumner Tainter and myself, which had resulted in the construction of a "*Photophone*," or apparatus for the production of sound by light;* and it will be my object to-day to describe the progress we have made in the investigation of photophonic phenomena since the date of this communication.

In my Boston paper the discovery was announced, that thin disks of very many different substances *emitted sounds* when exposed to the action of a rapidly-interrupted beam of sunlight. The great variety of material used in these experiments led me to believe that sonorousness under such circumstances would be found to be a general property of all matter.

At that time we had failed to obtain audible effects from masses of the various substances which became sonorous in the condition of thin diaphragms, but this failure was explained upon the supposition that the molecular disturbance produced by the light was chiefly a surface action, and that under the circumstances of the experiments, the vibration had to be transmitted through the mass of the substance in order to affect the ear. It was therefore supposed that, if we could lead to the ear, air that was directly in contact with the illuminated surface, louder sounds might be obtained, and solid masses be found to be as sonorous as thin diaphragms. First experiments made to verify this hypothesis pointed towards success. A beam of sunlight was focussed into one end of an open tube, the ear being placed at the other end. Upon interrupting the beam, a clear, musical tone was heard, the pitch depending upon the frequency of the interruption of the light, and the loudness upon the material composing the tube.

At this stage our experiments were interrupted, as circumstances called me to Europe.

While in Paris a new form of the experiment occurred to my mind, which would not only enable us to investigate the sounds

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* Proceedings of American Association for the Advancement of Science, Aug. 27th, 1880; see, also, American Journal of Science, vol. xx, p. 305; Journal of the American Electrical Society, vol. iii, p. 3; Journal of the Society of Telegraph Engineers and Electricians, vol. ix, p. 404; Annales de Chimie et de Physique, vol. xxi.

produced by masses, but would also permit us to test the more general proposition that *sonorousness, under the influence of intermittent light, is a property common to all matter.*

The substance to be tested was to be placed in the interior of a transparent vessel made of some material, which (like glass) is transparent to light, but practically opaque to sound.

Under such circumstances the light could get in, but the sound produced by the vibration of the substance could not get out. The audible effects could be studied by placing the ear in communication with the interior of the vessel by means of a hearing tube.

Some preliminary experiments were made in Paris to test this idea, and the results were so promising that they were communicated to the French Academy on the 11th of October, 1880, in a note read for me by Mr. Antoine Breguet.* Shortly afterwards I wrote to Mr. Tainter, suggesting that he should carry on the investigation in America, as circumstances prevented me from doing so myself in Europe. As these experiments seemed to have formed the common starting point for a series of independent researches of the most important character carried on simultaneously in America by Mr. Tainter, and in Europe by M. Mercadier,† Prof. Tyndall,‡ W. E. Röntou,§ and W. H. Preece,|| I may be permitted to quote from my letter to Mr. Tainter the passage describing the experiments referred to:

"METROPOLITAN HOTEL, RUE CAMBON, PARIS,
"*Nov.* 2, 1880.

"DEAR MR. TAINTER: * * * I have devised a method of producing sounds by the action of an intermittent beam of light from substances that cannot be obtained in the shape of thin diaphragms or in the tubular form; indeed, the method is specially adapted to testing the generality of the phenomenon we have discovered, as it can be adapted to solids, liquids, and gases.

"Place the substance to be experimented with in a glass test-tube,

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* *Comptes Rendus*, vol. xcl, p. 595.

† "Notes on Radiophony," *Comptes Rendus*, Dec. 6 and 13, 1880; Feb. 21 and 28, 1881. See, also, *Journal de Physique*, vol. x, p. 53.

‡ "Action of an Intermittent Beam of Radiant Heat upon Gaseous Matter." *Proc. Royal Society*, Jan. 13, 1881, vol. xxxi, p. 307.

§ "On the tones which arise from the intermittent illumination of a gas." See *Annalen der Phys. und Chemie*, Jan., 1881, No. 1, p. 155.

|| "On the conversion of Radiant Energy into Sonorous Vibration." *Proc. Royal Society*, March 10, 1881, vol. xxxi, p. 506.

connect a rubber tube with the mouth of the test-tube, placing the other end of the pipe to the ear. Then focus the intermittent beam upon the substance in the tube. I have tried a large number of substances in this way with great success, although it is extremely difficult to get a glimpse of the sun here, and when it does shine the intensity of the light is not to be compared with that to be obtained in Washington. I got splendid effects from crystals of bichromate of potash, crystals of sulphate of copper, and from tobacco smoke. A whole cigar placed in the test-tube produced a very loud sound. I could not hear anything from plain water, but when the water was discolored with ink a feeble sound was heard. I would suggest that you might repeat these experiments and extend the results," &c., &c.

Upon my return to Washington in the early part of January.* Mr. Tainter communicated to me the results of the experiments he had made in my laboratory during my absence in Europe.

He had commenced by examining the sonorous properties of a vast number of substances enclosed in test-tubes in a simple empirical search for loud effects. He was thus led gradually to the discovery that cotton-wool, worsted, silk, and fibrous materials generally, produced much louder sounds than hard rigid bodies like crystals, or diaphragms such as we had hitherto used.

In order to study the effects under better circumstances he enclosed his materials in a conical cavity in a piece of brass, closed by a flat plate of glass. A brass tube leading into the cavity served for connection with the hearing-tube. When this conical cavity was stuffed with worsted or other fibrous materials the sounds produced were much louder than when a test-tube was employed. This form of receiver is shown in Figure I.

Mr. Tainter next collected silks and worsteds of different colors, and speedily found that the darkest shades produced the best effects. Black worsted especially gave an extremely loud sound.

As white cotton wool had proved itself equal, if not superior, to any other white fibrous material before tried, he was anxious to obtain colored specimens for comparison. Not having any at hand, however, he tried the effect of darkening some cotton-wool with lamp-black. Such a marked reinforcement of the sound resulted that he was induced to try lamp-black alone.

About a teaspoonful of lamp-black was placed in a test-tube and

* On the 7th of January.

exposed to an intermittent beam of sunlight. The sound produced was much louder than any heard before.

Upon smoking a piece of plate-glass, and holding it in the intermittent beam with the lamp-black surface towards the sun, the sound produced was loud enough to be heard, with attention, in any part of the room. With the lamp-black surface turned from the sun the sound was much feebler.

Mr. Tainter repeated these experiments for me immediately upon my return to Washington, so that I might verify his results.

Upon smoking the interior of the conical cavity shown in Figure I, and then exposing it to the intermittent beam, with the glass lid in position as shown, the effect was perfectly startling. The sound was so loud as to be actually painful to an ear placed closely against the end of the hearing-tube.

The sounds, however, were sensibly louder when we placed some smoked wire gauze in the receiver, as illustrated in the drawing, Figure I.

When the beam was thrown into a resonator, the interior of which had been smoked over a lamp, most curious alternations of sound and silence were observed. The interrupting disk was set rotating at a high rate of speed, and was then allowed to come gradually to rest. An extremely feeble musical tone was at first heard, which gradually fell in pitch as the rate of interruption grew less. The loudness of the sound produced varied in the most interesting manner. Minor reinforcements were constantly occurring, which became more and more marked as the true pitch of the resonator was neared. When at last the frequency of interruption corresponded to the frequency of the fundamental of the resonator, the sound produced was so loud that it might have been heard by an audience of hundreds of people.

The effects produced by lamp-black seemed to me to be very extraordinary, especially as I had a distinct recollection of experiments made in the summer of 1880 with smoked diaphragms, in which no such reinforcement was noticed.

Upon examining the records of our past photophonic experiments we found in vol. vii, p. 57, the following note:

"Experiment V.—Mica diaphragm covered with lamp-black on side exposed to light.

"Result: distinct sound about same as without lamp-black.—*A. G. B., July 18th*, 1880.

"Verified the above, but think it somewhat louder than when used without lamp-black."—*S. T., July 18th*, 1880.

Upon repeating this old experiment we arrived at the same result as that noted. Little if any augmentation of sound resulted from smoking the mica. In this experiment the effect was observed by placing the mica diaphragm against the ear, and also by listening through a hearing-tube, one end of which was closed by the diaphragm. The sound was found to be more audible through the free air when the ear was placed as near to the lamp-black surface as it could be brought without shading it.

At the time of my communication to the American Association I had been unable to satisfy myself that the substances which had become sonorous under the direct influence of intermittent sunlight were capable of reproducing sounds of articulate speech under the action of an undulatory beam from our photophonic transmitter. The difficulty in ascertaining this will be understood by considering that the sounds emitted by thin diaphragms and tubes were so feeble that it was impracticable to produce audible effects from substances in these conditions at any considerable distance away from the transmitter; but it was equally impossible to judge of the effects produced by our articulate transmitter at a short distance away, because the speaker's voice was directly audible through the air. The extremely loud sounds produced from lamp-black have enabled us to demonstrate the feasibility of using this substance in an articulating photophone in place of the electrical receiver formerly employed.

The drawing (Fig. 2) illustrates the mode in which the experiment was conducted. The diaphragm of the transmitter (A) was only 5 centimeters in diameter, the diameter of the receiver (B) was also 5 centimeters, and the distance between the two was 40 meters, or 800 times the diameter of the transmitter diaphragm. We were unable to experiment at greater distances without a heliostat on account of the difficulty of keeping the light steadily directed on the receiver. Words and sentences spoken into the transmitter in a low tone of voice were audibly reproduced by the lamp-black receiver.

In Fig. 3 is shown a mode of interrupting a beam of sunlight for producing distant effects without the use of lenses. Two similarly-perforated disks are employed, one of which is set in rapid rotation, while the other remains stationary. This form of interrupter is also admirably adapted for work with artificial light. The receiver illustrated in the drawing consists of a parabolic reflector, in the focus of which is placed a glass vessel (A) containing lamp-black, or other sensitive substance, and connected with a hearing-tube. The beam of light is interrupted by its passage through the two slotted disks shown at B, and in operating the instrument musical signals like the dots and dashes of the Morse alphabet are produced from the sensitive receiver (A) by slight motions of the mirror(C) about its axis (D.)

In place of the parabolic reflector shown in the figure a conical reflector like that recommended by Prof. Sylvanus Thompson * can be used, in which case a cylindrical glass vessel would be preferable to the flask (A) shown in the figure.

In regard to the sensitive materials that can be employed, our experiments indicate that in the case of solids the physical condition and the color are two conditions that markedly influence the intensity of the sonorous effects. *The loudest sounds are produced from substances in a loose, porous, spongy condition, and from those that have the darkest or most absorbent colors.*

The materials from which the best effects have been produced are cotton-wool, worsted, fibrous materials generally, cork, sponge, platinum and other metals in a spongy condition, and lamp-black.

The loud sounds produced from such substances may perhaps be explained in the following manner: Let us consider, for example, the case of lamp-black—a substance which becomes heated by exposure to rays of all refrangibility. I look upon a mass of this substance as a sort of sponge, with its pores filled with air instead of water. When a beam of sunlight falls upon this mass, the particles of lamp-black are heated, and consequently expand, causing a contraction of the air-spaces or pores among them.

Under these circumstances a pulse of air should be expelled, just as we would squeeze out water from a sponge.

The force with which the air is expelled must be greatly increased by the expansion of the air itself, due to contact with the heated particles of lamp-black. When the light is cut off the converse process takes place. The lamp-black particles cool and contract, thus enlarging the air spaces among them, and the enclosed air also becomes cool. Under these circumstances a partial vacuum should

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* Phil. Mag., April, 1881, vol. xi, p. 286.

be formed among the particles, and the outside air would then be absorbed, as water is by a sponge when the pressure of the hand is removed.

I imagine that in some such manner as this a wave of condensation is started in the atmosphere each time a beam of sunlight falls upon lamp-black, and a wave of rarefaction is originated when the light is cut off. *We can thus understand how it is that a substance like lamp-black produces intense sonorous vibrations in the surrounding air, while at the same time it communicates a very feeble vibration to the diaphragm or solid bed upon which it rests.*

This curious fact was independently observed in England by Mr. Preece, and it led him to question whether, in our experiments with thin diaphragms, the sound heard was due to the vibration of the disk or (as Prof. Hughes had suggested) to the expansion and contraction of the air in contact with the disk confined in the cavity behind the diaphragm. In his paper read before the Royal Society on the 10th of March, Mr. Preece describes experiments from which he claims to have proved that the effects are wholly due to the vibrations of the confined air, and that the *disks do not vibrate at all.*

I shall briefly state my reasons for disagreeing with him in this conclusion:

1. When an intermittent beam of sunlight is focussed upon a sheet of hard rubber or other material, a musical tone can be heard, not only by placing the ear immediately behind the part receiving the beam, but by placing it against any portion of the sheet, even though this may be a foot or more from the place acted upon by the light.

2. When the beam is thrown upon the diaphragm of a "Blake Transmitter," a loud musical tone is procuced by a telephone connected in the same galvanic circuit with the carbon button, (A,) Fig. 4. Good effects are also produced when the carbon button (A) forms, with the battery, (B,) a portion of the primary circuit of an induction coil, the telephone (C) being placed in the secondary circuit.

In these cases the wooden box and mouth-piece of the transmitter should be removed, so that no air-cavities may be left on either side of the diaphragm.

It is evident, therefore, that in the case of thin disks a real vibration of the diaphragm is caused by the action of the intermittent beam, in-

dependently of any expansion and contraction of the air confined in the cavity behind the diaphragm.

Lord Rayleigh has shown mathematically that a two-and-fro vibration of sufficient amplitude to produce an audible sound would result from a periodical communication and abstraction of heat, and he says: " We may conclude, I think, that there is at present no reason for discarding the obvious explanation that the sounds in question are due to the bending of the plates under unequal heating." (Nature, xxiii, p. 274.) Mr. Preece, however, seeks to prove that the sonorous effects cannot be explained upon this supposition; but his experimental proof is inadequate to support his conclusion. Mr. Preece expected that if Lord Rayleigh's explanation was correct, the expansion and contraction of a thin strip under the influence of an intermittent beam could be caused to open and close a galvanic circuit, so as to produce a musical tone from a telephone in the circuit. But this was an inadequate way to test the point at issue, for Lord Rayleigh has shown (Proc. of Roy. Soc., 1877,) that an audible sound can be produced by a vibration, whose amplitude is *less than a ten-millionth of a centimetre*, and certainly such a vibration as that would not have sufficed to operate a "make-and-break contact" like that used by Mr. Preece. The negative results obtained by him cannot, therefore, be considered conclusive.

The following experiments (devised by Mr. Tainter) have given results decidedly more favorable to the theory of Lord Rayleigh than to that of Mr. Preece:

1. A strip (A) similar to that used in Mr. Preece's experiment was attached firmly to the centre of an iron diaphragm, (B,) as shown in Figure 5, and was then pulled taut at right angles to the plane of the diaphragm. When the intermittent beam was focussed upon the strip (A) a clear musical tone could be heard by applying the ear to the hearing tube (C,)

This seemed to indicate a rapid expansion and contraction of the substance under trial.

But a vibration of the diaphragm (B) would also have resulted if the thin strip (A) had acquired a to-and-fro motion, due either to the direct impact of the beam or to the sudden expansion of the air in contact with the strip.

2. To test whether this had been the case an additional strip (D)

was attached by its central point only to the strip under trial, and was then submitted to the action of the beam, as shown in Fig. 6.

It was presumed that if the vibration of the diaphragm (B) had been due to a *pushing force* acting on the strip (A,) the addition of the strip (D) would not interfere with the effect. But if, on the other hand, it had been due to the longitudinal expansion and contraction of the strip, (A,) the sound would cease, or, at least, be reduced. The beam of light falling upon strip (D) was now interrupted as before by the rapid rotation of a perforated disk, which was allowed to come gradually to rest.

No sound was heard excepting at a certain speed of rotation, when a feeble musical tone became audible.

This result is confirmatory of the first.

The audibility of the effect at a particular rate of interruption suggests the explanation that the strip (D) had a normal rate of vibration of its own.

When the frequency of the interruption of the light corresponded to this, the strip was probably thrown into vibration after the manner of a tuning fork, in which case a to-and-fro vibration would be propagated down its stem or central support to the strip (A.)

This indirectly proves the value of the experiment.

The list of solid substances that have been submitted to experiment in my laboratory is too long to be quoted here, and I shall merely say that we have not yet found one solid body that has failed to become sonorous under proper conditions of experiment.*

Experiments with Liquids.

The sounds produced by liquids are much more difficult to observe than those produced by solids. The high absortive power possessed by most liquids would lead one to expect intense vibrations from the action of intermittent light, but the number of sonorous liquids that have so far been found is extremely limited, and the sounds produced are so feeble as to be heard only by the greatest attention and under the best circumstances of experiment.

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* Carbon and thin microscopic glass are mentioned in my Boston paper as non-responsive, and powdered chlorate of potash in the communication to the French Academy, (Comtes Rendus, vol. xcl, p. 595.) All these substances have since yielded sounds under more careful conditions of experiment.

In the experiments made in my laboratory a very long test-tube was filled with the liquid under examination, and a flexible rubber-tube was slipped over the mouth far enough down to prevent the possibility of any light reaching the vapor above the surface. Precautions were also taken to prevent reflection from the bottom of the test-tube. An intermittent beam of sunlight was then focussed upon the liquid in the middle portion of the test-tube by means of a lens of large diameter.

Results.

Clear water	No sound audible.
Water discolored by ink	Feeble sound.
Mercury	No sound heard.
Sulphuric ether *	Feeble, but distinct sound.
Ammonia	" "
Ammonia-sulphate of copper	" "
Writing ink	" "
Indigo in sulphuric acid	" "
Chloride of copper *	" "

The liquids distinguished by an asterisk gave the best sounds.

Acoustic vibrations are always much enfeebled in passing from liquids to gases, and it is probable that a form of experiment may be devised which will yield better results by communicating the vibrations of the liquid to the ear through the medium of a solid rod.

Experiments with Gaseous Matter.

On the 29th of November, 1880, I had the pleasure of showing to Prof. Tyndall, in the laboratory of the Royal Institution, the experiments described in the letter to Mr. Tainter from which I have quoted above, and Prof. Tyndall at once expressed the opinion that the sounds were due to rapid changes of temperature in the body submitted to the action of the beam. Finding that no experiments had been made at that time to test the sonorous properties of different gases, he suggested filling one test-tube with the vapor of sulphuric ether, (a good absorbent of heat,) and another with the vapor of bi-sulphide of carbon, (a poor absorbent,) and he predicted that if any sound was heard it would be louder in the former case than in the latter.

The experiment was immediately made, and the result verified the prediction.

Since the publication of the memoirs of Röntgen* and Tyndall† we have repeated these experiments, and have extended the inquiry to a number of other gaseous bodies, obtaining in every case similar results to those noted in the memoirs referred to.

The vapors of the following substances were found to be highly sonorous in the intermittent beam: Water vapor, coal gas, sulphuric ether, alchohol, ammonia, amylene, ethyl bromide, diethylamene, mercury, iodine, and peroxide of nitrogen. The loudest sounds were obtained from iodine and peroxide of nitrogen.

I have now shown that sounds are produced by the direct action of intermittent sunlight from substances in every physical condition, (solids, liquid, and gaseous,) and the probability is therefore very greatly increased that sonorousness under such circumstances will be found to be a universal property of matter.

Upon Substitutes for Selenium in Electrical Receivers.

At the time of my communication to the American Association the loudest effects obtained were produced by the use of selenium, arranged in a cell of suitable construction, and placed in a galvanic circuit with a telephone. Upon allowing an intermittent beam of sunlight to fall upon the selenium a musical tone of great intensity was produced from the telephone connected with it.

But the selenium was very inconstant in its action. It was rarely, if ever, found to be the case, that two pieces of selenium (even of the same stick) yielded the same results under identical circumstances of annealing, &c. While in Europe last autumn, Dr. Chichester Bell, of University College, London, suggested to me that this inconstancy of result might be due to chemical impurities in the selenium used. Dr. Bell has since visited my laboratory in Washington, and has made a chemical examination of the various samples of selenium I had collected from different parts of the world. As I understand it to be his intention to publish the results of this analysis very soon, I shall make no further mention of his investigation than to state that he has found sulphur, iron, lead, and arsenic in the so-called "selenium," with traces of organic matter; that a quantitative examination has revealed the fact that sulphur constitutes nearly one per cent. of the whole mass; and that when

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* Ann. der Phys. und Chem., 1881, No. 1, p. 155.

† Proc. Roy. Soc., vol. xxxi, p. 307.

these impurities are eliminated the selenium appears to be more constant in its action and more sensitive to light.

Prof. W. G. Adams* has shown that tellurium, like selenium, has its electrical resistance affected by light, and we have attempted to utilize this substance in place of selenium. The arrangement of cell (shown in Fig. 7) was constructed for this purpose in the early part of 1880; but we failed at that time to obtain any indications of sensitiveness with a reflecting galvanometer. We have since found, however, that when this tellurium spiral is connected in circuit with a galvanic battery and telephone, and exposed to the action of an intermittent beam of sunlight, a distinct musical tone is produced by the telephone. The audible effect is much increased by placing the tellurium cell with the battery in the primary circuit of an induction coil, and placing the telephone in the secondary circuit.

The enormously high resistance of selenium and the extremely low resistance of tellurium suggested the thought that an alloy of these two substances might possess intermediate electrical properties. We have accordingly mixed together selenium and tellurium in different proportions, and, while we do not feel warranted at the present time in making definite statements concerning the results, I may say that such alloys have proved to be sensitive to the action of light.

It occurred to Mr. Tainter before my return to Washington last January, that the very great molecular disturbance produced in lamp-black by the action of the intermittent sunlight should produce a corresponding disturbance in an electric current passed through it, in which case lamp-black could be employed in place of selenium in an electrical receiver. This has turned out to be the case, and the importance of the discovery is very great, especially when we consider the expense of such rare substances as selenium and tellurium.

The form of lamp-black cell we have found most effective is shown in Fig. 8. Silver is deposited upon a plate of glass, and a zigzag line is then scratched through the film, as shown, dividing the silver surface into two portions insulated from one another, having the form of two combs with interlocking teeth.

Each comb is attached to a screw-cup, so that the cell can be

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* Proc. Roy. Soc., vol. xxiv, p. 163.

placed in an electrical circuit when required. The surface is then smoked until a good film of lamp-black is obtained, filling the interstices between the teeth of the silver combs. When the lamp-black cell is connected with a telephone and galvanic battery, and exposed to the influence of an intermittent beam of sunlight, a loud musical tone is produced by the telephone. This result seems to be due rather to the physical condition than to the nature of the conducting material employed, as metals in a spongy condition produce similar effects. For instance, when an electrical current is passed through spongy platinum, while it is exposed to intermittent sunlight, a distinct musical tone is produced by a telephone in the same circuit. In all such cases the effect is increased by the use of an induction coil; and the sensitive cells can be employed for the reproduction of an articulate speech as well as for the production of musical sounds.

We have also found that loud sounds are produced from lamp-black by passing through it an intermittent electrical current; and that it can be used as a telephonic receiver for the reproduction of articulate speech by electrical means.

A convenient mode of arranging a lamp-black cell for experimental purposes is shown in Fig. 9. When an intermittent current is passed through the lamp-black, (A,) or when an intermittent beam of sunlight falls upon it through the glass plate B, a loud musical tone can be heard by applying the ear to the hearing-tube C. When the light and the electrical current act simultaneously, two musical tones are perceived, which produce beats when nearly of the same pitch. By proper arrangements a complete interference of sound can undoubtedly be produced.

Upon the Measurement of the Sonorous Effects produced by Different Substances.

We have observed that different substances produce sounds of very different intensities under similar circumstances of experiment, and it has appeared to us that very valuable information might be obtained if we could measure the audible effects produced. For this purpose we have constructed several different forms of apparatus for studying the effects, but as our researches are not yet complete, I shall confine myself to a simple description of some of the forms of apparatus we have devised.

When a beam of light is brought to a focus by means of a lens, the beam diverging from the focal point becomes weaker as the distance increases in a calculable degree. Hence, if we can determine the distances from the focal point at which two different substances emit sounds of equal intensity, we can calculate their relative sonorous powers.

Preliminary experiments were made by Mr. Tainter during my absence in Europe to ascertain the distance from the focal point of a lens at which the sound produced by a substance became inaudible. A few of the results obtained will show the enormous differences existing between the different substances in this respect.

Distance from Focal Point of Lens at which Sounds became Inaudible with Different Substances.

Zinc diaphragm, (polished)		1.51 m.
Hard rubber diaphragm		1.90 m.
Tin-foil "		2.00 m.
Telephone "	(Japanned iron)	2.15 m.
Zinc "	(unpolished)	2.15 m.
White silk,	(In receiver shown in Fig. 1.)	3.10 m.
White worsted,	" " "	4.01 m.
Yellow worsted,	" " "	4.06 m.
Yellow silk,	" " "	4.13 m.
White cotton-wool,	" " "	4.38 m.
Green silk,	" " "	4.52 m.
Blue worsted,	" " "	4.69 m.
Purple silk,	" " "	4.82 m.
Brown silk,	" " "	5.02 m.
Black silk,	" " "	5.21 m.
Red silk,	" " "	5.24 m.
Black worsted,	" " "	6.50 m.
Lamp-black. In this case the limit of audibility could not be determined on account of want of space.		
Sound perfectly audible at a distance of		10.00 m.

Mr. Tainter was convinced from these experiments that this field of research promised valuable results, and he at once devised an apparatus for studying the effects, which he described to me upon my return from Europe. The apparatus has since been constructed and I take great pleasure in showing it to you to-day.

(1.) A beam of light is received by two similar lenses, (A B, Fig. 10,) which brings the light to a focus on either side of the interrupting disk (C.) The two substances, whose sonorous powers are to be compared, are placed in the receiving vessels (D E) (so arranged as to expose equal surfaces to the action of the beam) which communicate by flexible tubes (F G) of equal length, with the common hearing-tube (H.) The receivers (D E) are placed upon slides, which can be moved along the graduated supports (I K.) The beams of light passing through the interrupting disk (C) are alternately cut off by the swinging of a pendulum, (L.) Thus a musical tone is produced alternately from the substance in D and from that in E. One of the receivers is kept at a constant point upon its scale, and the other receiver is moved towards or from the focus of its beam until the ear decides that the sounds produced from D and E are of equal intensity. The relative positions of the receivers are then noted.

(2.) Another method of investigation is based upon the production of an interference of sound, and the apparatus employed is shown in Fig. 11. The interrupter consists of a tuning-fork, (A,) which is kept in continuous vibration by means of an electro-magnet, (B.)

A powerful beam of light is brought to a focus between the prongs of the tuning-fork, (A,) and the passage of the beam is more or less obstructed by the vibration of the opaque screens (C D) carried by the prongs of the fork.

As the tuning-fork (A) produces a sound by its own vibration, it is placed at a sufficient distance away to be inaudible through the air, and a system of lenses is employed for the purpose of bringing the undulating beam of light to the receiving lens (E) with as little loss as possible. The two receivers (F G) are attached to slides (H I) which move upon opposite sides of the axis of the beam, and the receivers are connected by flexible tubes of unequal length (K L) communicating with the common hearing-tube (M.)

The length of the tube (K) is such that the sonorous vibrations from the receivers (F G) reach the common hearing-tube (M) in opposite phases. Under these circumstances silence is produced when the vibrations in the receivers (F G) are of equal intensity. When the intensities are unequal, a residual effect is perceived. In operating the instrument the position of the receiver (G) remains constant, and the receiver (F) is moved to or from the focus of the beam until complete silence is produced. The relative positions of the two receivers are then noted.

(3.) Another mode is as follows: The loudness of a musical tone produced by the action of light is compared with the loudness of a tone of similar pitch produced by electrical means. A rheostat introduced into the circuit enables us to measure the amount of resistance required to render the electrical sound equal in intensity to the other.

(4.) If the tuning-fork (A) in Fig. 11 is thrown into vibration by an undulatory instead of an intermittent current passed through the electro-magnet, (B,) it is probable that a musical tone, electrically produced in the receiver (F) by the action of the same current, would be found capable of extinguishing the effect produced in the receiver (G) by the action of the undulatory beam of light, in which case it should be possible to establish an acoustic balance between the effects produced by light and electricity by introducing sufficient resistance into the electric circuit.

Upon the Nature of the Rays that Produce Sonorous Effects in Different Substances.

In my paper read before the American Association last August and in the present paper I have used the word "light" in its usual rather than its scientific sense, and I have not hitherto attempted to discriminate the effects produced by the different constituents of ordinary light, the thermal, luminous, and actinic rays. I find, however, that the adoption of the word "photophone" by Mr. Tainter and myself has led to the assumption that we belived the audible effects discovered by us to be due entirely to the action of luminous rays. The meaning we have uniformly attached to the words "photophone" and "light" will be obvious from the following passage, quoted from my Boston paper:

"Although effects are produced as above shown by forms of radiant energy, which are invisible, we have named the apparatus for the production and reproduction of sound in this way the 'photophone' *because an ordinary beam of light contains the rays which are operative.*"

To avoid in future any misunderstanding upon this point we have decided to adopt the term "*radiophone,*" proposed by Mr. Mercadier, as a general term signifying an apparatus for the production of sound by any form of radiant energy, limiting the words *thermophone, photophone,* and *actinophone* to apparatus for the production of sound by thermal, luminous, or actinic rays respectively.

M. Mercadier, in the course of his researches in radiophony, passed an intermittent beam from an electric lamp through a prism, and then examined the audible effects produced in different parts of the spectrum. (*Comptes Rendus*, Dec. 6th, 1880.)

We have repeated this experiment, using the sun as our source of radiation, and have obtained results somewhat different from those noted by M. Mercadier.

(1.) A beam of sunlight was reflected from a heliostat (A, Fig. 12) through an achromatic lens, (B,) so as to form an image of the sun upon the slit (C.)

The beam then passed through another achromatic lens (D) and through a bisulphide of carbon prism, (E,) forming a spectrum of great intensity, which, when focused upon a screen, was found to be sufficiently pure to show the principal absorption lines of the solar spectrum.

The disk interrupter (F) was then turned with sufficient rapidity to produce from five to six hundred interruptions of the light per second, and the spectrum was explored with the receiver, (G,) which was so arranged that the lamp-black surface exposed was limited by a slit, as shown.

Under these circumstances sounds were obtained in every part of the visible spectrum, excepting the extreme half of the violet, as well as in the ultra-red. A continuous increase in the loudness of the sound was observed upon moving the receiver (G) gradually from the violet into the ultra-red. The point of maximum sound lay very far out in the ultra-red. Beyond this point the sound began to increase, and then stopped so suddenly that a very slight motion of the receiver (G) made all the difference between almost maximum sound and complete silence.*

(2.) The lamp-blacked wire gauze was then removed and the interior of the receiver (G) was filled with red worsted. Upon exploring the spectrum as before, entirely different results were obtained. The maximum effect was produced in the green at that part where the red worsted appeared to be black. On either side of this point the sound gradually died away, becoming inaudible on the one side in the middle of the indigo, and on the other at a short distance outside the edge of the red.

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* The results obtained in this and subsequent experiments are shown in a tabulated form in Fig. 14.

(3.) Upon substituting green silk for red worsted, the limits of audition appeared to be the middle of the blue and a point a short distance out in the ultra-red. Maximum in the red.

(4.) Some hard-rubber shavings were now placed in the receiver (G.) The limits of audibility appeared to be on the one hand the junction of the green and blue, and on the other the outside edge of the red. Maximum in the yellow. Mr. Tainter thought he could hear a little way into the ultra-red, and to his ear the maximum was about the junction of the red and orange.

(5.) A test-tube containing the vapor of sulphuric ether was then substituted for the receiver (G.) Commencing at the violent end the test-tube was gradually moved down the spectrum and out into the ultra-red without audible effect, but when a certain point far out in the ultra-red was reached, a distinct musical tone suddenly made its appearance, which disappeared as suddenly on moving the test-tube a very little further on.

(6.) Upon exploring the spectrum with a test-tube containing the vapor of iodine, the limits of audibility appeared to be the middle of the red and the junction of the blue and indigo. Maximum in the green.

(7.) A test-tube containing peroxide of nitrogen was substituted for that containing iodine. Distinct sounds were obtained in all parts of the visible spectrum, but no sounds were observed in the ultra-red. The maximum effect seemed to me to be in the blue. The sounds were well marked in all parts of the violet, and I even fancied that the audible effect extended a little way into the ultra-violet, but of this I cannot be certain. Upon examining the absorption spectrum of peroxide of nitrogen it was at once observed that the maximum sound was produced in that part of the spectrum where the greatest number of absorption lines made their appearance.

(8.) The spectrum was now explored by a selenium cell, and the audible effects were observed by means of a telephone in the same galvanic circuit with the cell. The maximum effect was produced in the red about its junction with the orange. The audible effect extended a little way into the ultra-red on the one hand and up as high as the middle of the violet on the other.

Although the experiments so far made can only be considered as preliminary to others of a more refined nature, I think we are warranted in concluding that *the nature of the rays that produce sonorous effects in different substances depends upon the nature of the* *substances that are exposed to the beam, and that the sounds are in every case due to those rays of the spectrum that are absorbed by the body.*

The Spectrophone.

Our experiments upon the range of audibility of different substances in the spectrum have led us to the construction of a new instrument for use in spectrum analysis. The eye-piece of a spectroscope is removed, and sensitive substances are placed in the focal point of the instrument behind an opaque diaphragm containing a slit. These substances are put in communication with the ear by means of a hearing-tube, and thus the instrument is converted into a veritable "spectrophone," like that shown in Fig. 13.

Suppose we smoke the interior of our spectrophone receiver, and fill the cavity with peroxide of nitrogen gas. We have then a combination that gives us good sounds in all parts of the spectrum, (visible and invisible,) except the ultra-violet. Now, pass a rapidly-interrupted beam of light through some substance whose absorption spectrum is to be investigated, and bands of sound and silence are observed upon exploring the spectrum, the silent positions corresponding to the absorption bands. Of course, the ear cannot for one moment compete with the eye in the examination of the visible part of the spectrum; but in the invisible part beyond the red, where the eye is useless, the ear is invaluable. In working in this region of the spectrum, lamp-black alone may be used in the spectrophonic receiver. Indeed, the sounds produced by this substance in the ultra-red are so well marked as to constitute our instrument a most reliable and convenient substitute for the thermo-pile. A few experiments that have been made may be interesting.

(1.) The interrupted beam was filtered through a saturated solution of alum.

Result: The range of audibility in the ultra-red was slightly reduced by the absorption of a narrow band of the rays of lowest refrangibility. The sounds in the visible part of the spectrum seemed to be unaffected.

(2.) A thin sheet of hard rubber was interposed in the path of the beam.

Result: Well-marked sounds in every part of the ultra-red. No sounds in the visible part of the spectrum, excepting the extreme half of the red.

These experiments reveal the cause of the curious fact alluded to in my paper read before the American Association last August—that sounds were heard from selenium when the beam was filtered through both hard rubber and alum at the same time. (See table of results in Fig. 14.)

(3.) A solution of ammonia-sulphate of copper was tried.

Result: When placed in the path of the beam the spectrum disappeared, with the exception of the blue and violet end. To the eye the spectrum was thus reduced to a single broad band of blue-violet light. To the ear, however, the spectrum revealed itself as two bands of sound with a broad space of silence between. The invisible rays transmitted constituted a narrow band just outside the red.

I think I have said enough to convince you of the value of this new method of examination, but I do not wish you to understand that we look upon our results as by any means complete. It is often more interesting to observe the first totterings of a child than to watch the firm tread of a full-grown man, and I feel that *our* first footsteps in this new field of science may have more of interest to you than the fuller results of mature research. This must be my excuse for having dwelt so long upon the details of incomplete experiments.

I recognize the fact that the spectrophone must ever remain a mere adjunct to the spectroscope, but I anticipate that it has a wide and independent field of usefulness in the investigation of absorption spectra in the ultra-red.

Mr. Wm. B. Taylor inquired whether the sounds obtained from the two absorpion bands of the ammonia-sulphate of copper were octaves of each other. Mr. Bell replied that this matter had not as yet been investigated.

Prof. William B. Rogers, President of the National Academy of Sciences, being present as an invited guest, paid a high tribute to Mr. Bell upon the very great interest and high scientific value of the discovery just announced.

The next communication was by Mr. G. Brown Goode on the

SWORD-FISH AND ITS ALLIES.

Fig. 1.

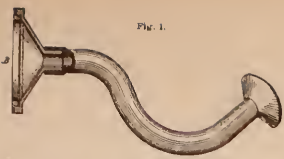
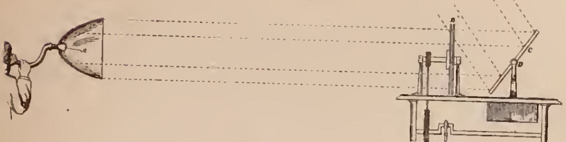


Fig. 2.

Fig. 3.



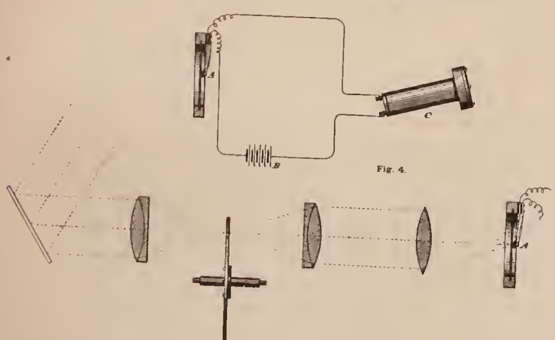


Fig. 4.

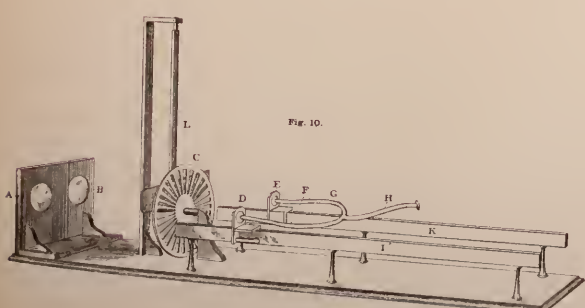
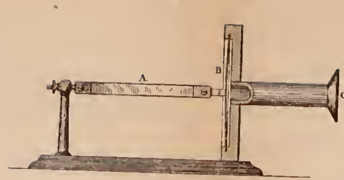


Fig. 10.

Fig. 5.



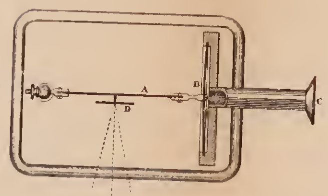


Fig. 6.

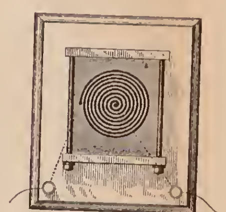


Fig. 7.

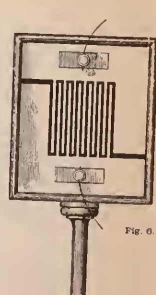


Fig. 6.

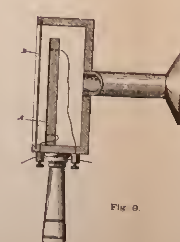


Fig. 9.

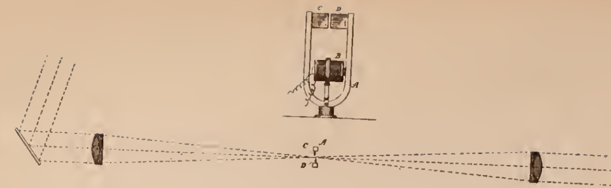


Fig. 11.

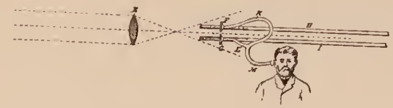
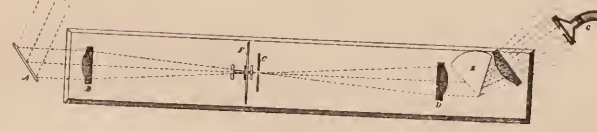


Fig. 12.



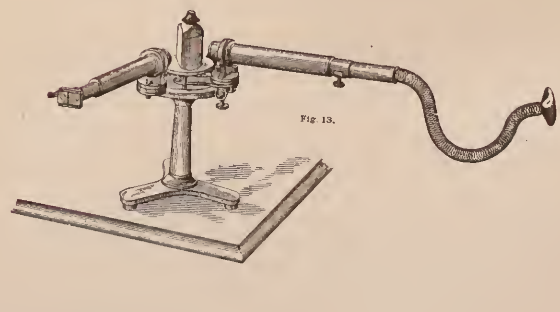
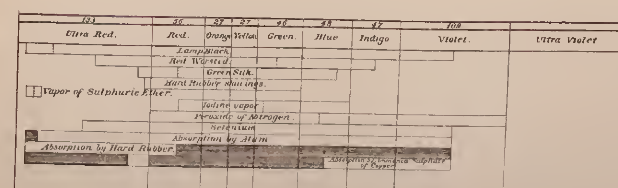


Fig. 13.

Fig. 14.



This paper will be found published in full in the Annual Report of the United States Fish Commission for the year 1880.

At the conclusion of Mr. GOODE'S paper the Society adjourned.

199TH MEETING. APRIL 30, 1881.

The President in the chair.

Forty-eight members present.

The recorder of the minutes of the last meeting being absent their consideration was postponed.

Mr. W. H. DALL made a communication on

RECENT DISCOVERIES IN ALASKA NORTH OF BEHRING STRAIT,

in which he alluded to the investigations carried on by the U. S. R. S. Corwin, Capt. Hooper, during the summer of 1880, including meteorology, sea temperatures and currents, as well as the investigation of the coal mines near Cape Lisburne. He described some observations made by the U. S. Coast Survey party under his charge in the same region and season, on board the U. S. S. Yukon. The migration of the Asiatic Eskimo; the sources of the warm waters of the eastern half of Behring Strait in Kotzebue and Norton Sound waters, moved by the tidal and river flow; the existence of a supposed new species of sheep allied to the Rocky Mountain bighorn (*Ovis montana*) in the east Siberian peninsula, and the character of Arctic vegetations were spoken of. Reasons for doubting the truth of the account of an alleged landing on Wrangell Land, in 1866, described in the Bremen Geographical Society's publication by a Capt. Dallmann were brought forward, and it was pointed out that the existence of Plover Island, of Siberian musk-oxen, and of certain conditions of the ice alleged by Dallmann, were in conflict with all that is definitely known by scientific men of those matters.

Remarks upon this paper were made by Messrs. ANTISELL, WHITE, FARQUHAR, HARKNESS, ALVORD, MASON, HAZEN, WELLING, ABBE, BESSELS, and GILL.

Mr. J. S. BILLINGS commenced a paper on Mortality Statistics of the Tenth Census, but at the usual hour of adjournment it was interrupted, to be resumed at the following meeting.

The Society then adjourned.

200TH MEETING. MAY 14, 1881.

The President in the Chair.

Thirty-six members present.

The minutes of the last two meetings were read and adopted.

The first communication of the evening was the continuation by Mr. J. S. BILLINGS of his remarks upon

MORTALITY STATISTICS OF THE TENTH CENSUS.

[Abstract.]

Mr. J. S. BILLINGS described the methods used in the Tenth Census to secure completeness and accuracy in the returns of mortality. The Superintendent of the Census sought to secure the aid of the physicians of the country, and for this purpose sent to each a small blank book, each leaf of which was arranged to record the facts connected with a single death. 70,306 such books were issued, and 24,057 returned at the end of the census year. The data from these books were compiled by causes of death, age, and sex, and the slips were then used to complete the enumerator's schedules. The total number of deaths reported from all sources for the census year will be a little over 800,000, or about 16 per 1,000 of living population, being an improvement in completeness over previous censuses. The results of the attempt to record the number sick on the day of the census are not very satisfactory, and it is feared they will be too incomplete to be used. Taking the schedules for the State of Rhode Island, which are believed to be the most complete, it is found that the number reported sick on the 30th of June was 11.18 per 1,000 of the whole population.

It is usual to estimate two years of sickness to each death, which would make the number constantly sick range from 30 to 40 per 1,000. In the army for five years the proportion was 43 per 1,000.

It seems probable that, while the proportion of sick shown by the Rhode Island count is too low, it is more nearly correct than any other data which we possess.

Mr. Billings continued his remarks upon the Methods of the Tenth Census, and described the methods of compiling the mortality statistics and the forms of tables to be used. The importance of these forms is greater than usual since they will probably serve to a certain extent as models for the State Censuses of 1885. The want of uniformity in tables of mortality was shown by a chart in which the various forms were compared. The various items given in a return of death, viz., sex, age, color, civil condition, nativity, parentage, occupation, month of death, locality and cause of death, were commented on, and it was shown that to present all these facts in their various relations, would require several hundred quarto volumes. A selection, therefore, becomes necessary. The relative value of giving the causes of death in detail is very much less in tables to be prepared from the enumerator's schedules than in those prepared from the returns of a system of registration where the cause of death in each case has been certified to by a physician.

The importance of a proper tabulation by locality is very great, and a certain amount of data should be given by counties. A form of mortality return by counties was shown and explained. The distinction between nativity and race or parentage was explained, and great importance attached to the giving the parentage as fully as possible in the present census.

The modes of compiling by schedule sheets, by cards, and by tallying machines were then explained. The subject of life tables for the United States was briefly discussed—the ground being taken that such a table for the whole country would have little or no practical value, and that life tables by States would be much more desirable and important.

Remarks were made on this paper by Messrs. Mason, Antisell, Toner, and Harkness.

The communication was followed by one from Mr. S. C. Busey, on the

RELATION OF METEOROLOGICAL CONDITIONS TO THE SUMMER DIARRHŒAL DISEASES.

[Abstract. The paper will be found in Vol. 32, Transactions American Medical Association.]

An analysis of the mortality statistics of these diseases leads to the following conclusions:

1. Diarrhœal diseases are far more destructive to infants than to adults.

2. They prevail almost exclusively during the warmest months of the year.

3. They are more prevalent in the region of this country north of the north line of the Gulf States and east of the Rocky Mountains.

The first two conclusions are universally admitted; the third is not so generally recognized.

Two additional propositions are suggested:

1. These diseases occur in groups, when the cases rapidly multiply during successive days for a week or fortnight, followed by an interval during which few or no cases occur.

2. These groups correspond with waves of continuous high temperature during day and night, which spread, at shorter or longer intervals during the summer months, over the northern climatic belt of this country, lasting from three to fourteen days, and varying in intensity at different times and in different years.

The first of these propositions cannot be established, because of the absence of statistical data relating to the beginning of the initial symptoms of the diseases; the second is proven by data supplied by the Signal Service Bureau. A comparison of these data with the mortality statistics shows:

1. That the month of July is the hottest and sickliest month of the year, most conducive to bowel affections, and most fatal to children under five years of age.

2. The epidemics of bowel affections of children, incident to the summer season, have their beginning nearly simultaneously with the first exacerbation of heat, which usually occurs in the latter half of June; and the maximum daily mortalities more frequently correspond with the maximum temperatures, which occur in periods of three or more days, at longer or shorter intervals during the summer months.

3. With the usual lowering of temperature and absence of ex-

cessive heat periods, which occur after the middle of August, the daily mortality declines.

4. The detrimental influence of summer temperature is intensified by sudden and acute elevations and falls.

5. Children under one year of age are most numerously and seriously affected.

Heat exhibits its deleterious influence in another and very important relation. It is one of the many conditions which, in conjunction, make up a season. A comparison of the statistics of the weekly mortality from diarrhœal diseases in the principal cities of the country grouped according to latitude, will exhibit the gradual increase of these diseases with the gradual advance of the summer solstice northward until it reaches its maximum during the period when all the elements which complete the season of summer are in their fullest activity; also a gradual decline with the return of the winter season.

The total movement of the wind is, perhaps, a more important influence than is generally believed. A comparison of the mortality data with the records of the monthly measurement of the wind, supplied by the Signal Service Bureau for the years 1875, 1876, 1877, 1878, 1879, and 1880, shows:

1. July is the month of greatest mortality and least movement of the wind.

2. The nearer the monthly movements of the wind approach uniformity, the less the mortality for summer diarrhœas.

3. Equality of climate corresponds with uniformity of and moderate or small movements of wind, and small mortalities.

4. Wide ranges of temperature correspond with large movements of wind and high mortalities from diarrhœal diseases.

5. Weekly mortalities from diarrhœal disease increase correspondingly with advance of the summer solstice northward, increasing and greater range of temperature, and larger and more fluctuating movements of wind.

Relative saturation of the air bears no constant relation to mortalities. Moisture in relative excess to the heat of an impure and stagnant atmosphere is the condition which supplies the most satisfactory explanation of its detrimental influence.

Remarks were made upon this paper by Messrs. HARKNESS, BILLINGS, and WOODWARD.

At the conclusion of this discussion the Society adjourned.

201ST MEETING. MAY 28, 1881.

The President in the Chair.

Thirty-four members present.

The minutes of the last meeting were read and adopted.

The first communication was by Mr. D. P. TODD on

THE SOLAR PARALLAX AS DERIVED FROM THE AMERICAN PHOTOGRAPHS OF THE TRANSIT OF VENUS, 1874, DECEMBER 8–9.

In the volume of observations of the transit of Venus recently issued, the photographs are presented in very nearly the form of equations of conditions involving the corrections of the relative right ascension and declination of the sun and Venus, and the correction of the adopted value of the solar parallax. The total number of photographs is 213, of which 84 were obtained at stations in the northern hemisphere, and 129 in the southern.

Every photograph gives one equation of condition in distance, s, of the form

$$0 = a\,\delta A + b\,\delta D + c\,\delta\omega - (\mathrm{o.} - \mathrm{C.})$$

The normal equations in s are—

$$\begin{aligned} +23.99\,\delta A + 24.71\,\delta D - 28.72\,\delta\omega - 82.17 &= 0 \\ +24.71\,\delta A + 184.66\,\delta D - 3.16\,\delta\omega - 439.51 &= 0 \\ -28.72\,\delta A - 3.16\,\delta D + 484.51\,\delta\omega + 21.72 &= 0 \end{aligned}$$

Their solution gives—

$$\begin{aligned} \delta A &= +1.''181 \pm 0.''202 \\ \delta D &= +2.''225 \pm 0.''070 \\ \delta\omega &= +0.''0397 \pm 0.''0418 \end{aligned}$$

Every photograph gives, likewise, one equation of condition in position-angle, p, of the form

$$0 = a'\,\delta A + b'\,\delta D + c'\,\delta\omega - (\mathrm{o'.} - \mathrm{C'.})$$

The normal equations in p are—

$$\begin{aligned} +8682117\,\delta A - 1404261\,\delta D - 138999.20\,\delta\omega - 142109.4 &= 0 \\ -1404261\,\delta A + 1521370\,\delta D - 25093.11\,\delta\omega + 10442.1 &= 0 \\ -138999.20\,\delta A + 25093.11\,\delta D + 7326.76\,\delta\omega + 2651.6 &= 0 \end{aligned}$$

Their solution gives—

$$\delta A = + 1.''109 \pm 0.''109$$
$$\delta D = + 0.''637 \pm 0.''224$$
$$\delta \omega = + 0.''0252 \pm 0.''0595$$

Combining these values of δ A, δ D, and $\delta\ \omega$ in accordance with their probable errors, we have, finally,

$$\delta A = + 0.''075 \pm 0.''006$$
$$\delta D = + 2.''083 \pm 0.''067$$
$$\delta \omega = + 0.''035 \pm 0.''034$$

The assumed value of ω being 8.″848, we have, therefore, for the mean equatorial horizontal parallax of the sun,

$$8.''883 \pm 0.''034,$$

corresponding to a distance between the centres of the sun and earth, equal to 92,028,000 miles.

(This paper appears in part in *The American Journal of Science* for June, 1881.)

Mr. Harkness remarked that the Americans who were engaged in the last transit observations may fairly congratulate themselves upon the results obtained from the photographs, as he had no doubt that they were more satisfactory and consistent than the photographic results obtained by any other nation. There may be said to be two distinct methods of obtaining photographs involving instruments differing widely from the other. The English method employed a telescope of four or five inches aperture producing an image of the sun about three-fourths of an inch in diameter. It is necessary to enlarge this image to a diameter of about four inches, and therefore they used in connection with it a Dallmeyer rapid rectilinear lens, enlarging it by that amount. It is obvious that this enlargement by the use of such a lens must be accompanied by an amount of distortion of the image, which, unless it can be accurately determined and eliminated, must introduce a serious error in the measurements of the negatives, and in the results derived from them. This distortion varies in the direction of radii from the optical center of the image, and is equal in circles about that center. Thus far the amount of this distortion has not been determined. The other method, employed by the Americans, involved the use of a lens with forty feet focal distance giving directly the required size of image, and involving no appreciable distortion inherently due to the construction of the apparatus, and thus avoided the causes of error just described. The focal length required to be determined with great accuracy, and this was readily effected.

Another difficulty arose from the fact that the diameter of the photographic picture on the negative was liable to variation, with a varying length of exposure; and the diameter of the image of Venus is liable to an inverse variation of the same kind. If the distance between the exterior boundaries of the sun and planet were measured, this error would be liable to vitiate the result and, hence, it was necessary to find the centers of the two images, and measure the distances between these central points. Mr. Harkness described the method by which this was satisfactorily accomplished.

There were about twenty plates which gave anomalous results. It was obvious after trial, that the difficulty was with the plates themselves and not due to the observers, since from any one plate a number of observers obtained corresponding results.

Mr. HARKNESS then spoke of the various methods employed to ascertain the sun's parallax: 1st, by measuring the velocity of light, and the time required for light to traverse known chords of the earth's orbit; 2d, by measuring the aberration of light; 3d, by measuring the parallax of the planet Mars; and 4th, by the analysis of the motions of the moon; all of which gave results in very close agreement.

The second communication was by Mr. G. K. GILBERT on

THE ORIGIN OF THE TOPOGRAPHICAL FEATURES OF LAKE SHORES.

This communication was reserved by the author.

After remarks by Mr. ANTISELL, the Society adjourned.

202D MEETING. JUNE 11, 1881.

The President in the Chair.

Fifty-seven members and visitors present.

The minutes of the last meeting were read and adopted.

The Chair announced to the Society that the General Committee had resolved that at the conclusion of the present meeting the Society would stand adjourned until the second Saturday in October.

The first communication of the evening was by Mr. J. J. WOODWARD, the President of the Society, entitled

A BIOGRAPHICAL SKETCH OF THE LATE DR. OTIS.

George Alexander Otis, Surgeon and Brevet Lieutenant-Colonel, United States Army, Curator of the Army Medical Museum, and Editor of the Surgical volumes of the Medical and Surgical History of the War of the Rebellion, died at Washington, D. C., February 23, 1881, at the comparatively early age of fifty years.

Surgeon Otis was descended from a cultivated New England family. His great grandfather, Ephraim Otis, was a physician who practiced at Scituate, Massachusetts. His grandfather, George Alexander Otis, was a well-known citizen of Boston, Massachusetts, whose early years were occupied by commercial pursuits. Mr. Otis was a man of education and literary tastes, who, so soon as his circumstances permitted, retired from business, and devoted himself entirely to books. He is remembered especially on account of his translation of Botta's History of the War of the Independence of the United States of America, published in 1820, an undertaking in which he was encouraged by James Madison and John Quincy Adams, and which he accomplished so well that the book ran through twelve editions. He died at an advanced age in June, 1863.

The father of Surgeon Otis, also George Alexander Otis, was born in 1804. He attended the preparatory course at the Boston Latin School, studied and graduated at Harvard College, after which he devoted himself, with much promise, to the profession of law. Mr. Otis was married February 9, 1830, to Anna Maria Hickman, of Newton, Massachusetts, daughter of Harris Hickman, a lawyer, born at Front Royal, Virginia, who had enjoyed an excellent professional reputation in early life in the Shenandoah Valley, and subsequently at Detroit, in the then Territory of Michigan. Of this marriage the subject of our biographical sketch was the only issue, Mr. Otis dying of consumption, June 18, 1831.

George Alexander Otis was born in Boston, Massachusetts, November 12, 1830. Left an infant to the tender care of his widowed mother, his early years were nurtured by a devoted love, which accompanied him through youth and manhood, smoothed the pillow of his last illness, and followed him to the grave.

When old enough to go to school, George was sent at first to the Boston Latin School, and afterwards to the Fairfax Institute, at Alexandria, Virginia, where he was prepared for college. In 1846 he entered Princeton College as a student of the sophomore class, and graduated with the degree of A. B., in 1849. Princeton conferred upon him the degree of Master of Arts in 1852.

At Princeton, Otis appeared as a slender, rather delicate youth, of highly nervous organization, whose literary tastes were not satisfied with the comparatively narrow curriculum of his Alma Mater. Always standing well in his college classes, that he did not take a still higher place was not due to lack of ability or of studious habits, but rather to his love of general literature, and the large proportion of his time expended in its cultivation. He had already acquired a fondness for French literature, which he never afterwards lost, and a taste for verse so far cultivated that when he came to graduate the Faculty assigned to him the task of preparing the commencement-day poem. Retiring and reserved in his manners, often silent and abstracted, the few who were admitted to his intimacy found his nature gentle and sympathetic, and several of the friendships he then formed lasted throughout his life.

By this time Otis had selected medicine as his profession. After leaving Princeton he went to Richmond, Virginia, where his mother was then residing, and began his studies in the office of Dr. F. H. Deane, of that city. In the fall of 1849 he proceeded to Philadelphia, and matriculated in the Medical Department of the University of Pennsylvania. That institution conferred upon him the degree of Doctor of Medicine in April, 1851. In those days the medical teachings of the University of Pennsylvania were shaped in no small degree by the influence of the Schools of Paris. Indeed, this was then true of almost all American medical teaching, and ambitious American medical students still looked with enthusiasm towards the lecture-rooms and hospitals of the French capital as affording the richest opportunities for the completion of their medical education. Accordingly Otis spent in Paris the first winter after he graduated in Philadelphia. He sailed from New York on the 16th of August, and reached Paris in the latter part of September, 1851.

During his stay in Paris, Otis made diligent use of the opportunities afforded for professional improvement. A manuscript note-book left among his papers shows that he devoted much time to the clinical teachings of the great French masters of that day. He listened to the instructions of Louis, Piorry, Cruveilhier, and Andral. It was at the time his expectation to give especial attention to the subject of ophthalmic surgery, and accordingly he attended with great diligence the clinics and didactic lectures of Desmarres, but he found the attractions of general operative surgery too strong to permit exclusive attention to this chosen branch, and he continually watched the operations, and listened to the lessons of such surgeons as Nélaton, Civiale, Malgaigne, Jobert (de Lamballe), Roux, and Velpeau. Moreover, the popular excitement which preceded the coup d'état of December 2, 1851, and the probability of bloodshed, directed his attention to the subject of military surgery. Already, November 4th, his note-book records a morning spent at the library of l'Ecole de Médecine in the study of Baron Larrey's "Mémoire," with which he was so well pleased that he at once purchased a copy for closer study. After the coup d'état a considerable number of those wounded at the barricades were carried to the hospitals for treatment, and Otis was thus enabled to take his first practical lessons in military surgery from Velpeau, Roux, and Jobert (de Lamballe).

Meanwhile, however, his diligence in medical studies did not prevent him from spending many pleasant hours in the art galleries and museums, where he found much to gratify his æsthetic nature. Moreover, he took a deep interest in the stirring panorama of French politics, as is shown by a series of letters he took time to write to the *Boston Evening Transcript.*

In the spring of 1852 Otis returned to the United States, reaching New York in the latter part of March. Immediately after his return he established himself at Richmond, Virginia, where he opened an office for general medical and surgical practice, and where his tastes and ambition soon led him to embark in his earliest enterprise in the domain of medical literature. In April, 1853, he issued the first number of *The Virginia Medical and Surgical Journal.* Dr. Howell L. Thomas, of Richmond, was associated with him as co-editor, but the financial risk was assumed entirely by Otis. The journal appeared monthly, each number containing over eighty pages octavo, the whole forming two annual volumes, commencing respectively with the numbers of April and October. It was handsomely printed, and contained from time to time a fair share of original articles, chiefly by physicians residing in Richmond and other parts of Virginia; but its most striking characteristic was the number of translations and abstracts from current French medical literature which appeared in its pages. Dr. Thomas, like his colleague, was a good French scholar, and had studied in Paris; both took part in the labor of translation and condensation, and as most of the articles were unsigned, it is not always possible to ascribe particular ones to the proper editor.

Notwithstanding its merits several causes contributed to interfere with the financial success of the journal. On the one hand, it was unsupported by the influence and business connections of an established publishing house, or of the faculty of any medical college. On the other hand, the success it might perhaps otherwise have achieved as a local organ of the medical profession in Virginia was impaired by the existence of an already-established rival, *The Stethoscope*, a monthly medical journal edited by Dr. P. Claiborne Gooch, at that time Secretary of the Medical Society of Virginia.

The field of local patronage was not large enough to support two such journals, and both suffered from the competition. Before the close of 1853, Otis found it necessary to secure an associate who could share in the pecuniary support of his enterprise. Thomas retired from the editorship, and was succeeded after the issue of the December number, by Dr. James B. McCaw, of Richmond, who became also part owner of the journal. *The Stethescope* appears to have suffered still more, for about the same time its editor entered into negotiations with the Virginia Medical Society, as a result of which he sold the journal, and the number of *The Stethescope* for January, 1854, appeared as "the property and organ of the Medical Society of Virginia, edited by a committee of the society."

This arrangement was, undoubtedly, for a time very prejudicial to the prosperity of the *Virginia Medical and Surgical Journal*, but its editors bravely maintained the struggle, and in the heated discussion concerning the purchase of *The Stethoscope*, that took place during the meeting of the Medical Society of Virginia in April, 1854, Otis, with characteristic gallantry, refused to surrender his independence to secure the passage of resolutions complimentary of the managment of his journal.

Otis had, by this time, become dissatisfied with his prospects of professional success in Richmond, and circumstances led him to select Springfield, Massachusetts, as his place of residence. He removed to that town during the summer of 1854. This necessitated changes in the management of the *Virginia Medical and Surgical Journal.* In May, 1854, Dr. J. F. Peebles, of Petersburg, Virginia, became associated with McCaw as one of its editors, while Otis retired from active participation in its direction, retaining, however, literary connection with it as corresponding editor.

Meanwhile, a single year proved sufficient to disgust the Virginia Medical Society with the task of editing a journal. Its management was found fruitful of unfortunate dissensions, and in May, 1855, the society wisely concluded to sell out. Under new auspices *The Stethoscope* continued to appear monthly until the close of the year, when an arrangement was effected by which it was united with *The Virginia Medical and Surgical Journal*, under the title of *Virginia Medical Journal*, with McCaw as editor, and Otis as corresponding editor.

Although his residence in Richmond had failed to secure for Otis a lucrative practice, this could not well have been expected at his early age. It had, however, given him some opportunities for acquiring experience at the bedside as well as in literature, and if he did not secure the profitable favor of the laity, he at least won for himself the respect and confidence of his professional brethren. He was an active member of the Virginia Medical Society, and represented that body in the American Medical Association at the Richmond meeting of May, 1852. He was also a member of the Richmond Medico-Chirurgical Society, which he represented in the American Medical Association at the New York meeting of May, 1853.

Established at Springfield, Massachusetts, Otis occupied himself more exclusively than heretofore with the duties of private practice, and with better pecuniary success than he had enjoyed at Richmond. He continued for a time to contribute translations, abstracts, and various items to the *Virginia Medical Journal;* but as the demands of his business became more urgent these became fewer, although he continued to be nominally corresponding editor of that journal until the close of 1859. As time wore on, he began to obtain considerable local reputation as a skillful surgeon, and would probably have acquired both wealth and distinction in civil surgical practice but for the outbreak of the War of the Rebellion. This changed the whole tenor of his life. So soon as it became clear to his mind that the struggle was likely to be a prolonged one, he resolved to devote himself to the service of his country. He received from Governor Andrew the appointment of Surgeon to the 27th Regiment of Massachusetts Volunteers, of which Horace C. Lee was Colonel, and was mustered into the service of the United States, September 14, 1861.

The 27th Regiment was raised in the western part of the State of Massachusetts, and was mustered into the service of the United States at Springfield. It left the State November 2, 1861, and proceeded by rail to the vicinity of Annapolis, Maryland, where it went into camp. Here it remained until January 6, 1862, when it was embarked on transports, and accompanied the North Carolina Expedition under General Burnside. It took part in the affair on Roanoke Island, February 8th; landed near Newburn, North Carolina, March 13th, and met with considerable losses during the battle of Newburn on the following day. The regiment remained in North Carolina until October 16, 1863, when it embarked for Fortress Monroe, Virginia, and after a short encampment at Newport News, proceeded to Norfolk, Virginia, where it remained through the following winter.

During almost the whole of this time Surgeon Otis accompanied his regiment and shared its fortunes; sometimes, indeed, performing other duties in addition to his regimental ones, as during the summer and fall of 1862, when he acted as Medical Purveyor to the Department of North Carolina. The exceptional periods were a few days in September, 1862, when he went as medical officer in charge of the steamer "Star of the South" with sick from Newburn to New York, and a few months in the early part of 1863, when he served on detached duty in the Department of the South. While in the Department of the South he attracted the attention of Surgeon Charles H. Crane, U. S. Army, then Medical Director of the Department (afterwards Assistant Surgeon-General of the Army), on whose recommendation he was placed, March 28th, by command of General Hunter, in charge of the hospital steamer "Cosmopolitan," then at Hilton Head, South Carolina, and directed the operations of that vessel in the transportation of the sick and wounded within the limits of the department until May 10, when he was ordered to carry a number of sick and wounded to New York harbor, and after landing them, to turn over the vessel to Surgeon Wm. Ingalls, of the 5th Massachusetts regiment. This order was promptly executed, the vessel was turned over as directed, May 13th, and Otis received a leave of absence for twenty days, at the expiration of which he returned to his regiment.

January 22, 1864, he was again detached and ordered to Yorktown, Virginia, to assume the duties of surgeon-in-chief of General Wistar's command. This responsible position he filled in a satisfactory manner from the first of February, when he reported for duty at Yorktown, until April 11, when he was relieved and assigned as surgeon-in-chief to General Heckman's division of the 18th Army Corps, then encamped near Portsmouth, Virginia. May 10th he received a sick leave for fifteen days, which, as his health was not restored at its expiration, was extended for thirty days more. June 26, 1864, he tendered his resignation as surgeon of the 27th Massachusetts regiment, and received an appointment as Assistant Surgeon of United States Volunteers, to date from June 30, 1864.

At this time business connected with his resignation and re-appointment brought Otis to Washington, where he renewed his acquaintance with Surgeon Crane, then on duty in the Surgeon General's Office. Surgeon Crane, while Medical Director of the Department of the South, had been most favorably impressed with the culture and ability of the Massachusetts surgeon, and now so effectually commended him to the Acting Surgeon General as to induce that officer to ask his detail for duty in his office. An order to that effect was issued by the Secretary of War July 22, 1864, and Otis was immediately assigned as an assistant to Surgeon John H. Brinton, U. S. Volunteers, at that time Curator of the Army Medical Museum, and engaged in the duty of collecting materials for the Surgical History of the War of the Rebellion. August 30, 1864, Otis was promoted to the rank of Surgeon of Volunteers, and October 3, 1864, was ordered to relieve Surgeon Brinton of his various duties.

From the first, Otis devoted himself with signal zeal and ability to the large and important duties of his new position. Immediately after he took charge of the Surgical Division he inaugurated a system of record books, which proved ultimately of great service in securing the accurate and complete record of individual cases for use in the Surgical History. The rapidly increasing surgical collection of the Army Medical Museum also received great attention from him, and he expended much time in its supervision and study.

Immediately after the close of the war, the Surgeon General of the Army became desirous of securing, by appropriate legislation, the funds necessary to complete and publish the Medical and Surgical History of the War. Accordingly he called upon Otis, and his colleague, Woodward, who had charge of the collection of materials for the Medical History and of the medical branches of the

Museum, to make reports on the extent and nature of the materials collected for the purpose in question. These reports were published by the Surgeon General November 1, 1865, as "Circular No. 6," for the year 1865. This circular was widely distributed, attracted great attention at the time, and satisfactorily attained the object which led to its publication. It formed a quarto volume of 166 pages, with a number of illustrations intended to indicate the character of those regarded as desirable for the Medical and Surgical History. The first half of the volume was occupied by the Surgical Report prepared by Otis. It was a thoughtfully prepared document, which excited the universal admiration of military surgeons in Europe as well as in America.

It became necessary after the close of the war to retain many of the staff surgeons of volunteers in the service for duty in the general hospitals or other purposes after the great armies had been disbanded, and Otis was, of course, retained with that rank as long as possible; but it was foreseen that the great work he had commenced would occupy a number of years, and he was induced to make arrangements for entering the army as an assistant surgeon. Accordingly he passed the examination prescribed by law, and February 28, 1866, received an appointment as Assistant Surgeon, U. S. Army, but he was not finally mustered out of service as surgeon of volunteers until June 4, 1866, and hence did not accept his commision as Assistant Surgeon U. S. A., until the 6th of that month.

Meanwhile Otis was devoting himself to the study and arrangement of the materials collected for the Surgical History with indefatigable energy, and while engaged upon that work received authority to publish two preliminary studies on special subjects connected therewith, which greatly increased the reputation he had won by his report in Circular No. 6. The first was *A Report on Amputation at the Hip-joint in Military Surgery*, published as Circular No. 7, Surgeon General's Office, July 1, 1867. In this he not merely presented and analyzed the histories of the several amputations at this joint reported to the Surgeon General's Office during the civil war, but discussed with the critical abilities of a master the whole literature of the subject so far as it was at the time accessible to him. An examination of this monograph shows that he had already pretty well begun to emancipate himself from the leading-strings of the French school, and had fully acquired the desire, so manifest in his subsequent work, to compare and weigh all accessible human knowledge on each branch of his subject before arriving at his own conclusions.

These characteristics were, if possible, still more fully displayed in the second of the studies referred to: *A Report on Excisions of the Head of the Femur for Gunshot Injury*, published as Circular No. 2, Surgeon General's Office, January 2, 1869; a monograph in which the subject was treated in a manner similar to that of Circular No. 7, but with a still greater wealth of literary resources. The appearance of each of these monographs was welcomed with acclamations of praise, in which the authoritative expressions of approval by the recognized masters of European surgery were united with the encomiums of the American military surgeons.

Great interest in the forthcoming Surgical History of the War was excited by these publications, and very high expectations were formed, which, however, were fully realized by the character of the *First Surgical Volume.* This volume was issued in 1870. It treated of the special wounds and injuries of the head, face, neck, spine, and chest, was richly illustrated, and discussed the vast amount of material collected during the civil war, in connection with the several subjects treated, with characteristic learning and ability. The *Second Surgical Volume* was issued in 1876. It treated of the wounds and injuries of the abdomen, pelvis, back, and upper extremities. Fully equal in interest and execution to the first volume, it was much more voluminous. The two volumes represent a prodigious amount of patient labor on the part of the editor. The extremely favorable manner in which they were received in surgical circles at home and abroad is well known.

During the interval between the appearance of these two volumes, and subsequently, Otis found time to prepare and publish several valuable reports on subjects connected with military surgery, of which the most important were: *A Report of Surgical Cases treated in the Army of the United States from* 1865 *to* 1871, issued as Circular No. 3 from the Surgeon General's Office, August 17, 1871, *A Report on a Plan for Transporting Wounded Soldiers by Railway in time of War,* Surgeon General's Office, 1875; and *A Report on the Transport of Sick and Wounded by Pack Animals,* issued as Circular No. 9 from the Surgeon General's Office in 1877. A full list of his official and other publications would occupy too much space to be presented in this place.

In the midst of this successful but laborious career, during the month of May, 1877, his health, never very robust, gave way, and, although he survived for several years, he was a constant invalid, to whom death came in the end as a welcome release from suffering. He was engaged at the time of his death on the third surgical volume, which he has left in an unfinished condition; a colossal fragment that must require great labor to complete in a manner worthy of the first two volumes.

Otis received the appointments of captain, major, and lieutenant-colonel by brevet, to date from September 29, 1866, "for faithful and meritorious services during the war." He was promoted to be surgeon in the army, with the rank of major, March 17, 1880. He was elected a foreign member of the Medical Society of Norway, October 26, 1870; a foreign corresponding member of the Surgical Society of Paris, August 11, 1875; and an honorary life member of the Massachusetts Medical Society in February, 1877. He was also at the time of his death a member of the Philosophical Society of Washington, and of the Academy of Natural Sciences of Philadelphia.

In expressing his high appreciation of the character and value of the surgical works of his late colleague, the writer of these pages does but echo the universal language of competent critics throughout the civilized world. On all sides the opinion has been expressed that they have not only made the name of Otis illustrious, but have reflected the greatest credit upon the intelligent liberality of the Government of the United States, and upon the Medical Corps of the Army.

During his connection with the Museum, Otis always took deep interest in the anatomical collection, now embracing about two thousand human crania. As early as January, 1873, the Surgeon General at his instance made a fruitless endeavor to procure an appropriation for the publication of an illustrated catalogue of this valuable collection. To facilitate this object Otis prepared a check-list of the specimens, which was printed in 1876, but the pecuniary means for preparing and publishing the larger work have not yet been provided.

Until his last illness Otis retained much of the fondness for polite literature which characterized him in early life. He had, moreover, considerable taste for music and the fine arts. These qualities made his companionship charming to those who enjoyed his intimacy.

Hesitating, often embarassed, in his manner in ordinary conversation, especially with strangers, he became eloquent when warmed by the discussion of any topic in which he took interest, and he took interest in a great variety of subjects besides those directly connected with the work of his life.

Many warm personal friends share the grief of his family at his untimely death, which, as has been well said by the Surgeon-General, "will be deeply deplored not only by the Medical Corps of the Army, but by the whole medical profession at home and abroad."

LIST OF THE PUBLICATIONS OF G. A. OTIS, M. D., Etc.

Case of Pericarditis in a child of four years and seven months of age. [Reported to the Medico-Chirurgical Society of Richmond, March 1, 1853.] The Virginia Medical and Surgical Journal, Vol. I, 1853, p. 33.

On Hemorrhage from the Umbilicus in new-born Infants. Same Journal, Vol. II, 1853, p. 49.

A Report of a Case in which an Enlargement of the Isthmus of the Thyroid Body was successfully extirpated. Same Vol., p. 115.

On the Per-chloride of Iron in the Treatment of Aneurisms. [Remarks appended to a translation of an article by *Malgaigne*: "De l'emploi du perchlorure de fer dans le Traitement des Anéurismes." L'Abeille Médicale, Octobre, 1853, p. 292 *et seq.*] Same Vol., pp. 295 and 497.

On the Local Treatment of Erysipelas. [Abstract of remarks made in the Medico-Chirurgical Society of Richmond, January 17, 1854.] Same Journal, Vol. III, 1854, p. 13.

Translation, with Notes, of Velpeau's Review of the Surgical Clinique of La Charité, during the Scholastic Year of 1853–4. [Translated from Le Moniteur des Hopitaux, 1854, p. 801 *et seq.*] Same Journal, Vol. IV, 1855, pp. 31, 111, and 321, and Vol. V, 1855, pp. 213, 298, and 378.

Remarks and Excerpts relating to Variola and Vaccinia. Virginia Medical Journal, Vol. VII, 1856, p. 109.

On Strangulated Hernia in Children. Same Journal, Vol. X, 1858, p. 201.

Letter to the Surgeon General of Massachusetts on the Sanitary Condition of the 27th Mass. Vols., from Camp Reed, near Springfield, Mass., October 5, 1861. The Boston Medical and Surgical Journal, Vol. 65, 1862, p. 204.

Letter to the same, on the same, from Camp Springfied, near Annapolis, Md. Same Vol., p. 435.

Letter to the same, from Newbern, N. C., March 28, 1862, [giving an account of the participation of the regiment in the battle of Newbern, and of his management of the wounded.] Same Journal, Vol. 66, 1862, p. 237.

The Surgical portion of (pp. 1-88) *Circular No. 6, War Department, Surgeon General's Office, November 1, 1865.* Reports on the extent and nature of the materials available for the preparation of a Medical and Surgical History of the Rebellion. Printed for the Surgeon General's Office by J. B. Lippincott & Co., Philadelphia, 1865, 4to., pp. 88.

Circular No. 7, War Department, Surgeon General's Office, Washington, July 1, 1867. A Report on Amputations at the Hip-joint in Military Surgery. 4to., pp. 87.

Observations on some Recent Contributions to the Statistics of Excisions and Amputations at the Hip for Injury. The American Journal of the Medical Sciences, Vol. LVI, July, 1868, p. 128.

Rejoinder to a Reply to a Review of Dr. Eve's Contribution on the History of Hip-joint Operation. The Buffalo Medical and Surgical Journal, Vol. VIII, August, 1868, p. 21.

Circular No. 2, War Department, Surgeon General's Office, Washington, January 2, 1869. A Report on Excision of the Head of the Femur for Gunshot Injury. 4to., pp, 141.

Medical and Surgical History of the War of the Rebellion, 1861–1865, Part 1, Vol. II, being the First Surgical Volume. Washington, Government Printing Office, 1870, 4to., pp. 650. Second issue, 1875.

Circular No. 3, War Department, Surgeon General's Office, Washington, August 17, 1871. A Report of Surgical Cases treated in the Army of the United States from 1865 to 1871. 4to., pp. 196.

Memorandum of a Case of Re-amputation at the Hip, with Remarks on the Operation. The American Journal of the Medical Sciences, Vol. LXI, January, 1871, p. 141.

A Report on the Plan for Transporting Wounded Soldiers by Railway in time of War. Washington, Surgeon General's Office, 1875, 8vo., pp. 56.

Description of Selected Specimens from the Surgical Section of the Army Medical Museum at Washington. [International Exhibition of 1876.] Gibson Bros., Washington, 1876, 8vo., pp. 22.

Description of the U. S. Army Medicine Transport Cart, Model of 1876, prepared in conjunction with Brevet Lieutenant Colonel D. L. Huntington, Assistant Surgeon U. S. A. [International Exhibition of 1876.] Gibson Bros., Washington, 1876, 8vo., pp. 16.

Check-List of Preparations and Objects in the Section of Human Anatomy of the U. S. Army Medical Museum. [International Exhibition of 1876.] Gibson Bros., Washington, 1876, pp. 135. Second edition, Gibson Bros., Washington, 1880, 8vo., pp. 194.

Medical and Surgical History of the War of the Rebellion, 1861–1865, Part II, being the Second Surgical Volume. Washington, Government Printing Office, 1876, 4to., pp. 1024. Second issue, 1877.

Circular No. 9, War Department, Surgeon General's Office, March 1, 1877. A Report to the Surgeon General on the Transport of Sick and Wounded by Pack Animals. 4to., pp. 32.

Report of a Board of Officers to decide on a Pattern of Ambulance Wagon for Army Use. [Prepared by him as recorder of the board.] Washington, Government Printing Office, 1878, 8vo., pp. 79.

Contributions from the Army Medical Museum. Boston Medical and Surgical Journal, Vol. XCVI, March, 1877, p. 361.

Article *Surgery* in Johnson's New Universal Cyclopædia. New York, A. J. Johnson & Son, 1878, Vol. IV, pp. 1678–1686.

Notes on Contributions to the Army Medical Museum by Civil Practitioners. Boston Medical and Surgical Journal, Vol. XCVIII, February, 1878, p. 163.

Recent Progress in Military Surgery. Same Vol., April, p. 531.

Photographs of Surgical Cases and Specimens, taken at the Army Medical Museum, with Histories of three hundred and seventy-five cases. Washington, Surgeon General's Office, 1866. 1881, 8 vols., 4to.

The next communication was by Mr. ALEXANDER GRAHAM BELL

UPON A MODIFICATION OF WHEATSTONE'S MICROPHONE AND ITS APPLICABILITY TO RADIOPHONIC RESEARCHES.

In August, 1880, I directed attention to the fact that thin disks or diaphragms of various materials become sonorous when exposed to the action of an intermittent beam of sunlight, and I stated my belief that the sounds were due to molecular disturbances produced in the substance composing the diaphragm.* Shortly afterwards Lord Raleigh undertook a mathematical investigation of the subject, and came to the conclusion that the audible effects were caused by the bending of the plates under unequal heating.† This explanation has recently been called in question by Mr. Preece,‡ who has

* Amr. Ass. for Advancement of Science, Aug. 27, 1881.
† Nature, Vol. XXIII, p. 274.
‡ Roy. Soc., Mar. 10, 1881.

expressed the opinion that although vibrations may be produced in the disks by the action of the intermittent beam, such vibrations are not the cause of the sonorous effects observed. According to him the ærial disturbances that produce the sound arise spontaneously in the air itself by sudden expansion due to heat communicated from the diaphragm; every increase of heat giving rise to a fresh pulse of air. Mr. Preece was led to discard the theoretical explanation of Lord Raleigh on account of the failure of experiments undertaken to test the theory.

He was thus forced, by the supposed insufficiency of the explanation, to seek in some other direction the cause of the phenomenon observed, and, as a consequence, he adopted the ingenious hypothesis alluded to above. But the experiments which had proved unsuccessful in the hands of Mr. Preece were perfectly successful when repeated in America under better conditions of experiment, and the supposed necessity for another hypothesis at once vanished. I have shown in a recent paper read before the National Academy of Science,* that audible sounds result from the expansion and contraction of the material exposed to the beam, and that a real to and fro vibration of the diaphragm occurs capable of producing sonorous effects. It has occurred to me that Mr. Preece's failure to detect with a delicate microphone the sonorous vibrations that were so easily observed in our experiments, might be explained upon the supposition that he had employed the ordinary form of Hughes' microphone shown in Fig. 1, and that the vibrating area was confined to the central portion of the disk. Under such circumstances it might easily happen that both the portions (A B) of the microphone might touch portions of the diaphragm which were practically at rest. It would, of course, be interesting to ascertain whether any such localization of the vibration as that supposed really occured, and I have great pleasure in showing to you to-night the apparatus by means of which this point has been investigated. [See Fig. 2.]

The instrument is a modification of the form of microphone devised in 1827 by the late Sir Charles Wheatstone, and it consists essentially of a stiff wire, (A,) one end of which is rigidly attached to the centre of a metallic diaphragm (B.) In Wheatstone's original arrangement, the diaphragm was placed directly against the ear

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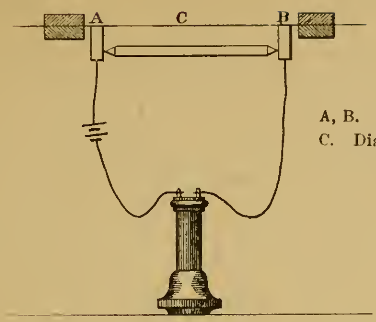
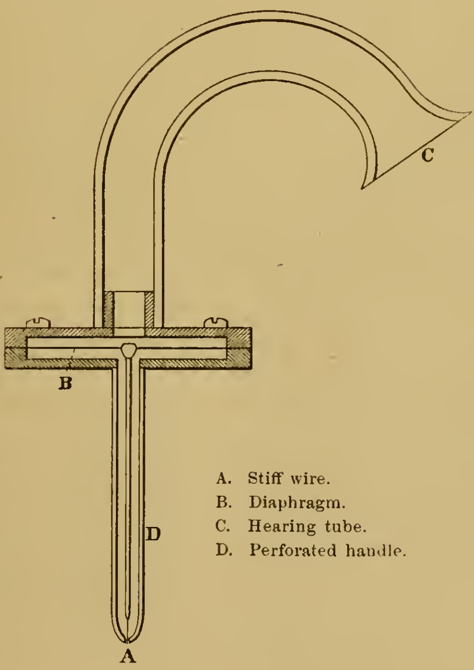
* April 21, 1881.

Fig 1.



A, B. Carbon supports.
C. Diaphragm.

Fig 2.



A. Stiff wire.
B. Diaphragm.
C. Hearing tube.
D. Perforated handle.

and the free extremity of the wire was rested against some sounding body, like a watch. In the present arrangement the diaphragm is clamped at the circumference like a telephone-diaphragm, and the sounds are conveyed to the ear through a rubber hearing-tube (C.) The wire passes through the perforated handle (D,) and is exposed only at the extremity. When the point (A) was rested against the centre of a diaphragm, upon which was focussed an intermittent beam of sunlight, a clear musical tone was perceived by applying the ear to the hearing-tube (C.) The surface of the diaphragm was then explored with the point of the microphone, and sounds were obtained in all parts of the illuminated area, and in the corresponding area on the other side of the diaphragm. Outside of this area on both sides of the diaphragm the sounds became weaker and weaker until at a certain distance from the centre they could no longer be perceived.

At the points where one would naturally place the supports of a Hughes' microphone [see Fig. 1,] no sound was observed. We were also unable to detect any audible effects when the point of the microphone was rested against the support to which the diaphragm was attached. The negative results obtained in Europe by Mr. Preece may, therefore, be reconciled with the positive results obtained in America by Mr. Tainter and myself. A still more curious demonstration of localization of vibration occurred in the case of a large metallic mass. An intermittent beam of sunlight was focussed upon a brass weight (1 kilogram,) and the surface of the weight was then explored with the microphone shown in Fig. 2. A feeble but distinct sound was heard upon touching the surface within the illuminated area, and for a short distance outside, but not in other parts.

In this experiment, as in the case of the thin diaphragm, absolute contact between the point of the microphone, and the surface explored was necessary in order to obtain audible effects. Now, I do not mean to deny that sound waves may be originated in the manner suggested by Mr. Preece, but I think that our experiments have demonstrated that the kind of action described by Lord Raleigh actually occurs and that it is sufficient to account for the audible effects observed.

The next communication was by Mr. J. M. Toner on

EARTH VIBRATIONS AT NIAGARA FALLS.

In June, 1874, the speaker, in company with Dr. J. D. Jackson, of Kentucky, visited the Clifton House on the Canada side of Niagara. On the night of his arrival he was kept awake by the illness of his companion, and his attention was drawn to the frequent rattling of the doors and windows of his room. He was first led to suppose, while speculating upon the cause, that the vibration might be due to pulsations in the air produced by the falling water; but upon further reflection concluded that it could not be satisfactorily explained in that way, as it continued independently of the direction of the wind. On the following day he made it the subject of conversation with others, but no one seemed to agree with him. He had occasion, however, to note when his chair was tilted back against the stone wall of the house that a tremulous motion, or grating was perceptible. At the time this tremor was a novelty to him, but subsequently he had met with allusions to it by several writers. He was led to the following explanation, viz: that the fall of such a large body of water through so great a vertical distance, must necessarily impart vibrations to the massive rocks which form the trough of the river above and below the falls, and that these vibrations are transmitted through the earth itself. To test this theory, he made on the next day the following experiments: A large carving dish holding water was placed on the rock between the falls and the hotel. Upon the water was poured some sweet oil, and it was seen that wave-rings appeared on the surface of the water. These rings were made more distinct by placing a mirror so as to view them by reflection. No rhythm was detected in these vibrations. The dish was placed in many localities, more than thirty in number, and at varying distances from the falls. Waves were observed in it from the Burning Spring above the falls, and as far as half a mile below the small suspension bridge. They were also noted on the steps of the little Episcopal Church, a mile west of the Hotel on the Canada side. Similar results were obtained on the American side.

At the conclusion of Mr. Toner's remarks the Society adjourned to October 8th.

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BULLETIN

OF THE

PHILOSOPHICAL SOCIETY

OF

WASHINGTON.

VOL. V.

Containing the Minutes of the Society from the 203d Meeting, October 8, 1881, to the 226th Meeting, Dec. 16, 1882.

PUBLISHED BY THE CO-OPERATION OF THE SMITHSONIAN INSTITUTION.

WASHINGTON:
1883.

JUDD & DETWEILER, PRINTERS,
WASHINGTON, D. C.

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CONSTITUTION, STANDING RULES,

AND

LIST OF OFFICERS AND MEMBERS

OF

THE PHILOSOPHICAL SOCIETY

OF

WASHINGTON.

CONSTITUTION

OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

Arlicle I. The name of this Society shall be The Philosophical Society of Washington.

Article II. The officers of the Society shall be a President, four Vice-Presidents, a Treasurer, and two Secretaries.

Article III. There shall be a General Committee, consisting of the officers of the Society and nine other members.

Article IV. The officers of the Society and the other members of the General Committee shall be elected annually by ballot; they shall hold office until their successors are elected, and shall have power to fill vacancies.

Article V. It shall be the duty of the General Committee to make rules for the government of the Society, and to transact all its business.

Article VI. This constitution shall not be amended except by a three-fourths vote of those present at an annual meeting for the election of officers, and after notice of the proposed change shall have been given in writing at a stated meeting of the Society at least four weeks previously.

STANDING RULES

FOR THE GOVERNMENT OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

JANUARY, 1881.

---

1. The Stated Meetings of the Society shall be held at 8 o'clock P. M. on every alternate Saturday; the place of meeting to be designated by the General Committee.

2. Notice of the time and place of meeting shall be sent to each member by one of the Secretaries.

When necessary, Special Meetings may be called by the President.

3. The Annual Meeting for the election of officers shall be the last stated meeting in the month of December.

The order of proceedings (which shall be announced by the Chair) shall be as follows:

First, the reading of the minutes of the last Annual Meeting.

Second, the presentation of the annual reports of the Secretaries, including the announcement of the names of members elected since the last annual meeting.

Third, the presentation of the annual report of the Treasurer.

Fourth, the announcement of the names of members who having complied with Section 12 of the Standing Rules, are entitled to vote on the election of officers.

Fifth, the election of President.

Sixth, the election of four Vice-Presidents.

Seventh, the election of Treasurer.

Eighth, the election of two Secretaries.

Ninth, the election of nine members of the General Committee.

Tenth, the consideration of Amendments to the Constitution of the Society, if any such shall have been proposed in accordance with Article VI of the Constitution.

Eleventh, the reading of the rough minutes of the meeting.

4. Elections of officers are to be held as follows:

In each case nominations shall be made by means of an informal ballot, the result of which shall be announced by the Secretary; after which the first formal ballot shall be taken.

In the ballot for Vice-Presidents, Secretaries, and Members of the General Committee, each voter shall write on one ballot as many names as there are officers to be elected, viz., four on the first ballot for Vice-Presidents, two on the first for Secretaries, and nine on the first for Members of the General Committee; and on each subsequent ballot as many names as there are persons yet to be elected; and those persons who receive a majority of the votes cast shall be declared elected.

If in any case the informal ballot result in giving a majority for any one, it may be declared formal by a majority vote.

5. The Stated Meetings, with the exception of the annual meeting, shall be devoted to the consideration and discussion of scientific subjects.

The Stated Meeting next preceding the Annual Meeting shall be set apart for the delivery of the President's Annual Address.

6. Sections representing special branches of science may be formed by the General Committee upon the written recommendation of twenty members of the Society.

7. Persons interested in science, who are not residents of the District of Columbia, may be present at any meeting of the Society, except the annual meeting, upon invitation of a member.

8. Similar invitations to residents of the District of Columbia, not members of the Society, must be submitted through one of the Secretaries to the General Committee for approval.

9. Invitations to attend during three months the meetings of the Society and participate in the discussion of papers, may, by a vote of nine members of the General Committee, be issued to persons nominated by two members.

10. Communications intended for publication under the auspices of the Society shall be submitted in writing to the General Committee for approval.

11. New members may be proposed in writing by three members of the Society for election by the General Committee: but no person shall be admitted to the privileges of membership unless he signifies his acceptance thereof in writing within two months after notification of his election.

12. Each member shall pay annually to the Treasurer the sum of five dollars, and no member whose dues are unpaid shall vote at the annual meeting for the election of officers, or be entitled to a copy of the Bulletin.

In the absence of the Treasurer, the Secretary is authorized to receive the dues of members.

The names of those two years in arrears shall be dropped from the list of members.

Notice of resignation of membership shall be given in writing to the General Committee through the President or one of the Secretaries.

13. The fiscal year shall terminate with the Annual Meeting.

14. Members who are absent from the District of Columbia for more than twelve months may be excused from payment of the annual assessments, in which case their names shall be dropped from the list of members. They can, however, resume their membership by giving notice to the President of their wish to do so.

15. Any member not in arrears may, by the payment of one hundred dollars at any one time, become a life member, and be relieved from all further annual dues and other assessments.

All moneys received in payment of life membership shall be invested as portions of a permanent fund, which shall be directed solely to the furtherance of such special scientific work as may be ordered by the General Committee.

STANDING RULES

OF THE

GENERAL COMMITTEE OF THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

JANUARY, 1881.

---

1. The President, Vice-Presidents, and Secretaries of the Society shall hold like offices in the General Committee.

2. The President shall have power to call special meetings of the Committee, and to appoint Sub-Committees.

3. The Sub-Committees shall prepare business for the General Committee, and perform such other duties as may be entrusted to them.

4. There shall be two Standing Sub-Committees; one on Communications for the Stated Meetings of the Society, and another on Publications.

5. The General Committee shall meet at half-past seven o'clock on the evening of each Stated Meeting, and by adjournment at other times.

6. For all purposes except for the amendment of the Standing Rules of the Committee or of the Society, and the election of members, six members of the Committee shall constitute a quorum.

7. The names of proposed new members recommended in conformity with Section 11 of the Standing Rules of the Society, may be presented at any meeting of the General Committee, but shall lie over for at least four weeks before final action, and the concur-

rence of twelve members of the Committee shall be necessary to election.

The Secretary of the General Committee shall keep a chronological register of the elections and acceptances of members.

8. These Standing Rules, and those for the government of the Society, shall be modified only with the consent of a majority of the members of the General Committee.

RULES

FOR THE

PUBLICATION OF THE BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

JANUARY, 1881.

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1. The President's annual address shall be published in full.

2. The annual reports of the Secretaries and of the Treasurer shall be published in full.

3. When directed by the General Committee, any communication may be published in full.

4. Abstracts of papers and remarks on the same will be published, when presented to the Secretary by the author in writing within two weeks of the evening of their delivery, and approved by the Committee on Publications. Brief abstracts prepared by one of the Secretaries and approved by the Committee on Publications may also be published.

5. Communications which have been published elsewhere, so as to be generally accessible, will appear in the Bulletin by title only, but with a reference to the place of publication, if made known in season to the Committee on Publications.

NOTE. *The attention of members to the above rules is specially requested.*

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

ELECTED DECEMBER 17, 1881.

President .... WILLIAM B. TAYLOR.

Vice Presidents ...... J. K. BARNES, J. E. HILGARD,
J. C. WELLING, J. J. WOODWARD.

Treasurer .... CLEVELAND ABBE.

Secretaries .... MARCUS BAKER, T. N. GILL.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

J. S. BILLINGS, WILLIAM HARKNESS,
C. E. DUTTON, GARRICK MALLERY,
J. R. EASTMAN, SIMON NEWCOMB,
E. B. ELLIOTT, J. W. POWELL,
C. A. SCHOTT.

STANDING COMMITTEES.

On Communications:

MARCUS BAKER, *Chairman.* C. E. DUTTON, T. N. GILL.

On Publications:

T. N. GILL, *Chairman.* CLEVELAND ABBE, S. F. BAIRD,* MARCUS BAKER.

* As Secretary of the Smithsonian Institution.

LIST OF MEMBERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

Corrected to May, 1882.

(*a*) indicates a *founder* of the Society.
(*b*) indicates *deceased*.
(*c*) indicates *absent* from the District of Columbia and excused from payment of dues until announcing their return.
(*d*) indicates *resigned*.
(*e*) indicates *dropped* for non-payment or nothing known of him.

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Abbe, Cleveland	Army Signal Office. 2017 I St. N. W.	1871, Oct. 29
Abert, Sylvanus Thayer	Engineer's Office, War Department. 1724 Penn. Ave. N.W.	1875, Jan. 30
Adams, Henry	1605 H St	1881, Feb. —
Aldis, Asa Owen	1617 Rhode Island Ave. N. W	1873, Mar. 1
Allen, James	Army Signal Office. 1707 G St. N. W.	1882, Feb. 25
Alvord, Benjamin	1207 Q St. N. W	1872, Mar. 23
Antisell, Thomas (*a*)	Patent Office. 1311 Q St. N. W	1871, Mar. 13
Avery, Robert Stanton	Coast and Geodetic Survey Office. 320 A St. S. E.	1879, Oct. 11
Babcock, Orville Elias	2024 G St. N. W	1871, June 9
Bailey, Theodorus (*b*)		1873, Mar. 1
Baird, Spencer Fullerton (*a*)	Smithsonian Institution. 1445 Mass. Ave. N. W.	1871, Mar. 13
Baker, Frank	326 C St. N. W	1881, May 14
Baker, Marcus	Coast and Geodetic Survey Office. 1205 Rhode Island Ave. N. W.	1876, Mar. 11
Bancroft, George	1623 H St. N. W	1875, Jan. 16
Barnes, Joseph K (*a*)	Surg. Genl's Office. 1723 H St. N. W.	1871, Mar. 13
Bartley, Thomas Welles	Office, 1343 F St. N.W. Res., 1016 13th St. N. W.	1873, Mar. 29
Bates, Henry Hobart	Patent Office. 1313 R St. N. W	1871, Nov. 4
Beardslee, Lester Anthony (*c*)	Navy Department.	1875, Feb. 27
Bell, Alexander Graham	1221 Conn. Ave. N.W. Res., 1302 Conn. Ave. N. W.	1879, Mar. 29
Bell, Chichester Alexander	1221 Conn. Ave. N.W. Res., 2023 Mass. Ave. N. W.	1881, Oct. 8
Benét, Stephen Vincent (*a*)	Ordnance Office, War Department. 1717 I St. N. W.	1871, Mar. 13
Bessels, Emil	Smithsonian Institution. 1441 Mass. Ave. N. W.	1875, Jan. 16
Billings, John Shaw (*a*)	Surg. Genl's Office. 3027 N St. N. W.	1871, Mar. 13
Birney, William	330 4½ St. N.W. Res., 1901 Harewood Ave., Le Droit Park.	1879, Mar. 29
Birnie, Rogers (*c*)	Cold Spring, Putnam Co., N. Y.	1876, Mar. 11
Burchard, Horatio Chapin	Director of the Mint, Treasury Dept. Res., Riggs House.	1879, May 10
Burnett, Swan Moses	1215 I St. N. W	1879, Mar. 29
Busey, Samuel Clagett	1525 I St. N. W	1874, Jan. 17
Capron, Horace (*a*)	The Portland	1871, Mar. 13
Case, Augustus Ludlow (*c*)	Navy Department. Bristol, R. I	1872, Nov. 16
Casey, Thomas Lincoln (*a*)	Engineer Bureau, War Department. 1419 K St. N. W.	1871, Mar. 13

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Caziarc, Louis Vasmer	Army Signal Office. 1446 N St. N. W.	1882, Feb. 25
Chase, Salmon Portland (*a b*)		1871, Mar. 13
Chickering, John White, Jr.	Deaf Mute College, Kendall Green	1874, Apr. 11
Christie, Alexander Smyth	Coast and Geodetic Survey Office. 1102 14th St. N. W.	1880, Dec. 4
Clapp, William Henry	Army Signal Office. 806 18th St. N.W.	1882, Feb. 25
Clark, Edward	Architect's Office, Capitol. 417 4th St. N. W.	1877, Feb. 24
Clark, Ezra Westcott	Revenue Marine Bureau, Treasury Department. Res., Woodley road.	1882, Mar. 25
Clarke, Frank Wigglesworth (*c*)	University of Cincinnati. Albion Place, Cincinnati, Ohio.	1874, Apr. 11
Coffin, John Huntington Crane (*a*)	1901 I St. N. W.	1871, Mar. 13
Collins, Frederick (*b*)		1879, Oct. 21
Comstock, John Henry	Cornell University, Ithaca, N. Y.	1880, Feb. 14
Coues, Elliott	Smithsonian Inst. 1321 N St. N. W	1874, Jan. 17
Craig, Benjamin Faneuil (*a b*)		1871, Mar. 13
Craig, Robert	Army Signal Office. 1008 I St. N. W.	1873, Jan. 4
Craig, Thomas	Johns Hopkins Univ., Baltimore, Md.	1879, Nov. 22
Crane, Charles Henry (*a*)	Surg. Genl's Office. 1909 F St. N. W.	1871, Mar. 13
Curtis, Josiah	428 7th street N. W. Riggs House	1874, Mar. 28
Cutts, Richard Dominicus	Coast and Geodetic Survey Office. 1725 H St. N. W.	1871, Apr. 29
Dall, William Healey (*a*)	P. O. Box 406. 1119 12th St. N. W.	1871, Mar. 13
Davis, Charles Henry (*b*)		1874, Jan. 17
Davis, Charles Henry	Navy Department. 1705 Rhode Island Ave. N. W.	1880, June 19
Dean, Richard Crain (*b*)		1872, Apr. 23
De Caindry, William Augustin	Commissary General's Office. 924 19th St. N. W.	1881, Apr. 30
De Land, Theodore Louis	Treasury Dept. 126 7th St. N. E.	1880, Dec. 18
Dewey, George (*d*)	Light House Board. 826 14th St. N.W.	1879, Feb. 15
Doolittle, Myrick Hascall	Coast and Geodetic Survey Office. 1925 I St. N. W.	1876, Feb. 12
Dorr, Fredric William (*b*)		1874, Jan. 17
Dunwoody, Henry Harrison Chase	Army Signal Office. 1412 G St. N. W.	1873, Dec. 20
Dutton, Clarence Edward	Geological Survey	1872, Jan. 27
Dyer, Alexander B. (*a b*)		1871, Mar. 13
Eastman, John Robie	Naval Observatory. 2721 N St. N. W.	1871, May 27
Eaton, Amos Beebe (*a b*)		1871, Mar. 13
Eaton, John	Bureau of Education, Interior Dept. 712 East Capitol St.	1874, May 8
Eldredge, Stewart (*c*)		1871, June 9
Elliot, George Henry (*a d*)	Engineer Bureau, War Department	1871, Mar. 13
Elliott, Ezekiel Brown (*a*)	Mint Bureau, Treasury Department. 607 I St. N. W.	1871, Mar. 13
Endlich, Frederic Miller	Smithsonian Institution	1873, Mar. 1
Ewing, Charles (*c*)		1874, Jan. 17
Ewing, Hugh (*c*)	Lancaster, Ohio	1874, Jan. 17
Farquhar, Edward Jessop	Patent Office Library. 1915 H St. N.W.	1876, Feb. 12
Farquhar, Henry	Coast and Geodetic Survey Office. 726 20th St. N. W.	1881, May 14
Ferrel, William	Coast and Geodetic Survey Office. 471 C St. N. W.	1872, Nov. 16
Fletcher, Robert	Surgeon Genl's Office. 314 Ind. Ave.	1873, Apr. 10
Flint, Albert Stowell	Naval Observatory. 1209 Rhode Island Ave. N. W.	1882, Mar. 25
Flint, James Milton	Smithsonian Inst. Riggs House	1881, Mar. 26
Foote, Elisha (*a c*)		1871, Mar. 13
Foster, John Gray (*b*)		1873, Jan. 18
French, Henry Flagg	Treasury Department. 137 East Capitol St.	1882, Mar. 25
Frisby, Edgar	Naval Observatory. 3006 P St. N. W.	1872, Nov. 16
Fristoe, Edward T	Columbian College. College Hill N.W.	1873, Mar. 29
Gale, Leonard Dunnell	1230 Mass. Ave. N. W.	1874, Jan. 17
Gallaudet, Edward Miner	Deaf Mute College, Kendall Green	1875, Feb. 27
Gannett, Henry	Geological Survey. 1881 Harewood Ave., Le Droit Park.	1874, Apr. 11
Gardner, James Terry (*c*)	State Library, Albany, N. Y.	1874, Jan. 17
Garnett, Alexander Young P. (*d*)	1317 N. Y. Ave. N. W.	1878, Mar. 16
Gihon, Albert Leary	Navy Department. 1736 I St. N. W.	1880, Dec. 18

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Gilbert, Grove Karl	Geological Survey. Le Droit Park	1873, June 7
Gill, Theodore Nicholas (*a*)	Smithsonian Inst. 321-323 4½ St. N. W.	1871, Mar. 13
Godding, William Whiting	Government Asylum for the Insane	1879, Mar. 29
Goode, George Brown	National Museum. 1620 Mass. Av. N. W.	1874, Jan. 31
Goodfellow, Edward	Coast and Geodetic Survey Office	1875, Dec. 18
Goodfellow, Henry (*d*)	Bureau of Military Justice, War Dept.	1871, Nov. 4
Gore, James Howard	Columbian College. 1305 Q St. N. W.	1880, Mar. 14
Graves, Edward Oziel (*c*)		1874, Apr. 11
Graves, Walter Hayden (*c*)	Denver, Colorado	1878, May 25
Greely, Adolphus Washington (*c*)		1880, June 19
Green, Bernard Richardson	1738 N St. N. W	1879, Feb. 15
Green, Francis Mathews	Bureau of Navigation, Navy Dept	1875, Nov. 9
Greene, Benjamin Franklin (*a c*)	West Lebanon, N. H	1871, Mar. 13
Greene, Francis Vinton	War Department. 1915 G St. N. W	1875, Apr. 10
Gunnell, Francis Mackall (*c*)	600 20th St. N. W	1879, Feb. 1
Hains, Peter Conover (*c*)	Office Light House Engineer, Charleston, S. C.	1879, Feb. 15
Hall, Asaph (*a*)	Naval Observatory. 2715 N St. N. W.	1871, Mar. 13
Hanscom, Isaiah (*b*)		1873, Dec. 20
Harkness, William (*a*)	Naval Observatory. 1415 G St. N. W.	1871, Mar. 13
Hassler, Ferdinand Augustus (*c*)	Tustin City, Los Angeles Co., Cal	1880, May 8
Hayden, Ferdinand Vandeveer (*ac*)	Geological Survey. 1803 Arch street, Philadelphia, Penna.	1871, Mar. 13
Hazen, Henry Allen	Army Signal Office. 1209 R. I. Av. N. W.	1882, Mar. 25
Hazen, William Babcock	Army Signal Office. 1601 K St. N. W.	1881, Feb. —
Henry, Joseph (*a b*)		1871, Mar. 13
Henshaw, Henry Wetherbee	Bureau of Ethnology. 903 M St. N. W.	1874, Apr. 11
Hilgard, Julius Erasmus (*a*)	Coast and Geodetic Survey Office. 1709 Rhode Island Ave. N. W.	1871, Mar. 13
Hill, George William	Nautical Almanac Office. 318 Ind. Ave. N. W.	1879, Feb. 1
Holden, Edward Singleton (*c*)	Madison, Wisconsin	1873, June 21
Holmes, William Henry	Geological Survey	1879, Mar. 29
Hough, Franklin Benjamin (*c*)	Agricultural Department	1879, Mar. 29
Howell, Edwin Eugene (*c*)	Rochester, N. Y	1874, Jan. 31
Howgate, Henry W		1873, Jan. 18
Humphreys, Andrew Atkinson (*a*)	S. E. Corner 15th and K Sts. N. W.	1871, Mar. 13
Huntington, David Lowe	Army Med. Museum. 1709 M St. N. W.	1877, Dec. 21
Jackson, Henry Arundel Lambe (*c*)	War Department	1875, Jan. 30
James, Owen (*c*)	Hyde Park, Penna	1880, Jan. 3
Jeffers, William Nicolson (*d*)	Navy Department	1877, Feb. 24
Jenkins, Thornton Alexander (*a*)	2115 Penn. Ave. N. W	1871, Mar. 13
Johnson, Arnold Burgess	Light House Board, Treasury Dept. 501 Maple Ave., Le Droit Park.	1878, Jan. 19
Johnson, Joseph Taber	937 New York Ave. N. W	1879, Mar. 29
Johnston, William Waring	1401 H St. N. W	1873, Jan. 21
Kampf, Ferdinand (*b*)		1875, Dec. 18
Keith, Reuel (*c*)		1871, Oct. 29
Kidder, Jerome Henry	Navy Department. 1601 O St. N. W.	1880, May 8
Kilbourne, Charles Evans	Army Signal Office. Lexington House.	1880, June 19
King, Albert Freeman Africanus	726 13th St. N. W	1875, Jan. 16
King, Clarence (*d*)		1879, May 10
Knox, John Jay	Treasury Dept. 1127 10th St. N. W	1874, May 8
Kummell, Charles Hugo	Coast and Geodetic Survey Office. 608 Q St. N. W.	1882, Mar. 25
Lane, Jonathan Homer (*a b*)		1871, Mar. 13
Lawver, Winfield Peter	Mint Bureau, Treasury Department. 1912 I St. N. W.	1881, Feb. 19
Lee, William	2111 Penn. Ave. N. W	1874, Jan. 17
Lincoln, Nathan Smith	1514 H St. N. W	1871, May, 27
Lockwood, Henry H. (*d*)		1871, Oct. 29
Loomis, Eben Jenks	Nautical Almanac Office. 1413 College Hill Terrace N. W.	1880, Feb. 14
Lull, Edward Phelps	Navy Department. 1313 M St. N. W	1875, Dec. 4
Lyford, Stephen Carr (*d*)	Ordnance Office, War Department	1873, Jan. 18
Macauley, Henry Clay (*c*)		1880, Jan. 3
McGuire, Frederick Bauders	1306 F St. N. W. Res., 614 E St. N. W.	1879, Feb. 15
Mack, Oscar A. (*b*)		1872, Jan. 27
McMurtrie, William	Agricultural Dept. 1728 I St. N. W	1876, Feb. 26

NAME.	P. O ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Mallery, Garrick	Bureau of Ethnology. P. O. Box 585. Res., 1323 N St. N. W.	1875, Jan. 30
Marvin, Joseph Badger (*c*)		1878, May 25
Marvine, Archibald Robertson (*b*).		1874, Jan. 31
Mason, Otis Tufton	Columbian College. 1305 Q St. N. W.	1875, Jan. 30
Meek, Fielding Bradford (*a b*)		1871, Mar. 13
Meigs, Montgomery (*c*)	War Department. Rock Island, Ill.	1877, Mar. 24
Meigs, Montgomery Cunningham (*a*)	1239 Vermont Ave. N. W	1871, Mar. 13
Menocal, Aniceto Garcia	Navy Yard, Washington, D. C	1877, Feb. 24
Mew, William Manuel	Army Medical Museum. 942 New York Ave. N. W.	1873, Dec. 20
Milner, James William (*b*)		1874, Jan. 31
Morris, Martin Ferdinand (*c*)	717 12th St. N. W.	1877, Feb. 24
Mussey, Reuben Delavan	P. O. Box 618. Res., 508 5th St. N. W.	1881, Dec. 3
Myer, Albert J. (*a b*)		1871, Mar. 13
Myers, William (*c*)	Office of Commissary General, War Department.	1871, June 23
Newcomb, Simon (*a*)	Navy Department. 1336 11th St. N.W.	1871, Mar. 13
Nichols, Charles Henry (*c*)		1872, May 4
Nicholson, Walter Lamb (*a*)	Topographer of Post Office Dept. 1322 I St. N. W.	1871, Mar. 13
Nordhoff, Charles	New York Herald Bureau. 1027 New York Ave. N. W.	1879, May 10
Osborne, John Walter	212 Delaware Ave. N. E	1878, Dec. 7
Otis, George Alexander(*a b*)		1871, Mar. 13
Packard, Robert Lawrence (*e*)	Patent Office. 2022 G St. N. W	1875, Feb. 27
Parke, John Grubb (*a*)	Engineer Bureau, War Department. 16 16½ St. N. W.	1871, Mar. 13
Parker, Peter (*a*).	2 La Fayette Square	1871, Mar. 13
Parry, Charles Christopher (*c*)	Burlington, Iowa	1871, May 13
Patterson, Carlile Pollock (*b*)		1871, Nov. 17
Paul, Henry Martyn (*c*)	University of Tokio, Japan	1877, May 19
Peale, Albert Charles (*c*).	Schuylkill Haven, Schuylkill Co., Pa.	1874, Apr. 11
Peale, Titian Ramsay (*a c*)		1871, Mar. 13
Peirce, Benjamin (*a b*)		1871, Mar. 13
Peirce, Charles Sanders (*c*)	Coast and Geodetic Survey Office. Res., Baltimore, Md.	1873, Mar. 1
Pilling, James Constantine	Geological Survey. 903 M St. N. W	1881, Feb. 19
Poe, Orlando Metcalfe	Headquarters of the Army. 1507 Rhode Island Ave. N. W.	1873, Oct. 4
Porter, David Dixon (*d*)	1710 H St. N. W	1874, Apr. 11
Powell, John Wesley	Geological Survey. 910 M St. N. W	1874, Jan. 17
Prentiss, Daniel Webster	1224 9th St. N. W	1880, Jan. 3
Pritchett, Henry Smith (*c*)	Washington University, St. Louis, Mo.	1879, Mar. 29
Rathbone, Henry Reed (*c*)		1874, Jan. 17
Ridgway, Robert (*c*)	Smithsonian Inst. 1214 Va. Av. N.W.	1874, Jan. 31
Riley, Charles Valentine	Agricultural Dept. 1700 13th St. N.W.	1878, Nov. 9
Riley, John Campbell (*b*)		1877, May 19
Ritter, William Francis McKnight	Nautical Almanac Office. 16 Grant Place.	1879, Oct. 21
Rodgers, Christopher Raymond Perry (*c*)	1723 I St. N. W	1872, Mar. 9
Rodgers, John (*b*).		1872, Nov. 16
Rogers, Joseph Addison (*c*)	Naval Observatory	1872, Mar. 9
Russell, Israel Cook	Geological Survey	1882, Mar. 25
Sands, Benjamin Franklin (*a*)	816 15th St. N. W	1871, Mar. 13
Saville, James Hamilton	342 D St. (La. Ave.) N. W. Res., 1315 M St. N. W.	1871, Apr. 29
Sawyer, Frederic Adolphus (*e*)		1873, Oct. 4
Schaeffer, George Christian (*a b*)		1871, Mar. 13
Schott, Charles Anthony (*a*)	Coast and Geodetic Survey Office. 212 1st St. S. E.	1871, Mar. 13
Searle, Henry Robinson	1223 10th St. N. W	1877, Dec. 21
Seymour, George Dudley	607 7th St. N.W. Res., 1007 9th St. N.W.	1881, Dec. 3
Shellabarger, Samuel	Room 23, Corcoran Building. Res., 812 17th St. N. W.	1875, Apr. 10
Sherman, John	1317 K St. N. W	1874, Jan. 17
Sherman, William Tecumseh (*a d*)	War Department. 817 15th St. N. W	1871, Mar. 13
Shufeldt, Robert Wilson	Surg. Genl's Office. 819 17th St. N.W.	1881, Nov. 5

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Sicard, Montgomery (*c*)	Ordnance Bureau, Navy Department. 1404 L St. N. W.	1877, Feb. 24
Sigsbee, Charles Dwight	Hydrographic Office, Navy Dept. 3319 U St., West Washington.	1879, Mar. 1
Skinner, Aaron Nicholas (*e*)	Naval Observatory. 1726 10th St. N.W.	1875, Feb. 27
Smith, David (*c*)	Navy Department	1876, Dec. 2
Smith, Edwin	Coast and Geodetic Survey	1880, Oct. 23
Spofford, Ainsworth Rand	Library of Congress. 1621 Mass. Ave. N. W.	1872, Jan. 27
Stearns, John (*c*)		1874, Mar. 28
Stone, Ormond (*c*)	Leander McCormick Observatory, University of Virginia.	1874, Mar. 28
Story, John Patten	Army Signal Office. 921 17th St. N.W.	1880, June 19
Taylor, Frederick William	Smithsonian Institution. 1120 Vermont Ave. N. W.	1881, Feb. 19
Taylor, George (*e*)	804 E St. N. W. Res., 1120 Vermont Ave. N. W.	1873, Mar. 1
Taylor, William Bower (*a*)	Smithsonian Inst. 457 C St. N. W	1871, Mar. 13
Thompson, Almon Harris (*c*)	Ivanpah, Greenwood Co., Kansas	1875, Apr. 10
Tilden, William Calvin (*c*)	Army Medical Museum	1871, Apr. 29
Todd, David Peck (*c*)	Amherst, Mass	1878, Nov. 23
Toner, Joseph Meredith	615 Louisiana Ave	1873, June 7
Twining, William J. (*b*)		1878, Nov. 23
Upton, Jacob Kendrick (*d*)	Cooke & Co., cor. 15th St. and Penn. Ave. 1721 De Sales St.	1878, Feb. 2
Upton, William Wirt	2d Comptroller's Office, Treasury Dept. 810 12th St. N. W.	1882, Mar. 25
Upton, Winslow	Army Signal Office. 1441 Chapin St. N. W.	1880, Dec. 4
Vasey, George	Agricultural Dept. 1437 S St. N. W	1875, June 5
Waldo, Frank	Army Signal Office. 1427 Chapin St. N. W.	1881, Dec. 3
Walker, Francis Amasa (*c*)	Mass. Inst. of Technology, Boston, Mass.	1872, Jan. 27
Ward, Lester Frank	Geological Survey. 1464 R. I. Av. N.W.	1876, Nov. 18
Warren, Charles (*e*)	Bureau of Education. 1208 N St. N. W.	1874, May 8
Webster, Albert Lowry	Geological Survey. P. O. Box 591	1882, Mar. 25
Welling, James Clarke	Columbian College	1872 Nov. 16
Wheeler, George M. (*c*)	Engineer Bureau, War Department	1873, June 7
Wheeler, Junius B (*a c*)	West Point, New York	1871, Mar. 13
White, Charles Abiathar	Geological Survey. Le Droit Park	1876, Dec. 16
White, Zebulon Lewis (*c*)	Providence, Rhode Island	1880, June 19
Wilson, Allen D.	Geological Survey	1874, Apr. 11
Wilson, James Ormond	Franklin School Building. 1439 Mass. Ave. N. W.	1873, Mar. 1
Winlock, William Crawford	Naval Observatory. 1903 F St. N. W.	1880, Dec. 4
Wolcott, Christopher Columbus (*d*)	War Department	1875, Feb. 27
Wood, Joseph (*c*)	Asst. Engineer B. & P. R. R.	1875, Jan. 16
Wood, William Maxwell (*c*)	Navy Department	1871, Dec. 2
Woodward, Joseph Janvier (*a*)	Army Med. Museum. 620 F St. N. W.	1871, Mar. 13
Woodworth, John Maynard (*b*)		1874, Jan. 31
Yarnall, Mordecai (*b*)		1871, Apr. 29
Yarrow, Harry Crécy	814 17th St. N. W	1874, Jan. 31
Zumbrock, Anton	Coast and Geodetic Survey Office. 306 C St. N. W.	1875, Jan. 30

Number of *founders*	44
" members *deceased*	28
" " *absent*	52
" " *resigned*	12
" " *dropped*	5
" " *active*	149
Total number enrolled	246

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

203D MEETING. OCTOBER 8, 1881.

The Society, in accordance with the notice of adjournment at its last June meeting, resumed its sessions.

The President (Mr. J. J. WOODWARD) in the Chair.

Thirty-eight members present.

Mr. G. K. GILBERT read a communication on

THE QUATERNARY CLIMATE OF THE GREAT BASIN.

The matters contained in this communication were a summary of certain chapters which will appear from the pen of Mr. Gilbert in the Second Annual Report of the Director of the United States Geological Survey now in press. The observations of which the communication was a resume were made in his capacity of Geologist in charge of the Exploration of the Utah Division.

Remarks were made on Mr. Gilbert's communication by Mr. THOMÀS ANTISELL.

Mr. E. B. ELLIOTT also made a communication on

ACCRUED INTEREST ON GOVERNMENT SECUTITIES.

Mr. W. B. TAYLOR exhibited to the Society a photographic print from a single negative including about 140 degrees of panorama. The ordinary camera does not usually comprise more than about 60 degrees, and requires as a necessary condition of good definition perfect stability of the lens and the plate. In the present case, an inspection of the two houses presented in the rural view, (especially of the longer one near the middle of the picture,) with the curved road winding between them to the right, shows that a revolving camera was employed; the long sensitive plate having evidently been simultaneously moved transversely in the reverse direction to that of the objective. This perfect co-ordination of the revolving and sliding movements could be obtained by a mechanical gearing; and the extended landscape be thus successively impressed upon advancing portions of the plate—probably through a vertical slit in a diaphragm immediately in front of the plate. That the core-lation of movement has been very perfect is evidenced by the admirable precision of every detail in the photograph. It will be observed that the three men standing in different parts of the field of view are one and the same individual, who has had time to pass behind the instrument, and to twice take a new position in advance of the moving camera. By bending the long card into a concave arc somewhat more than the third of a cylinder, and placing the eye at the axis of curvature, it will be seen that the various slight distortions of perspective (particularly in the houses) are completely corrected.

Mr. J. M. Toner exhibited, *apropos* to the approaching centennial of the surrender of Cornwallis at Yorktown, certain well preserved specimens of coins and medals of national historic interest, viz:

(1.) Bronze copy of medal given to Washington on the evacuation of Boston.

(2.) A bronze copy of a medal of Lafayette.

(3.) A bronze copy of a medal of Columbus.

(4.) A very fine half dollar of 1785.

(5.) A very fine Washington cent of 1791.

204th Meeting. October 22, 1881.

The President in the Chair.

Forty members present.

Mr. A. B. Johnson presented the following communication on

RECENT INVESTIGATIONS BY THE LIGHT-HOUSE BOARD ON THE ANOMALIES OF SOUND FROM FOG SIGNALS.

Among our erroneous popular notions is one which occasionally brings practical men, even ship-masters, to grief. It is the idea that sound is always heard in all directions from its source according to its intensity or force, and according to the distance of the hearer from it. Instances of this fallacy have accumulated, and they are emphasized by shipwrecks caused by the insistance of mariners on the infallibility of their ears, who have accepted unquestioned the guidance of sound signals during fog as they have that of light-houses during clear weather. The fact is, audition is subject to aberrations, and under circumstances where little expected. We have learned by sad experience that implicit reliance on sound signals may, as it has, lead to danger if not to death.

The wreck of the steamer Rhode Island, on Bonnet Point in Narragansett Bay, which happened on November 6, 1880, when a million dollars in property was lost, was caused, it was said, by the failure of the fog-signal on Beaver Tail Point to sound at that time. Thereupon the Light-House Board, which has charge of the sixty and more fog-signals on our coasts, made an investigation which showed that the fog-signal was in full operation when the wreck took place; but it also brought out the fact, that while there was no lack in the volume of the sound emitted by the signal, there was often a decided lack in the audition of that sound, so much so that it would not be heard at the intensity expected, nor at the place expected; indeed it would be heard faintly where it ought to be heard loudly, and loudly where it ought to be heard faintly; that it could not be heard at all at some points, and then further away it could be heard better than near by; that it could be heard and lost and heard and lost again, all within reasonable ear shot, and all this while the signal was in full blast and sounding continuously.

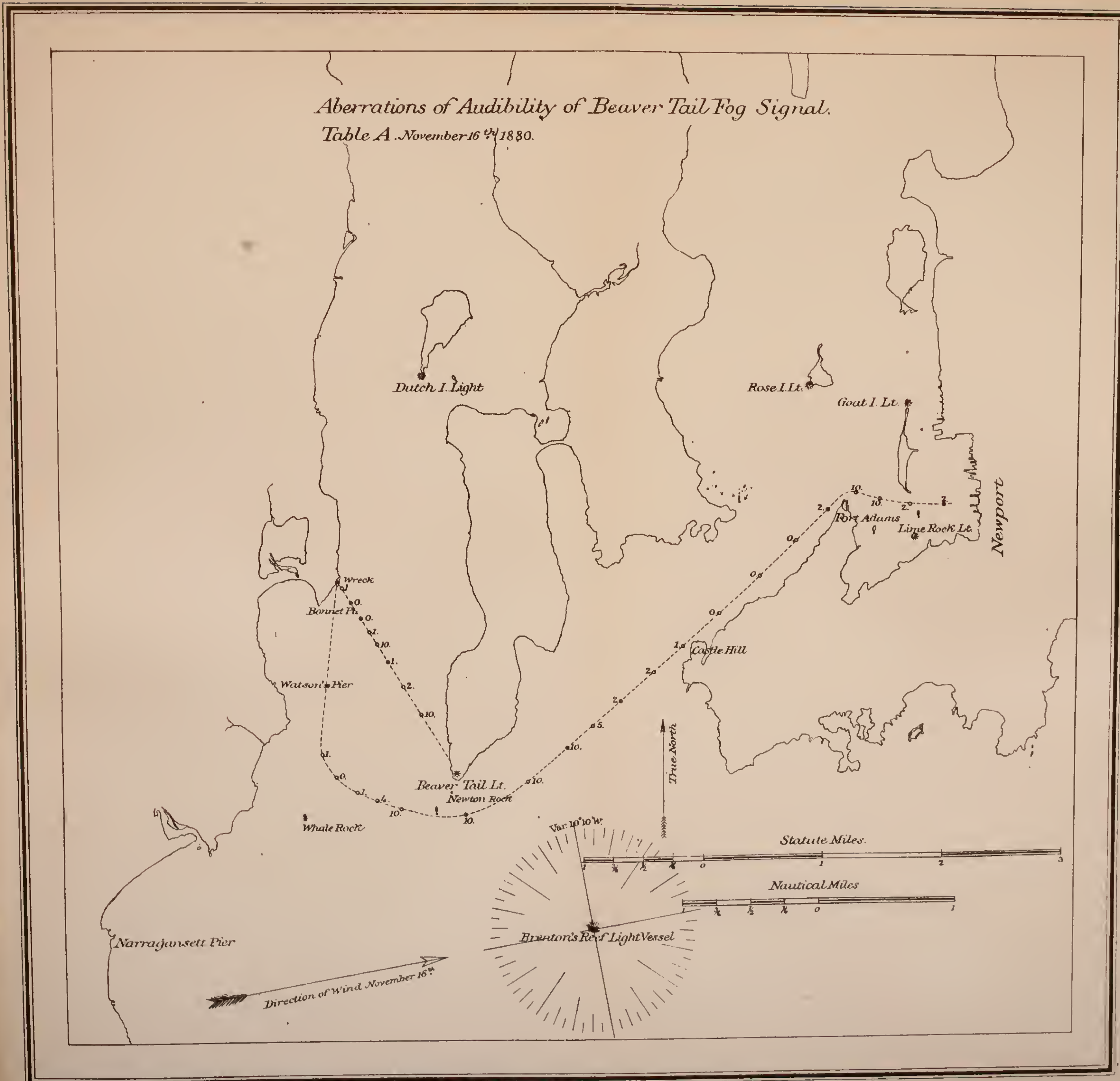
The following table, A, will give the results obtained by the officer of the navy who investigated these phenomena, and reported to the Light-House Board:

Table A.

Observations on Beaver Tail Fog-Signal, Rhode Island, made on November 16, 1880, from a sail-boat, Thermometer at beginning 58°, ending 67°; Wind moderate from the West; Weather clear and cold, with a bright sun. Time, beginning 11.15 A. M.

Number of Observation.	Distance from Beaver Tail Fog-Signal in statute miles.	Intensity of sound in scale of 10.	Remarks.
1	½	10	
2	⅞	2	
3	1 1/16	1	
4	1¼	10	
5	1⅜	1	
6	1½	0	
7	1⅝	0	
8	1⅞	1	Close to Bonnet Point changed course and ran almost due south.
9	1⅛	1	1½ miles from last station.
10	1	0	¼ mile from last station.
11	⅞	1	" " "
12	⅝	4	" " "
13	½	10	" " "
14	⅜	10	About opposite Beaver Tail, ½ mile from last station, and in the axis of trumpet.
15	½	10	About ½ mile from last station, and running for Newport, heading nearly northeast.
16	1	10	About ½ mile from last station.
17	1¼	5	" ½ " "
18	1½	2	" ¼ " "
19	1⅞	2	" ¼ " "
20	2⅛	1	" ½ " "
21	2½	0	" ¼ " "
22	3½	0	" ½ " "
23	3⅞	2	" ½ " "
24	4	10	About ¼ mile from last station, just off Ft. Adams.
25	4¼	10	Under the lee of Fort Adams.
26	4½	2	
27	4⅝	2	
28	4¾	2	
29	5	2	Newport.

Last summer, I had an opportunity while on a light-house steamer, to experience something of the variations in the audition of the Beaver Tail fog-signal. When the steamer left the light-



Aberrations of Audibility of Beaver Tail Fog Signal.
Table A. November 16th 1880.
Dutch I. Light
Rose I. Lt.
Goat I. Lt.
Newport
Fort Adams
Lime Rock Lt.
Wreck
Bonnet Pt.
Watson's Pier
Castle Hill
Beaver Tail Lt.
Newton Rock
Whale Rock
True North
Var. 10°10' W.
Statute Miles.
Nautical Miles
Brenton's Reef Light Vessel
Narragansett Pier
Direction of Wind November 16th

house landing, the fog-signal was to sound for a given time, and to commence when the steamer had reached a given point, half a mile distant. When that point was reached, we could see by the steam-puffs coming from the 'scape pipe, that the signal was being blown; but we could not hear its sound; nor did we, as we continued on our course, running away from the light station for the next five minutes. When near to Whale Rock, less than a mile and a half distant from the signal, the steamer was stopped, silence was ordered fore and aft, and we all listened intently. The expert naval officers thought they heard a trace of the fog-signal, but my untrained ears failed to differentiate it from the moan of the whistling buoy close to us. Yet the blasts of the ten-inch steam whistle, for which we were listening, can often be heard at a distance of ten miles.

Soon after, I had another opportunity to further observe the operations of this signal. We left Narragansett Pier, R. I., on Aug. 6, 1881, at 4 P. M., in a dense fog, with a strong breeze from the W. S. W., and a heavy chop sea. We wished to ascertain how far the Beaver Tail fog-signal could be heard dead to windward and in the heaviest of fogs. At Whale Rock, one and one-third miles from it, we did not hear a trace of it. Then the steamer was headed directly for Beaver Tail Point, and we ran slowly for it by compass, until the pilot stopped the steamer, declaring we were almost aboard of the signal itself. Every one strained his ears to hear the signal but without success; and we had begun to doubt of our position when, the fog lifting slightly, we saw the breakers in altogether too close proximity for comfort. We passed the point as closely as was safe; and, when abreast of it and at right angles with the direction of the wind, the sound of the fog-signal broke on us suddenly and with its full power. We then ran down the wind to Newport, and carried the sound with us all the way. The fog continuing during the next day, the signal kept up its sound, and we heard it distinctly and continuously at our wharf, though five miles distant.

On the night of May 12, 1881, about midnight, the Galatea, a propeller of over 1500 tons burden, with a full load of passengers and freight, bound through Long Island Sound from Providence to New York, grounded in a dead calm and a dense fog on Little Gull Island, about one-eighth of a mile from and behind the fog-signal, and got off two days later without damage to herself or loss of life or freight. It was as usual alleged that the fog-signal, a steam siren, at Little Gull Light, was not in operation at the time of the accident, and the Light-House Board, also, as usual, immediately ordered an investigation. This was made by the Assistant Inspector of the Light-House District, a naval officer, who reported that after taking the sworn evidence of the light-keepers at Little Gull and the other light-stations within hearing distance, of other Government officers who were, for the time being, so located that they might have had knowledge of the facts, and of the officers of vessels that were within ear shot, including those of the Galatea, he reached the conclusion that the fog-signal was sounding at the time of the accident; and that, although the fog-signal was heard at Mystic, fifteen miles distant in another direction, and although it was heard on a steam tug a mile beyond the Galatea; that it was heard faintly, if at all, on that vessel; and if heard at all, was so heard as to be misleading, though the Galatea was but one-eighth of a mile from the source of the sound.

This report is in itself full of interest. It appears that this officer spent several days steaming around Little Gull, while the fog-signal was in full blast, in various kinds of weather, and that he found the aberrations in audition here were as numerous and even more eccentric than those before mentioned as experienced at Beaver Tail. The results of his observations are given in Tables B and C; and in each case the condition of the atmosphere as to humidity, pressure, temperature and motion are shown, as is also the then tidal condition.

Table B.

Fog Signal Tests at Little Gull Island, Long Island Sound, July 11, 1881. Time 10 A. M. Wind, N.N.E., force 2. Barometer, 29.77; Thermometer, 61. Weather at commencement, dark, overcast with squalls of Scotch mist from N.N.E. It began to clear at 11:30 A.M.

Number of Observation.	Time of Observation.	Distance from Little Gull Island fog signal in stat. miles.	Intensity of sound in scale of ten.	Remarks.
	h. m.			
1	10 10	1⅝	1	
2	10 15	2⅜	½	A faint murmur is put at ½ of 1, in scale of 10.
3	10 18	2½	0	
4		3⅜	0	

Number of Observation.	Time of Observation.	Distance from Little Gull Island fog signal in stat. miles.	Intensity of sound in scale of ten.	REMARKS.
	h. m.			
5	10 25	3⅝	0	
6		3½	0	
7		3½	½	About ½ mile from last station.
8	10 50	3½	1	
9		3⅝	0	
10		3⅝	1	About ½ mile from last station.
11		3¾	2	About ½ mile from last station.
12	11 09	3½	2	Changed course and ran a little S. of W.
13		3¾	3	
14	11 15	2⅞	3	
15	11 25	2½	4	
16		2¾	5	
17	11 35	2½	7	
18		2⅛	7	
19		1½	8	
20	11 55	½	9	
21		½	10	
22	12 03	⅜	10	About ½ mile from last station.
23	12 07	⅜	7	
24		1⅞	2	
25	12 14	1⅞	1	
26	12 19	2¼	1½	
27	12 23	2¾	½	Changed course.
28	12 40	2¾	½	Faint murmur.
29	12 52	3½	0	Changed course.
30	1 01	2	½	
31	1 06	1⅝	1-2	
32	1 12	1⅝	5	
33	1 18	¾	10	
34		⅝	10	Almost west of fog-signal.
35		1¼	10	
36	1 35	1½	8	Changed course.
37		1⅝	8	
38	1 42	⅞	10	Stood N. E.; sound gradually increasing.
39	1 52	½	3	
40	1 55	⅞	2	Changed course.
41		¾	2	
42	2 01	⅜	2	
43	2 02	⅛	10	
44		⅜	10	
45		¾	8	
46		1	7	
47		1¾	5	
48	4 29	2	2	
49		2¾	1	
50	4 38	3⅜	0	Lost the sound.
51		3¾	0	
52	4 45	4¼	0	Bartletts Reef light-ship; wheels stopped and no sound.

Table C.

Observations at Little Gull Island, Long Island Sound, July 15, 1881, commencing at 6.30 A. M. Thermometer, 59° Fahr. Barometer, 29.80. Wind, W.N.W., force 3, hauling to the westward and increasing gradually.

Number of Observation.	Time of Observation.	Distance from Little Gull Island fog-signal in stat. miles.	Intensity of sound in a scale of ten.	Remarks.
	h. m.			
1	6 32	1¾	10	
2	6 57	2¼	10	Changed course, running S. by W. ½ W.
3		2¼	8	About ½ mile from last station.
4		2⅜	7	
5		3¼	4	
6	7 17	3¾	3	Changed course, running E.
7		3⅝	2	About ½ mile from last station.
8		3¾	1	" " "
9		3½	5	" " "
10	7 28	3⅜	7	Changed course, running N. by W. ½ W.
11		2½	8	
12		2½	5	About ½ mile from last station.
13		2	5	Changed course, running W.
14	7 50	2¾	5	
15		2⅞	3	
16		3⅛	2	
17	8 00	3¾	0	Sound lost.

On August 3d, I had an opportunity to hear this fog-signal myself, and to note its audibility. The wind was from the south and very light; the air was damp, smoky, hazy, and, as the sailors say, hung low; the barometer stood at 29·90; the tide was about flood. Our steamer was run for six miles in the axis of the siren's trumpet, which was sounded for our benefit at its full force. Note was made every third minute in a scale of ten of the intensity of the sound, and it was found that the audition decreased normally with the distance for the first two miles; at 2½ miles it had fallen off one-half; at 3 miles it had fallen to one-tenth its power; at 3½ miles away we could hear but a faint murmur, and when 4 miles distant, we had lost it completely; and yet there seemed to be no reason why we should not have heard it clearly at three times that distance.

The next morning was calm, but heavy with white fog; yet we heard the Little Gull siren distinctly though it was 10½ miles off, as we lay at our dock in New London. The steamer ran out of the harbor, but was compelled to anchor so thick was the fog ; yet we heard Little Gull though 7½ miles off, at a force of 6 in the scale of ten, and the sound was so clear cut and distinct that we could differentiate it from the siren at the New London light, which was much nearer to us. The steamer worked round to inspect the neighboring lights, and we heard the Little Gull siren when at North Dumpling light station, 7 miles off, at a force of 6; at Morgan's Point Light, 10 miles off, at a force of 5, and we continued to hear it at an intensity of from 5 to 6 as we worked around among the other lights, within a compass of 10 miles, till the fog broke and the siren ceased.

Opportunity soon occurred for making more critical experiments. On a fine day we ran out to Little Gull, had the siren started under full steam, and then, following out a pre-arranged program, ran round Little Gull Island in such way, as to describe a rectangle of about 8 by 10 miles, its longest side running nearly north and south. No fixed rate of speed was maintained, but the steamer slowed, backed, or stopped, as was necessary. The atmosphere was what the sailors call lumpy, and Prof. Tyndall calls non-homogeneous. Prof. Henry, when writing of a like condition, said: *"As the heat of the sun increases during the first part of the day, the temperature of the land rises above that of the sea, and this excess of the temperature *produces upward currents of air*, disturbing the general flow of wind, both at the surface of the sea and at an elevation above." Observations were made and noted in a scale of ten, of the force or intensity of the signal's sound as it reached us at the end of each minute. The following Table D shows a sufficient number of the results for our purposes, taken from the tabulated schedule of our notes. The table also shows the condition of the atmosphere during our observations.

*L. H. Board's Rep. for 1875, page 116.

Table D.

Observations at Little Gull Island, Long Island Sound, August 9, 1881, commencing at 10 A. M. Thermometer—Dry Bulb, 73°.09, Wet Bulb, 73° Fahr. Barometer, 29.77 Wind, S. W., force, 3. Cir. Strat. Clouds about the horizon.

Number of Observation.	Time of Observation.	Distance from Little Gull Island in statute miles.	Intensity of sound in scale of ten.	Number of Observation.	Time of Observation.	Distance from Little Gull Island in statute miles.	Intensity of sound in scale of ten.
	h. m.				*h. m.*		
1	10 30	0¼	10	16	12 04	2⅝	9
2	10 32	0⅓	10	17	12 08	2¼	9
3	10 34	0½	10	18	12 13	2⅛	5
4	10 36	1	10	19	12 20	2⅛	3
5	10 37	1¼	0	20	12 28	3¼	1
6	10 48	2	0	21	12 35	3½	0½
7	10 57	3	0	22	12 41	3⅜	0
8	11 02	3	0	23	12 45	3	1
9	11 08	3 1/16	1	24	12 57	2½	0
10	11 15	3½	3	25	12 58	2⅜	0
11	11 23	4⅛	4	26	1 02	1½	1
12	11 38		8	27	1 20	1¾	0½
13	11 42	2¾	9	28	1 24	1⅝	0½
14	11 54	3	9	29	1 30	0¾	0
15	11 57	3¼	9	30	1 32	0¼	10

At 4 P. M. two of us went in a row boat to Little Gull from the steamer which lay to her anchor half a mile off, and verified the fact that the fog-signal had been in full operation during the time of our observations by the report of the steamer's mate, who had been left there for that purpose. It then occurred to us to investigate still more closely what appeared to be a space—a circle of silence—in which we had, during the experiments of the morning, failed to hear the signal. After having had the siren put in full operation again, we pulled toward the nearer end of Great Gull Island, the siren sounding meantime with earsplitting force. When about 600 yards away we suddenly lost the sound as completely as if the signal had stopped. Pulling toward the steamer, not more than 200 yards, we reached a position at right angles with the axis of the siren's trumpet when we suddenly heard the sound again at its full force. Thus, in pulling 500 yards, we passed from complete audition of the signal to absolute inaudition; and then we passed back again to complete audition by pulling 200 yards in

another direction. All this took place within half an hour in open water, always in full view of the signal station, and without any visible obstacle being interposed or removed.

While on the island we learned that one of the light-house keepers, who had been on leave, had just returned from Sag Harbor, twenty miles away to the southeast. He had failed to hear the signal at all, until opposite the eastern end of Great Gull Island, and until he was within half a mile of the siren which was in full operation.

On the next morning our steamer anchored about a mile north of Little Gull; the wind was light, the air was clear, and the day was warm and beautiful. As it had been preceded by a warm night the atmosphere was homogeneous, and it was expected that we should have a day of normal audition and barren of curious phenomena. After the siren had commenced its noise we ran down to a point within half a mile of the light-house, and then steamed for Plum Island, running a little south of east for six miles, when we returned as nearly as might be on our own track. The results were curious. We lost half the force of the sound when within a quarter of a mile of the siren; a moment later we had lost four-fifths of it. Running another half mile we were off the middle of Great Gull Island, and the sound had increased to a force of four; in five minutes more it had dropped to three; from that on, until we reached the end of our six mile run, it gradually weakened, and it had dropped to a force of two when we turned and ran back to our anchorage. It is particularly curious that the sound had the same intensity at three-sixteenths of a mile from its source, and at six whole miles from that point, while it varied from two to ten in a scale of ten between those points. The results of the trip are more fully and exactly given in Table E.

Thinking that possibly this peculiarity might have been induced by those differences of temperature in the strata of the atmosphere suggested by Dr. Tyndall as probable cause for such phenomena, effort was made to ascertain something of these differences by sending a thermometer to the upper air. In the course of the afternoon we made a kite some six feet high, attached to it a self-registering thermometer, and after a number of trials succeeded in getting it up about five hundred feet, and in hauling it safely in again after it had been up over an hour. The thermometer had a wet bulb, and beside was protected from the direct rays of the sun; but it registered only half a degree more of heat at its highest point than it had done in the pilot-house. The course the kite took showed no difference between the air currents alow and aloft.

TABLE E.

Observations at Little Gull Island, Long Island Sound, August 10, 1881, commencing at 10:30 A. M. Dry Bulb Thermometer, 76°, Wet Bulb, 75°. Barometer, 29.40. Wind, W. by N., force 3, and steady throughout. Day clear and beautiful.

Number of Observation.	Time of Observation.	Distance from Little Gull Island in a direct line in statute miles.	Intensity of sound in a scale of ten.	Number of Observation.	Time of Observation.	Distance from Little Gull Island in a direct line in statute miles.	Intensity of sound in a scale of ten.
	h. m.				*h. m.*		
1	10 36	$1\frac{1}{16}$	10	7	10 59	$2\frac{1}{16}$	2 to 3
2	10 40	$0\frac{3}{4}$	10	8	11 07	$2\frac{1}{2}$	2 to 3
3	10 44	$0\frac{1}{4}$	5	9	11 29	$2\frac{5}{8}$	2 to 3
4	10 45	$0\frac{3}{16}$	2	10	11 45	$5\frac{1}{16}$	2 to 3
5	10 49	$0\frac{3}{4}$	4	11	11 52	$5\frac{1}{2}$	2
6	10 53	$1\frac{1}{4}$	3	12	12 02	6	2

The Light House Board has known from the first that aberrations in audibility might occur near any fog-signal. When the fog-trumpet was set up at Beaver Tail Point in 1856, the Naval Secretary of the Board, then Lieutenant, now Rear Admiral Jenkins, U. S. N., in company with Mr. Daboll, its inventor, found, in returning to Newport, that they lost the sound of the signal between Beaver Tail and Fort Adams, and recovered it again between the Fort and Newport, as did later observers, and that this failure to hear it did not result from any failure of the signal to operate.

The Board's publications show that Prof. Henry, its scientific adviser, had the subject for many years continuously under advisement, and that between 1865 and 1878, many experiments were made, and various reports on them were submitted to the Board, as to the use and value of its several kinds of fog-signals. In 1870 the Board directed General Duane, of the U. S. Engineers, then and still in its service, to make a series of experiments to ascertain the comparative value of its different signals. In his report the General said, speaking of the steam fog-signals on the coast of Maine:

*"There are six steam fog-whistles on the coast of Maine; there have been frequently heard at a distance of twenty miles, and as frequently cannot be heard at the distance of two miles, and this with no perceptible difference in the state of the atmosphere.

"The signal is often heard at a great distance in one direction, while in another it will be scarcely audible at the distance of a mile. This is not the effect of wind, as the signal is frequently heard much farther against the wind than with it; for example, the whistle on Cape Elizabeth can always be distinctly heard in Portland, a distance of nine miles, during a heavy northeast snow-storm the wind blowing a gale directly from Portland toward the whistle."

* * * * * * * * * * *

"The most perplexing difficulty, however, arises from the fact that the signal often appears to be surrounded by a belt, varying in radius from one to one and a half miles, from which the sound appears to be entirely absent. Thus, in moving directly from a station, the sound is audible for the distance of a mile, is then lost for about the same distance, after which it is again distinctly heard for a long time. This action is common to all ear-signals, and has been at times observed at all the stations, at one of which the signal is situated on a bare rock twenty miles from the main land, with no surrounding objects to affect the sound."

Prof. Henry, in considering the results of Gen. Duane's experiments, and his own, some of which were made in company with Sir Fred'k Arrow and Capt. Webb, H. B. M. Navy, both of the British Light-House Establishment, who were sent here to study and report on our fog-signal system, formulated these abnormal phenomena. He said they consisted of:

"1. The audibility of a sound at a distance and its inaudibility nearer the source of sound.

"2. The inaudibility of a sound at a given distance in one direction, while a lesser sound is heard at the same distance in another direction.

"3. The audibility at one time at a distance of several miles, while at another the sound cannot be heard at more than a fifth of the same distance.

"4. While the sound is generally heard further with the wind than against it, in some instances the reverse is the case.

"5. The sudden loss of a sound in passing from one locality to another in the same vicinity, the distance from the source of sound being the same." †

These experiments were not confined to our own shores. Dr. Tyndall, the well known English physicist, who stands in the same relation to the British Light-House Establishment that Prof. Henry did to our own, writes thus:

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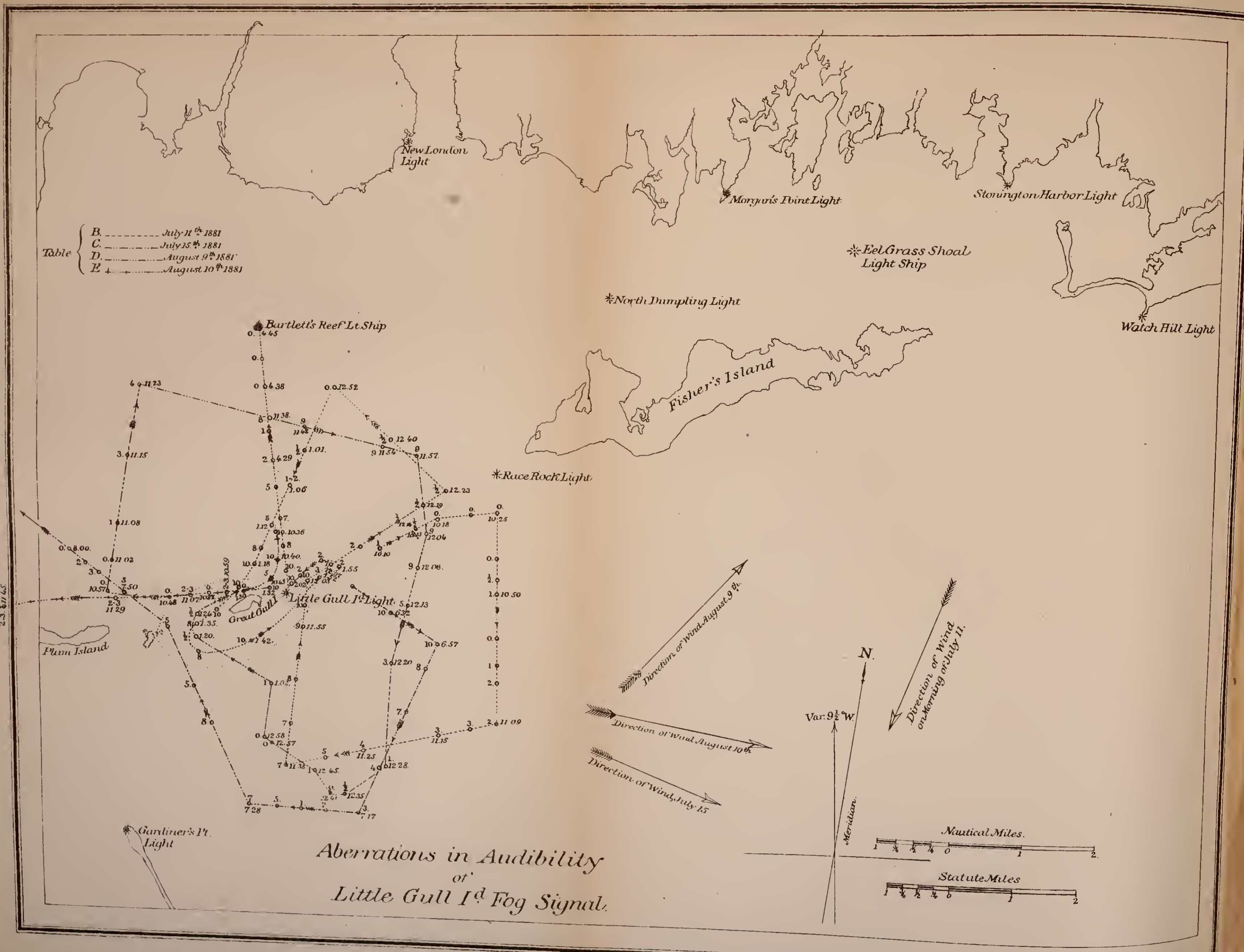
*Annual Rep't L. H. Board 1874, pp. 99–100.

† L. H. B. Annual Rep. 1875, page 106.



Aberrations in Audibility of Little Gull Id. Fog Signal.

"With a view to the protection of life and property at sea, in the years 1873 and 1874, this subject received an exhaustive examination, observational and experimental. The investigation was conducted at the expense of the Government, and under the auspices of the Elder Brethren of the Trinity House [the governing body of the British Light-House Establishment.]

"The most conflicting results were at first obtained. On the 19th of May, 1873, the sound range was 3½ miles; on the 20th it was 5½ miles; on the 2d of June 6 miles; on the 3d more than 9 miles; on the 10th 9 miles; on the 25th 6 miles; on the 26th 9¼ miles; on the 1st of July 12¾ miles; on the 2d 4 miles, while on the 3d, with a clear, calm atmosphere and smooth sea, it was less than 3 miles." *

The officer who made the reports, as to the fog-signals at Beaver Tail and Little Gull, after the accidents to the steamers Rhode Island and Galatea heretofore mentioned, was the Assistant Inspector of the Third Light-House District, Lieut. Comd'r F. E. Chadwick, U. S. N.; and it was he who had charge of the Light-House steamer while the foregoing observations were being made, after Capt. George Brown, U. S. N., the Inspector—to whom I am indebted for many courtesies on this trip—was called elsewhere by other official duties. Mr. Chadwick brought to this work an unbiased mind, trained in the severest schools of scientific investigation. His object in all his experiments was simply to ascertain the exact truth for practical official purposes. He had not proposed, even to himself, to make any generalizations from his observations. But he kindly answered certain of my questions as to the opinions which had forced themselves upon him, and his answers are here set down for the consideration of those who use these fog-signals overmuch as a guide for their ships.

"It seems to me" he said "that navigators should understand that when attempting to pick up a fog-signal attention must be given to the direction of the wind, and that if they are to windward, (in a moderate breeze,) the chances are very largely against hearing it, unless close to; that there is nearly always a sector of about 120° to windward of the signal in which it either cannot be heard at all, or in which it is but faintly heard. Thus, with the wind E. S. E., so long as they are bearing from the signal between N. E. and South, there is a large chance that the signal will not be audible until it is very close.

"As they bring the signal to bear at right angles with the wind, the sound will almost certainly in the case of light wind increase, and it will soon assume its normal volume—being heard almost without fail in the leeward semicircle.

"Fog, to my mind, and so far as my experience goes, is not a factor of any consequence whatever in the question of sound. Signals may be heard at great dis-

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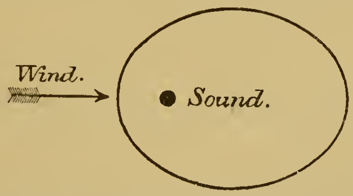
*Sound, by Tyndall, 3d Edition English, page 324.

tances through the densest fogs, which may be totally inaudible in the same directions and at the same distances in the clearest atmosphere. It is not meant by this last statement that the fog may assist the sound; as at another time the signal may be absolutely inaudible in a fog of like density, where it had before been clearly heard. That fog has no great effect can easily be understood when it is known, (as it certainly is known by observers,) that even snow does not deaden sound—there being no condition of the atmosphere so favorable for the far reaching of sound signals as is that of a heavy N. E. snow storm, due supposably to the homogeneity produced by the falling snow.

"It seems to be well established by numerous observations that on our own northern Atlantic coasts the best possible circumstances for hearing a fog-signal are in a northeast snow storm, and, so far as these observations have extended, they seem to point to the extraordinary conclusion that they are best heard with the observer to windward of the signal; and that in light winds the signal is best heard down the wind, or at right angles with the wind.

"The worst conditions for hearing sound seem to be found in the atmosphere of a clear, frosty morning on which a warm sun has risen and has been shining for two or three hours.

"The curve of audibility in a light or moderate breeze, in general, is similar to that plotted by Prof. Henry, as in the accompanying diagram.



"I think it is established that there are two great causes for these phenomena, non homogeneity of the atmosphere, and the movement of the wind; how this latter acts no one can say. The theory of retardation of the lower strata of the atmosphere near the earth's surface, as advanced by Prof. Stokes, of England,* seems good for moderate winds, but it hardly holds in cases where the siren is heard from eighteen to twenty miles to windward during N. E. gales."

While the mariner may usually expect to hear the sound of the average fog-signal normally as to force and place, he should be prepared for occasional aberrations in audition. It is impossible at this point in the investigations which are still in progress, to say when, where or how the phenomena will occur. But certain suggestions present themselves even now as worthy of consideration.

It seems that the mariner should, in order to pick up the sound of the fog-signal most quickly when approaching it from the wind-

*See Henry on Sound, p. 533; or, Sm. Rept., 1878, p. 533; or, L.-H. B. Rept. for 1875, p. 120. See Henry on Sound, p. 512, and Taylor in Am. Jour. Sci., 3d series, XI, p. 100, also, Rept. Brit. Assoc., XXIV, 2d part, p. 27.

ward, go aloft; and that, when approaching it from the leeward, the nearer he can get to the surface of the water the sooner he will hear the sound.

It also appears that there are some things the mariner should not do.

He should place no negative dependence on the fog-signal; that is, he should not assume that he is out of hearing distance because he fails to hear its sound.

He should not assume that, because he hears a fog-signal faintly, he is at a great distance from it.

Neither should he assume that he is near to it because he hears the sound plainly.

He should not assume that he has reached a given point on his course because he hears the fog-signal at the same intensity that he did when formerly at that point.

Neither should he assume that he has not reached this point because he fails to hear the fog-signal as loudly as before, or because he does not hear it at all.

He should not assume that the fog-signal has ceased sounding because he fails to hear it even when within easy earshot.

He should not assume that the aberrations of audibility which pertain to any one fog-signal pertain to any other fog-signal.

He should not expect to hear a fog-signal as well when the upper and lower currents of air run in different directions; that is when his upper sails fill and his lower sails flap; nor when his lower sails fill and his upper sails flap.

He should not expect to hear the fog-signal so well when between him and it is a swiftly flowing stream, especially when the tide and wind run in opposite directions.

He should not expect to hear it well during a time of electric disturbance.

He should not expect to hear a fog-signal well when the sound must reach him over land, as over a point or an island.

And, when there is a bluff behind the fog-signal, he should be prepared for irregular intervals in audition, such as might be produced could the sound ricochet from the trumpet, as a ball would from a cannon; that is, he might hear it at 2, 4, 6, 8 and 10 miles from the signal, and lose it at 1, 3, 5, 7, 9 and 11 miles distance, or at any other combination of distances, regular or irregular.

These deductions, some made, as previously mentioned, by several of the first physicists of the age, and some drawn from the original investigations here noted, are submitted for consideration rather than given as directions. They are assumed as good working hypotheses for use in further investigation. While it is claimed that they are correct as to the localities in which they were made, it seems proper to say that they have not been disproved by the practical mariners who have given them some personal consideration, and who have tried to carry them into general application. Hence these suggestions have been set down in the hope that others with greater knowledge and larger leisure may give the subject fuller attention, and work out further results.

If the law of these aberrations in audibility can be evolved and some method discovered for their correction, as the variations of the compass are corrected, then sound may be depended upon as a more definite and accurate aid to navigation. Until then, the mariner will do well when he does not get the expected sound of a fog-signal, to assume that he may not hear a warning that is faithfully given, and then to heave his lead, and resort to the other means used by the careful navigator to make sure of his position.

Mr. Cleveland Abbe remarked that it seemed to him if these anomalies were due to the refraction of sound in a vertical plane, then a few feet of increase in the altitude of the observer or of the signal itself, would make a great difference in the result. To this Mr. Johnson replied that the observations made on board the vessels were attended with the same results as to degree of audibility, whether the observer were stationed upon the mast, deck, or near the water line of the vessel.

Mr. William B. Taylor said that the interesting observations presented by Mr. Johnson were in the main entirely corroborative of the results announced by our late President, Prof. Henry; and the anomalies noted furnished striking confirmation of the explanations and generalizations reached by him, while they as strikingly discredited as incongruous the rival hypothesis of hygroscopic flocculence in the atmosphere as a notable occasion of acoustic disturbance. When we consider the wide areas over which fog-signals are designed to be conveyed—through which spaces the atmosphere can rarely be uniform, either in its temperature or its movements—we can readily understand that from these two prominent conditions of sound-refraction, acoustic rays are commonly propagated in quite sensibly curved or often serpentine directions; and that while these inequalities will sometimes favor audibility at given points, they will as often impair or defeat it. Moreover, these deformations of sound waves are not confined to vertical planes, since it has been shown that *lateral* refractions may exist, giving false impressions of direction as well as of distance.

As we have no means of either controlling or accurately determining these simultaneous differences of wind and temperature, we are forced to admit that the practical difficulties attending these anomalies of sound propagation are insoluble and incurable. But we must not hence abandon sound-signalling as either hopeless or inefficient, since it is the best—or rather the only—method at our disposal of giving warning and guidance to the befogged mariner.

Two partial alleviations of the recognized defects are suggested. The first is to place the siren or the steam whistle at considerable elevations, say on the top of skeleton towers, perhaps higher than those ordinarily employed as light-towers; at which points they could readily be operated from the ground. This would, in many cases, counteract the tendency to local acoustic shadows or bands of silence, though in other cases it would be quite ineffectual. The second expedient is, (if not too expensive,) to greatly multiply the number of such signals at available points about dangerous coasts or inlets, with proper distinctions to clearly specialize their indications, in order that the mariner failing to catch the sound from one direction, might have the probability of picking up the sound from a different azimuth. As these sound instruments may be operated at considerable distances from the engine, and even at practically inaccessible positions, on rocks or on buoys, *danger points* especially should be guarded by fog-signals, not necessarily of great power, but capable, at least, of covering the radius of actual insecurity.

Remarks were made by Mr. WILLIAM B. TAYLOR on the relation of fog and snow storms to audibility.

With regard to fog, Mr. TAYLOR said, we are not to conceive the sound vibrations as passing alternately through air and water, (as a ray of light does,) but taking into view the average wave-length of sound (several feet ordinarily) and the enormous number of water particles contained in that space, we must contemplate the whole mass as a homogeneous medium taking up the sound waves in the same manner, whether the air were perfectly dry, or were precipitating excessive moisture in the form of rain. In the absence of sensible wind, the air thus supersaturated with moisture would be practically very homogeneous, and thus generally well adapted to the normal transmission of sound.

A similar remark applies to falling snow, (when not accompanied with strong wind,) with the additional circumstance that, while the precipitation and congelation would tend to warm the upper regions of the air, any melting of the snow as it fell would cool the lower region. This condition of relative warmth above and cold below is favorable to the conveyance of sound to a distance—as first pointed out by Prof. Osborn Reynolds, of Manchester,—by reason of the expanding spherical wave-front being slightly more accelerated above than below, (in accordance with well known principles,) and thus causing the horizontal or slightly rising sheets of sound to be dished downward.

The next communication was by Mr. WILLIAM HARKNESS on the relative accuracy of different methods of determining the solar parallax.

This paper is published in full in the *American Journal of Science* for November, 1881, No. 131, vol. 22, pp. 375–394.

205TH MEETING. NOVEMBER 5, 1881.

The President in the Chair.

Forty-three members present.

MR. J. C. WELLING presented the following communication on

ANOMALIES OF SOUND SIGNALS.

In the year 1865 Prof. Henry, while making some observations on the intensity of sounds, discovered that a sound moving against the wind, and which was inaudible to the ear of an observer on the deck of a vessel, might sometimes be regained by ascending to the mast-head; that is, sound is sometimes more readily conveyed by the upper current of the air than by the lower.

This fact, with other corroborative facts, did not, he says, reveal its full significance to him until he was able to interpret it by the aid of the hypothesis of Prof. Stokes. (Transactions of the British Scientific Association for 1867, Vol. 24,) according to which there is—when the wind blows—a difference of velocities between the upper and the lower strata of the atmosphere, resulting from the retardation of the lower stratum by friction with the ground. This unequal movement of the atmosphere disturbs the spherical form of the sound waves, and tends to make them somewhat of the form of an ellipsoid, the section of which by a vertical diametral plane, parallel to the direction of the wind, is an ellipse, meeting the ground at an obtuse angle on the side towards which the wind is blowing, and at an acute angle on the opposite side. But as sound moves in a direction perpendicular to the front of the sound waves, it follows that sounds moving with a favorable wind tend to be tilted downwards toward the ground; and sounds moving against an opposing wind tend to be tilted upward until, finally, they pass above the head of a listener standing on the ground.

The effect of different elevations on the audibility of the same sound has been brought within the sphere of scientific experiment. In some experiments made by Prof. Reynolds in 1874, on "a flat meadow," by the aid of an electrical bell, placed one foot from the ground, it was found that elevation affected the range of sound against the wind "in a much more marked manner than at right angles." He adds: "Over the grass no sound could be heard with the head on the ground at twenty yards from the bell, and at thirty yards it was lost with the head three feet from the ground, and its full intensity was lost when standing erect at thirty yards. At seventy yards, when standing erect, the sound was lost at long intervals, and was only faintly heard even then; but it became continuous again when the ear was raised nine feet from the ground, and it reached its full intensity at an elevation of twelve feet."*

In some experiments made by Prof. Henry, in 1875, he found that while sound moving at right angles to the wind could not be heard as far as sound moving with the wind, yet it was equally true

* London, Ed., and Dub. Ph. Mag. for 1875, Vol. 50.

of sounds moving against the wind and at right angles to the wind, that they could both be better heard on the top of a high tower than on the surface of the ground.*

Baron Humboldt, in observations made on the intensity of sounds at the Falls of the Orinoco, remarked their greater audibility by night than by day, and referred their comparative weakness by day to the effect of atmospheric disturbances arising from ascending currents of rarified air and descending currents of heavier air, which broke up the homogeneity of the atmosphere, and thereby obstructed the transmission of sound. It is a necessary complement of this hypothesis that sound which fails to be transmitted through the atmosphere, because of "the reflections which it endures at the limiting surfaces of the rarer and the denser air," is liable to be returned to the hearer in the shape of aerial echoes rebounding from the acoustic cloud which the primary sound is not able to pierce; and hence the logical place assigned to echoes by Dr. Tyndall, when, adopting and applying the Humboldt hypothesis, he says that "rightly interpreted and followed out, these aerial echoes lead to a solution which penetrates and reconciles the phenomena from beginning to end." "On this point," he says, "I would stake the issue of the whole inquiry. * * * The echoes afford the easiest access to the core of this question." †

The conflicting hypotheses of Humboldt and Stokes, as respectively applied by Tyndall and Henry in interpreting the abnormal phenomena of sound, are here cited as prefatory to some much older observations made under the same head by Dr. W. Derham, in his elaborate paper entitled "Experiments and Observations on the *Motion of Sound,* and other things pertaining thereto," as read before the Royal Society in 1708. This paper, written in Latin, is the report of a systematic inquiry into phenomena pertaining to the velocity and motion of sounds, and treats only incidentally on the intensity of sounds; but, nevertheless, it contains some interesting statements under this latter head.‡

The subject of echoes is the first which engages the writer's attention. He says that echoes produced by sound-reflecting objects situated near a sounding body may sometimes be heard through many

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* Rep. of Light-House Board, 1875, p. 119.

† "Sound," p. xxiv.

‡ Phil. Trans. of Royal Society, Jan. and Feb., 1708.

miles, as well as the primary sound, or even better than the latter. He observes that echoes produced by the firing of cannon on the Thames river, between Deptford and Cuckold's Point, came to his ears in a multiple form, repeated five or six times, and the terminal crash of the echo was the loudest. This last feature was observed even when the multiple sounds were nine or ten in number. To this he adds: "When I have heard the crashes of heavy artillery, especially in a still and clear atmosphere, I have often observed that a *murmur* high in the air preceded the report. And in thin fog I have often heard the sound of cannon running in the air, high above my head, through many miles, so that this murmur has lasted fifteen seconds. This continuous murmur, in my opinion, comes from particles of vapor suspended in the atmosphere which resist the course of the sound waves, and reverberate them back to the ears of the observer after the manner of undefined echoes.*

Mr. Richard Townley, an intelligent observer, having written to Dr. Derham, in a letter from Rome, that "sounds are rarely heard as far at Rome as in England and in other northern regions, and having cited in support of this statement some observations drawn from the firing of cannon in the castle of St. Angelo, Dr. Derham caused an enquiry on this point to be made in Italy, under the auspices of the British Minister at Florence. The enquiry was conducted by Joseph Averani, a Professor in the University of Pisa. Guns were fired at Florence, and observers were stationed at different points in Leghorn and its vicinity to mark the effect of the reports. The observers stationed in the Light-House and the Marzocco tower, in the lower part of the city, heard no reports, but observers stationed on an old fortress in the upper part of the city, and other observers placed on Monte Rotondo, about five miles from Leghorn in the direction of Mount Nero, (and, therefore, more in the direction of the wind which was blowing across the path of the sound,) were able to hear the reports.

Another series of experiments was made on water, by firing cannon at Leghorn, and stationing observers at Porto Ferrajo in the Island of Elba, a distance of about sixty miles. In this case the reports were better heard in still air than when the wind was either favorable or unfavorable, and were not heard at all points equally well, but only at those which were a little the more elevated.†

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* Derham, p. 10. † *Ibid.*, pp. 18, 19, 20.

As to the result of these observations, it was easy for Dr. Derham to conclude that sounds are heard as far in Italy as in England, when the conditions of the atmosphere are the same; and these experiments are here cited only for the light they shed on the comparative antiquity of the observation that elevation has an important bearing on the audibility of sounds.

As to the causes which really affect the intensity of sounds, Dr. Derham seems to have had a very obscure and imperfect notion. His observations under this head are mainly a bundle of contradictions, and the causes of these variations he prudently leaves to be investigated by others, seeing, as he says, "that it equally exceeds the grasp of his mind to discover them, and to assign what may be the proper medium or vehicle of sound." He does not, however, fall into the error of measuring the acoustic transparency of the atmosphere by its optic transparency, for he says that the clearest day he can remember, when wind and everything else seemed to concur in promoting the force and velocity of sound, was a day when he could not hear the firing of cannon at a distance easily penetrated by their reports on former occasions. The effect of clear or foggy air on sound, he says, is very uncertain, but as to *thick fogs* and snow, he affirms that they are certainly powerful dampers of sound, an observation now abundantly proved to be erroneous.

From some observations made by Gen. Duane, at Portland, Maine, in 1871, it appears that the fog-signal at that point is often surrounded by a belt of silence, varying from one to one and a half miles in radius.

From some observations made by Prony, Mathieus, and Arago, at Villejuif, and by Humboldt, Bouvard, and Gay-Lussac, at Montlhéry, in France, the two towns being 11.6 miles from each other, it was noticed that while every report of the cannon fired at Montlhéry was heard with the greatest distinctness, nearly every report from Villejuif failed to reach Montlhéry. The air at the time was calm, with a slight movement of wind from Villejuif toward Montlhéry, or "against the direction in which the sound was best heard." These observations were made in 1822.

In 1872, Prof. Henry observed the same non-reciprocity of sound in approaching the Whitehead fog-signal on the coast of Maine. At a distance of six miles the signal was heard; at a distance of three miles from the shore the sound of the signal was lost, and was not regained until the vessel approached within a quarter of a mile of the station. During all this time of silence the sound of the steamer's whistle was distinctly heard at the Whitehead station; that is, a lesser sound was heard from the steamer to the station, "while a sound of greater volume was unheard in the opposite direction." The wind at the time was blowing in favor of the steamer's whistle, and against the fog-signal.*

In a paper presented to the Royal Society in 1874, Prof. Reynolds showed that the form of the sound-wave is liable to flexure from changes in the temperature of the atmosphere as well as from the unequal motion of wind.†

These abnormal phenomena of sound, considered in connection with the hypothesis of Prof. Stokes, as enlarged and applied by Prof. Henry, may be reduced into the following generalizations which, if accurate in point of logical form, and true in point of the facts to which they are applied, may be stated under the guise of aphorisms, as follows:

1. "Where the condition of the air is nearest that of a calm, the larger will be the curve of audition, and the nearer will the shape of the curve approach to a circle, of which the point of origin of the sound, or the point of perception will be the centre." [This aphorism is stated abstractly from any consideration of temperature refraction which, so far as it exists, will always tend to modify the shape of the curve of audition.]‡

2. Apart from all consideration of temperature refraction, a sound will be heard furthest in the direction of a gentle wind, because the portion of the sound-wave thrown down from above, in this case, is re-enforced by the sound reflected from the surface, and will thus more than compensate for the loss by friction.||

3. Other things being equal, the area of audition will be proportionally diminished in the case of sounds moving against winds more or less strong, because the sonorous waves will be refracted above the ears of the observer. (Stokes, Henry and Reynolds.)

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* Rep. Light-House Board, 1874, p. 108.

† London, Ed., and Dublin Phil. Mag. for 1875, Vol. 50, p. 52.

‡ Light-House Report for 1875, p. 125.

|| *Ibidem.* Cf., Tyndall's Sound, p. 311. Cf., Reynolds in Lon., Ed., and Dub. Ph. Mag. for 1875, Vol. 50, pp. 63, 68.

4. The area of audition will be diminished in the case of a sound moving with an overstrong favoring wind, because the sound-waves in this case will be so rapidly and strongly thrown down to the ground that the intensity of the sound will suffer more diminution from absorption and friction than can be supplied by the upward reflection of the sound rays conspiring with the gradual downward flexure of the sound-waves, as in the case of a gentle favoring wind.*

5. Sounds moving *against* a gentle wind will, *cæteris paribus*, be heard further than similar sounds moving *with* an overstrong favoring wind, for reasons already implied, because the downward flexure of the sound-waves, being excessive in the latter case, tends to extinguish the conditions of audibility more rapidly than is done by the slight upward refraction in the former case.

6. When sounds moving against the wind are heard further than similar sounds moving with a wind of equal strength, it is because of a dominant upper wind blowing at the time in a direction opposite to that at the surface.†

7. A sound moving against the wind, and so refracted as in the end to be thrown above the head of the observer will, at the point of its elevation, leave an acoustic shadow. But this acoustic shadow, at a still further stage, may be filled in by the lateral spread of the sound-waves, or may be extinguished by the downward flexure of the sound waves, resulting from an upper current of wind moving in an opposite direction to that at the surface, or resulting in a less degree from an upper stratum of still air. Under these circumstances, there will be areas of silence enclosed within areas of audition.‡

8. As sounds may be refracted either by wind, or by changing temperatures, or by both combined, it follows that, under many circumstances, a sound lost at one elevation may be regained at a higher elevation.||

9. As sounds moving against the wind are liable to become inaudible (by being tilted over the head of the observer) even before

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* Light-House Report, 1875, p. 125.

† Light-House Report for 1877: Experiments on Sound, p. 13.

‡ Experiments on Sound, 1877, p. 8.

|| Henry and Reynolds. Cf., Delaroche, Ann. de Chim., 1816, Tome I, p. 180.

their intensity has been extinguished, we may find in this fact an explanation of the statement made by Reynolds, that "on all occasions the effect of wind seems to be rather against distance than distinctness." *

10. As sounds may be inaudible at certain distances and elevations without being wholly extinguished, it follows that the comparative inaudibility of sounds at different times cannot always be cited as an evidence of their relative intensities. The comparative inaudibility may be a function of variable refraction rather than of variable intensity. Hence the law of inverse squares, though perfectly true in its theoretical application to the measurement of the intensity of all sounds, cannot always be legitimately used to calculate backwards from the audibility of a sound, as empirically ascertained at a given point and elevation, to its relative intensity as previously heard at the same point and elevation.

11. The hypothesis of Stokes, as applied by Henry, does not exclude the hypothesis of Humboldt, but reduces the latter to a very subordinate and inappreciable place in interpreting the abnormal phenomena of sound.

12. The hypothesis of Stokes, as applied by Henry, does not exclude the reasoning or the experimental proofs by which Prof. Reynolds demonstrates that differences in temperature exert a refracting power in sound, but finds in that refraction an influence which may sometimes accelerate and sometimes retard the refraction produced by wind.†

The next communication was by Mr. C. H. Koyl, Fellow of the Johns Hopkins University, on

THE STORAGE OF ELECTRIC ENERGY.

After discussing the subject from an historical point of view, concluding with a description of the improved form of secondary battery lately invented by M. Faure, the author proceeded to state the

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* Lon., Ed., and Dub. Ph. Mag. for 1875, Vol. 50, p. 63.

† Rep. Light-House Board 1875, p. 125, cf. Reynolds; Lon., Ed., and Dub. Ph. Mag. for 1875, Vol. 50, p. 71.

results of some investigations carried on independently in this country by Mr. J. A. Maloney and Mr. Franz Burger, of Washington, and afterward by himself in connection with them.

Mr. Maloney and Mr. Burger had been aiming to interpose in the circuit of the electric lamp a reservoir of energy which should perform the same function for the electric lamp that a gasometer did for a gas-burner, viz., prevent its flickering by keeping a constant or nearly constant potential on the main line, even though the current from the source should be irregular.

A long course of experiment convinced them that plates of lead immersed in dilute sulphuric acid form a combination preferable to any other for giving return currents when once these plates have been made part of an electric circuit. They noticed what they believed to be an oxide of lead formed on one plate, and since the thicker the coating of oxide the greater the effect, they began to regard this layer as a sort of sponge which, in some way, held the electricity, and they concluded to increase the holding capacity of the cell by increasing the thickness of the sponge. Oxide of lead was accordingly purchased and painted on, with results which were surprising. The storage of electricity in large quantity was effected. This was of course independent and without any knowledge of Mr. Faure's work in Europe, but the chief merit of their inquiry lies in the rapidity with which they grasped the idea of *mechanically* increasing the sponge-like coating.

While they were testing the capabilities of the battery and were still endeavoring to improve it, the announcement was made of Mr. Faure's similar inventions. Soon after the battery was submitted for experiment to three members of this Society, and subsequently the co-operation of the author was invited for further study of the subject.

On examining the plates during their summer investigations they found reason for believing that the published theory of the action of the cell was but partly correct; for after the plates had been charged the changes of color and, therefore, of chemical constitution, upon which the return current was supposed to depend, were found, in general, not to take place until the return current had been passing for some time. If so, in something else than chemical combination must lie the storage capacity of these cells. The conclusion arrived at from their investigations was that the change of red-lead into peroxide upon one plate and into spongy lead upon the other required only a small part of the oxygen and hydrogen liberated by the primary current and that the remainder was mechanically held in the coatings.

Several minor considerations support this view, and the principal experiments upon which the proof should rest, viz., the liberation of the gas in a vacuum or by slight application of heat in general succeed. Some anomalies, however, are presented which require further study, but which the author hopes soon to reconcile with the theory of mechanical storing.

A discussion followed, in which several members participated.

206TH MEETING. NOVEMBER 17, 1881.

The President in the chair.

Thirty-eight members present.

The communication for the evening was by Mr. G. K. GILBERT

ON BAROMETRIC HYPSOMETRY.

This communication was reserved by the author, and his views and investigations in connection with this subject will be found in a paper contributed by him to the Second Annual Report of the Director of the United States Geological Survey.

A brief discussion ensued, and one or two points were questioned

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207TH MEETING. DECEMBER 3, 1881.

The President in the chair.

Seventy-six members and visitors present.

Under the rules this meeting, being the next preceding the annual meeting, was set apart for the delivery of the address of the retiring President of the Society. Calling Vice-President Hilgard to the chair, the President of the Society, Mr. J. J. WOODWARD, then read the following address:

MODERN PHILOSOPHICAL CONCEPTIONS OF LIFE.

I address you this evening in accordance with the fifth of the new Standing Rules for the government of the Philosophical Society of Washington, adopted in January last, which directs that the stated meeting next preceding the annual meeting for the election of officers shall be set apart for the delivery of the President's Annual Address. By the rules adopted at the first organization of the society the President's address was directed to be delivered on the evening of the annual meeting after the election of officers had taken place. It was found, however, that the elections always occupied the whole meeting, so that the address was necessarily postponed until after the term of office for which the President was elected had expired. During the presidency of the illustrious Professor Henry, who by common consent was re-elected annually, the inconvenience of this arrangement was not felt. But I understood the general sense of the Society last year to be that an annual change of President is desirable, and that this standing rule was adopted in view of that feeling, in order to give the retiring President a convenient opportunity for the delivery of his address before his term of office expires.

For my own part I was last year, and am now, thoroughly convinced of the desirability of electing a new President annually in a society like ours. I think on the one hand that it is a measure well calculated to increase the interest taken in the society by its members, and on the other hand that the preparation of a formal annual address would be too great a tax upon the time of a President re-elected from year to year. I think, too, that there is much propriety in a suggestion which I heard expressed in many quarters last year, that our President should be selected alternately, from what may be called for convenience, the Physical and Biological sides of the society, so that having been myself elected as in some sort a representative of the Biological side, it is my hope that you will at the next meeting elect as my successor a representative of the Physical side. With this brief explanation I will proceed at once to the consideration of the subject I have selected for the present occasion.

I propose to invite your attention this evening to some thoughts on the *Modern Philosophical Conceptions of Life.* The theme is so large that it would be idle to attempt its systematic treatment in the course of a single evening; nor do I pretend to be in possession of any satisfactory solution of this ancient question, of which I might offer you an abstract or outline, pending the fuller presentation of my results elsewhere. Yet I have ventured to hope that a discussion of some of the considerations involved, and a brief statement of certain views that I have been led to entertain, would not be without interest, and perhaps might prove of actual service, especially to those of you who are engaged in biological pursuits.

Undoubtedly the conception of life most popular at the present time is that which assumes all the phenomena of living beings to be the necessary results of the chemical and physical forces of the universe, and claims, or intimates, that wherever this has not yet been proven to be the case the evidence will hereafter be forthcoming. This doctrine, which may conveniently be designated the chemico-physical hypothesis of life, has readily found its way from the speculative writings of philosophers to the rostrums of some of our teachers of chemistry and physics who boldly declare, in their class-lectures and public addresses, that the forces at work in the inorganic world are fully adequate to explain all the phenomena of living beings, and prophesy that the time is soon coming "when the last vestige of the vital principle as an independent entity shall disappear from the terminology of science." [1]

Now, most of these gentlemen are not embarrassed by any very definite or detailed knowledge of the physiological and pathological phenomena which a tenable theory of life must be competent to explain, while they do know, or at least ought to know, a great deal of chemistry and physics; the confidence with which they maintain their creed is therefore readily understood. Much more surprising is it to find the same doctrine embraced by numerous zoologists, physiologists, nay, even pathologists, among them men who cannot for a moment be supposed to be unacquainted with the phenomena to be explained, and of whose abilities and reasoning powers it is impossible for me to think or speak otherwise than respectfully. Yet I cannot but believe that they have adopted the chemico-physical hypothesis, not so much because they are really satisfied with it as a scientific explanation of all the phenomena, as because they are unduly biased in its favor by the utterances of the great philosopher who has done, as I think we will all agree, such good service to biological science by elaborating and popularizing the doctrine of evolution.

It is only natural that such a bias should exist. The discussion of the nature of life—in the case of man at least—has always, and not unreasonably, been conjoined with the discussion of the nature of the soul, and the philosophers who have won highest repute in the latter discussion, have always been willing enough to offer solutions of the life-problem, and have never had any difficulty in finding followers even among those whose special lines of investigation might be supposed to impose upon them the duty of independent inquiry into the meaning of life.

Just as it was in the old time, with regard to this matter, so it is now. When Galen undertakes to discuss the complex phenomena of the Psyche, as manifested by the human species, he openly and continually confesses the extent to which he relies upon the authority of Plato; and when the dicta of the master are such as to require a special effort of faith on the part of the disciple, he honestly exclaims "Plato indeed appears to be persuaded of this, as for me, whether it be so or not, I am unable to dispute the question with him."[2]

In like manner, did they venture to be as frank as Galen was, most of the modern biologists who have adopted the chemico-physical theory of life would, I presume, confess "as to this matter our opinions are derived from Mr. Herbert Spencer's Principles of Biology—what are we that we should venture to dispute as to questions like these with him."

Nevertheless in striking contrast to this chemico-physical hypothesis of life, which is to be regarded as the fashionable faith of the hour, there still survives in many quarters, and especially among physicians, a disposition to regard indiscriminately almost all the phenomena of living beings as peculiar manifestations of a vital principle. So strong, indeed, is the faith of some of these modern vitalists, that they seem to shut their eyes to the evidence already in our possession as to the actual participation of known chemical and physical forces in the operations going on within living bodies, and appear almost to resent the willing aid that chemistry and physics afford to the physiological investigator of the present day.

Nay, further than this, in the inevitable reaction that is beginning to make itself felt against the avowed revival of the materialism of Epicurus and Lucretius—for we all know now that the chemico-physical hypothesis of life is not a new induction of modern science, but an ancient Greek speculation reappearing in modern petticoats—that other Greek speculation of the threefold Psyche, the doctrine taught by Plato and Aristotle, and which Galen accepted on their authority, the doctrine of a vegetable, an animal, and a rational soul, a human trinity coexisting in every human being, is once more rehabilitated and finding followers—likely, indeed, as I think, to obtain more followers than perhaps any of you yet suppose. And these followers are by no means confined to metaphysicians or churchmen, they can be found also already among the biologists. It is an English biologist of good repute, and of no mean abilities, who takes occasion, in a technical biological work published this very year, to express his belief that the Greek conception of the threefold Psyche "appears to be justified by the light of the science of our own day."[3]

For myself I must confess at once that I am quite unable to join either of these opposing camps as a partizan. I cannot accept the more strictly vitalistic views, because I am compelled continually to recognize the operation of purely chemical and physical forces in living beings. On the other hand, there are whole groups of phenomena characteristic of living beings, and peculiar to them, for which the chemico-physical hypothesis offers no intelligible explanation.

From this point of view the various processes and functions of living beings may indeed be divided into two classes, of which the first may be regarded with more or less certainty as the special results, under special conditions, of the very same forces that operate in the inorganic world; while the second, to which alone I would apply the term vital, are not merely in every respect peculiar to living beings, and hitherto utterly inexplicable by the laws of chemistry and physics, but are so different in character from the phenomena of the inorganic world that it does not seem rational to attempt to explain them by these laws.

Let me refer briefly to the processes and functions belonging to the first class. Here I place all those more strictly chemical processes by which, within the very substance of vegetable protoplasm, inorganic elements are combined into organic matter, as well as those which produce all the various subsequent transformations, whether in plants or animals, of the organic matter thus prepared. This general conception includes of course, in the case of the higher animals, all the chemical phases of the processes of digestion, assimilation and tissue-metamorphosis or metabolism, including secretion and excretion; in the case of the lower animals and plants, so much of these several functions as belongs to each species.

Now please to understand that when I say I recognize all the chemical phases of these processes to be the results of the ordinary chemical laws, I do not entertain any mental reservation with regard to the unrestricted application of these laws. I cannot for a moment agree with those physiologists who have imagined the vital principle to thwart, or interfere with, or counteract these laws in any way. I know, indeed, that we are far from being as thoroughly acquainted, as we may by and by hope to be, with the chemical phenomena of living beings; that many of the questions are very difficult, so that as yet, with all our labor, we have obtained but partial or even contradictory results; but I find in this only a reason for further investigation—no logical difficulty of a radical kind. In a general way I recognize that the matter of which living beings are composed is built up of elementary substances belonging to the inorganic world, and that it consists of atoms possessed of the very same properties, and obedient to the very same laws as like atoms in inorganic bodies. Yet I confess I find in all this no reason for denying the existence of a vital principle; only I do not figure this principle in my mind as a hostile power interfering in any way with the chemical tendencies of the atoms present; I liken its operations rather to those of the chemist in his laboratory who obtains the results he needs only on the condition of most rigid obedience to chemical laws.

Intimately associated with some of the chemical processes just enumerated are those chemical processes of respiration, in which the chemical affinities of the oxygen of the atmosphere are directly or indirectly the means of promoting tissue metamorphosis, as well as of reducing at once to simpler forms some portion of the various complex substances derived from the food. These chemical processes are undoubtedly the chief original sources of the heat and mechanical power manifested by animals. Of course they receive heat also from without by conduction and radiation; but this is a small matter to the heat generated within them; of course, too, mechanical power is continually transformed into heat within the body of animals, but this neither increases nor diminishes the total amount of energy liberated.

I yield my hearty assent to that modern scientific induction[4] which sees in the potential energy of the complex chemical compounds supplied to animals by their food, the essential source of all the actual energy of the body, whether manifested in the form of heat or work. In a general way the reduction of these complex chemical compounds by oxidation into the much simpler ones, urea, carbon dioxide, and water, is the means by which potential is converted into actual energy. In the case of plants, too, the source of any little heat that may be developed under special conditions, and of such sluggish motions as actually occur, is doubtless to be found in the reduction to simpler combinations by oxidation of a part of the organic matter already formed. The chief function of the vegetable world, however, is to build up, by means of the solar energy, those complex and unstable organic compounds that supply the animal world with food. Nevertheless, while I yield my hearty assent to this generalization, and freely admit that it is more than a mere deduction from the general doctrine of the conservation of energy—that in fact it affords the most satisfactory explanation yet suggested for a large number of observed phenomena—it is my duty to caution you against the erroneous supposition that any one has ever yet succeeded in affording a rigorous demonstration of the truth of the generalization by an adequate series of actual experiments.

Various attempts have, indeed, been made of late years to determine experimentally both for animals and for man, the potential energy contained in the food of a given period, and the actual energy liberated during the same time in the form of heat and work. I think, however, that all practical physiologists who have looked into the question will agree with me that the numerical results hitherto obtained must be received with the utmost caution.[5] Difficulties exist on both sides of the problem. It is comparatively easy, no doubt, to obtain a close approximation to the quantity and composition of the food; but to represent numerically what becomes of it in the body, to deduct correctly what passes through unchanged, and ascertain with reasonable accuracy the amount of carbon dioxide, water, and urea, into which the rest is transformed;

these are questions which have taxed the utmost resources of investigators, and as to which our knowledge is yet in its infancy.

On the other hand, the direct measurement of the resulting heat and work has hitherto proved still less satisfactory. It would seem to be a very simple thing to place an animal in a calorimeter, and measure the heat-units evolved in a given time, as Lavoisier and Laplace attempted to do in the latter part of the last century, and we have been told that "Lavoisier's guinea-pig placed in the calorimeter gave as accurate a return for the energy it had absorbed in its food as any thermic engine would have done."[6] But this assertion is not supported by the results of actual experiment. We know now that many precautions, unknown to Lavoisier, must be taken to secure any approach to accuracy in calorimetric experiments with animals, and just as the method is being brought to something like perfection by arranging for the respiratory process and its influence on the results, and by other necessary modifications of the primitive rude attempts,[7] doubts are beginning to arise as to whether after all the conditions in which the animal is placed in the calorimeter are not so far abnormal as seriously to vitiate the results;[8] so that in fact the most approved numerical expressions of the heat-production of the body to be found in the books are based rather upon calculation of the amount that ought to be produced by the oxidation of an estimated quantity of food than upon actual calorimetric observations.

Nor do we find it any easier when we attempt the actual measurement of the amount of work produced by an animal from a given amount of food. Indeed, in attempting to formulate an equation between the potential energy of the food and the actual amount of heat and work in any given case, we are met with the special difficulty that the animal does not evolve less heat because it is doing work than it does when it is at rest; on the contrary, it actually evolves more heat, consuming for the purpose more food than usual—or if this is not forthcoming, consuming a part of its own reserve of adipose tissue—so that from this source fresh complications of the problem arise.

The labor and ingenuity with which all these difficulties have been encountered is certainly worthy of the highest praise, and I willingly admit the probably approximate truth of the figures generally in use, say 2¼ to 2¾ million gramme-degrees as the daily average heat-production of an adult man, and 150,000 to 200,000 metre-killogrammes as his capacity for daily mechanical work.[9] Nevertheless these figures are after all only probable approximations, and there still exists, with regard to these questions, a large and inviting field for the application of chemical and physical methods to physiological research.

All the mechanical work done by living beings is effected by means of certain contractions of their soft tissues. The movements of the amœba, so often described of late years, may be taken as the type of the simplest form of these contractions. Similar movements occur, with more or less activity, in the protoplasm of all young cells, and in the higher animals are strikingly illustrated by the movements of the white corpuscles of the blood and the wandering cells of the connective tissue. In the lowest animal forms these simple amœboid movements of the protoplasm are the only movements, but in the higher forms, besides these, certain special contractile tissues make their appearance, by which the chief part of the mechanical work done is effected; these are the striated and unstriated muscular fibres.

On account of the extreme minuteness of the little protoplasmic bodies in which the amœboid movements are manifested, the investigation of the mechanical means by which these movements are effected has not as yet been attempted, although a great mass of details have been accumulated by actual observation with regard to the phenomena themselves and the conditions under which they occur. Very little more has been done with regard to the contractions of the unstriated muscular fibres. The striated muscles, however, have been made the subject of a host of researches, and I suppose the conclusions to which we may ultimately be led by these can be regarded, with but little reservation, as applicable to the function of the unstriated muscles, and also to the simpler amœboid protoplasmic contractions.

Yet, notwithstanding the vast amount of experimental labor and speculative ingenuity that has been lavished, since the time of Haller, upon the question of the contraction of the striated muscle, it must be confessed in the honest language of Hermann,[10] that the problem still mocks our best endeavors. For myself, I am unwilling to believe that the phenomena of muscular contraction, or indeed, of any of the varieties of protoplasmic contraction by which animals effect mechanical work, will not by and by be fully and satisfactorily explained on chemico-physical principles. I cannot for a moment give my adherence to the dogmatism of those modern vitalists who insist that the contractions of a muscle, or of an amœba, are essentially vital phenomena; for this would be to claim that life can create force. But it would be folly to shut our eyes to the circumstance that no chemico-physical explanation of muscular contraction yet offered has been so convincingly supported by facts as to command the universal assent of competent physiologists.

Of the various hypotheses devised to explain muscular contraction, those which regard the phenomena as in some way resulting from electrical disturbances have long enjoyed great popularity. Such of these hypotheses as still survive are based upon the electrical manifestations actually observed in living muscles. It has been pretty generally accepted in accordance with the observations of Du Bois-Reymond, whose brilliant series of experiments in animal electricity [11] is deservedly renowned, that even quiescent living muscles are in a state of electrical tension. If, for example, a muscle composed of parallel longitudinal fibres, be exposed with suitable precautions, and divided near each extremity by a transverse incision, the surface of the muscle will be found to be positive to the cut ends, and if one of a pair of non-polarizable electrodes, connected with a suitable galvanometer, is placed in contact with the surface of the muscle and the other in contact with one of the cut ends, the existence of a current is made manifest. The conditions are, moreover, such that while the maximum effect is produced when the equator of the surface is connected with the centre of one of the cut ends; more or less current will also be manifested whenever any two points of the surface are thus connected with the galvanometer, provided they are not equidistant from the equator. In such cases the point most distant from the equator is always negative. The electro-motive force of this natural current of the quiescent muscle varies greatly, but has been found by Du Bois-Reymond to amount sometimes to as much as .08 Daniell in one of the thigh muscles of the frog.[12] In muscles of different form, or cut differently from what has just been described, the currents are somewhat differently arranged, but the example just given must suffice for my present purpose.

In accordance with the observations of the same investigator, it is claimed that during a muscular contraction the electrical tension diminishes, the normal muscle-current experiences a negative variation, and this occurs in such a way, that as the wave of actual contraction moves along the muscle, which it does, according to the observations of Bernstein and Hermann,[13] with a velocity of about 3 metres per second, it is preceded by a wave of negative variation. This negative variation is indeed so trifling, if the muscle contracts but once, that it is difficult to observe it; but when the contractions succeed each other with great rapidity, as in artificially produced tetanus, it may become sufficient to neutralize completely the deflection of the galvanometer due to the current of the quiescent muscle.

But the belief that the electrical currents, shown to exist in the quiescent muscles in these experiments, exist also in uninjured animals has not remained unchallenged. Since 1867 it has been attacked especially by Hermann,[14] who has endeavored to show that these currents are produced only under the special conditions of the experiments, and that there are in reality no natural muscle-currents at all. It was well known that the currents observed in the experiments varied greatly under different circumstances, and it seemed a significant fact that they should be most intense when the muscle was removed from the body and had both ends cut off. If the muscle was removed with its tendinous extremities still attached, the current was usually found to be very feeble, or entirely absent, until the ends were well washed in salt and water, or dipped in acid. Du Bois-Reymond had explained this by supposing the natural ends of the muscle to be protected by what he called a parelectronomic layer of positive elements that must be removed before the natural current could be made manifest. On the other hand, Hermann has endeavored to show that the parts injured by the knife, or acted on by the salt or acid, enter at once into the well-known condition of rigor mortis, and only become negative to the still living portions of the muscle in consequence of this change. That electrical disturbances actually occur in contracting muscles he admits, but endeavors to show that they are due simply to the fact that the changes preceding contraction make the affected part of the muscle negative to every part less modified or wholly unaltered. Hence, if an uninjured muscle be caused, under proper precautions, to contract simultaneously in all its parts, it will be found that the contraction is wholly unaccompanied by any muscle-current.[15]

Observations that appear to support these views of Hermann have been brought forward by Englemann.[16] On the other hand

Du Bois-Reymond has defended his views with vigor, and sharply criticised, of course, the labors and logic of his assailant.[17] I need not at present express any opinion as to the merits of this voluminous controversy. It is enough for my purpose to indicate the questions at issue as sufficiently important and uncertain to be well worthy of independent experimental criticism.

Suppose, however, this criticism should result in showing that Hermann is wholly in the wrong, and that the muscle-currents observed by Du Bois-Reymond really exist in healthy muscles. How, then, shall these currents explain the phenomena of muscular contraction? I presume that no physiologist of the present day is misled by the superficial comparison, which Mayer and Amici were led by their microscopical studies of the muscles of insects to make between the striated muscular fibre and a Voltaic pile.[18] But the molecular theory by which Du Bois-Reymond has endeavored to explain his natural muscle-currents and their negative variation would appear to open up an inexhaustible mine of speculative possibilities for those who are inclined to speculate.

Yet the old experiment of Schwann[19] has always been a stumbling-block in the way of any theory that would explain muscular contraction by the action of a force which must increase inversely as the square of the distance between the molecules, for the force of the contraction, as it actually occurs, diminishes as the muscle shortens; and hence we find so good a physiologist as Radcliffe[20] reviving, in a modified form, the old hypothesis of Matteucci,[21] in accordance with which the electrical tension of the fibre, in the state of rest, causes a mutual repulsion of the molecules, and so elongates the muscle, while the contraction is merely the effect of the elasticity of the tissue, which asserts itself so soon as the repulsive force is diminished by the negative variation that precedes contraction.

In consequence of these and other difficulties many physiologists are beginning to regard the electrical phenomena as subordinate accidents of the chemical processes that go on in muscle, and endeavor to explain muscular contraction as resulting directly from these chemical processes themselves. Arthur Gamgee[22] has adopted as most probable the chemical hypothesis of Hermann.[23] This assumes the contraction to result from the decomposition of a complex nitrogenous compound supposed to be contained in the muscular tissue, and named inogen. During contraction inogen breaks down into carbon dioxide, lactic acid, (Fleischmilchsäure,) and gelatinous myosin. The rearrangement of molecules necessary to produce the latter body determines the contraction. Subsequently the gelatinous myosin combines with the necessary materials furnished by the blood, and becomes inogen again. This decomposition and recomposition goes on also while the muscle is at rest, but, as then the gelatinous myosin is reconverted into inogen as rapidly as it is formed, no contraction results.

Du Bois-Reymond declares all this to be merely unsupported hypothesis.[24] Gamgee himself admits that it is, after all, not very clear why the gelatinous myosin should contract. Michael Foster,[25] who wholly rejects this particular chemical hypothesis, nevertheless seems quite sure that the true explanation will be found to be a chemical one. He insists that muscular contraction is essentially a translocation of molecules, and declares that whatever the exact way in which this translocation is effected may be, it is fundamentally the result of a chemical change, or, as he describes it, "an explosive decomposition of certain parts of the muscle-substance."

The purpose I have in view does not require, fortunately, that I should attempt to decide whether these more purely chemical theories of muscular contraction, or the more purely electrical theories, are best entitled to confidence. My object has been effected, if I have impressed you with the fact that wide differences of opinion still exist as to the nature of the process, and that further investigation is indispensable for the settlement of existing controversies.

The subject just briefly discussed brings us naturally to the consideration of the nature of the action of the motor nerves, by which, in all animals possessed of a muscular and nervous system, the contraction of the muscles is regulated and determined.

The hypothesis which identifies the nervous currents with electricity was propounded in the posthumous work of Hausen[26] in 1743, and, notwithstanding all the difficulties and objections it has encountered, still survives in a modified form in many contemporaneous minds. Those who hold to this view appeal in its support to the electrical phenomena actually observed in nerves in accordance with the investigations of Du Bois-Reymond. These observations have long been widely accepted as conclusive proof that natural currents exist in the quiescent nerve of the same general character as those attributed to the quiescent muscle, which I outlined a few minutes ago. The electro-motive force of this current was found by Du Bois-Reymond[27] to be equal to .022 Daniell in the sciatic nerve of the frog. When a nervous impulse passes along the nerve the natural current is diminished; it experiences a negative variation, which, according to Bernstein,[28] when the impulse results from a very potent stimulation, may more than neutralize the natural current. The same physiologist has shown that this negative variation moves along the nerves of the frog at the rate of 28 metres per second; that is, at the same rate as the nervous impulse itself, as determined without reference to the electrical phenomena.

As in the case of the muscle-currents, these phenomena have been differently interpreted by Hermann,[29] who denies the existence of any natural nerve-current in uninjured nerves, and ascribes those observed in the experiments to the circumstance that the parts of the nerve dead or dying, in consequence of the section, become negative to the living nerve. The negative variation produced by the stimulation of a nerve he explains by assuming that the stimulated part of the nerve becomes, in consequence of the changes resulting from the stimulation, negative to the unstimulated parts. I will not attempt to enter to-night into the merits of the controversy still in progress with regard to this question; nor will I pause to discuss the exceedingly curious and interesting phenomena of electrotonus,[30] concerning which, I will only say that the question has even been raised by Radcliffe as to how far these phenomena are peculiar to nerves, and how far they may be regarded as mere phenomena of the electrical currents employed, which would be equally manifested under similar circumstances if a wet string or other bad conductor should be substituted for the nerve.[31]

However these disputes may be ultimately decided; whatever the actual facts with regard to the electrical manifestations in nerves at rest or in action, may ultimately prove to be, there is a group of easily repeated elementary experiments which seem to show pretty distinctly that whatever the nervous impulse may be, it is not merely an electrical current.

It was known already when Haller wrote[32] that a string tied tightly around a nerve, although it in no wise interferes with the passage of electrical currents, puts a speedy end to the transmission of nervous impulses. With this old experimental difficulty uncontradicted, it seems strange that anyone should declare at the present time that "the main objections raised to the electrical character of nerve energy is based upon its slow propagation."[33] In fact this latter objection is altogether a subordinate difficulty which may perhaps be entirely explained away; the main experimental objection does not relate to the velocity, but to the conditions of the propagation of the nervous impulse. If, instead of tying a string around it, the nerve be merely pinched or bruised well with a pair of forceps so as to destroy its delicate organic texture; if it be compressed tightly by a tiny metallic clamp; if it be divided by a sharp knife, and the cut ends brought nicely into contact, or brought in contact with the extremities of a piece of copper wire, it will still conduct electrical currents as well as ever, but can no longer transmit the nervous impulse. So, too, there are certain poisons, such as the woorara, which completely destroy the capacity of the nerve for transmitting nervous impulses, without in the least diminishing its conductivity for electricity.[34]

In view of these and other practical difficulties, the best instructed modern physiologists no longer attempt to identify the nervous impulse with the electrical phenomena by which it is accompanied. Du Bois-Reymond himself has suggested that the nervous agent "in all probability is some internal motion, perhaps even some chemical change, of the substance itself contained in the nerve-tubes, spreading along the tubes."[35] Herbert Spencer came to the conclusion that "nervous stimulations and discharges consist of waves of molecular change"[36] flowing through the nerve-fibres; and I suppose that most physiologists at the present time think of the nervous current in some such way as this. Even those who attach most importance to the electrical phenomena will, I take it, agree with Michael Foster, that these "are in reality tokens of molecular changes in the tissue much more complex than those necessary for the propagation of a mere electrical current."[37]

We do not, however, as yet possess any sufficient foundation of facts on which to build a reasonable hypothesis as to the nature of the molecular disturbances that accompany a nervous impulse. The labors of the physiological chemists have taught us nothing with regard to the changes that go on, except that the axis-cylinder which, in the inactive living nerve is alkaline, becomes acid after long continued activity, or after death.[38] We can measure the velocity with which the impulse travels; we can study the conditions under which it arises; we can believe, as I certainly do, that it will ultimately receive a chemico-physical explanation, but its real nature we do not yet know.

So far as we can ascertain, the phenomena of the conduction of nervous impulses by the sensitive nerves are so similar to those of the conduction of motor impulses, that any explanation ultimately adopted for the one will probably apply to the other also. When, however, we ascend to the study of the nervous centres, by which sensitive and motor nerves are connected together, and attempt the interpretation of the complex functions of nerve-cell, ganglion, spinal cord, and brain, we find that none of the hypotheses hitherto brought forward to explain the observed phenomena repose on any defensible chemico-physical basis.

I cannot, of course, undertake to give to-night even the most meagre outline of the wondrous mechanism which physiological experiments show must exist. That reflex actions, co-ordinated muscular movements, and all the complex phenomena of this class, do depend upon a wonderfully complex mechanism, and occur in strict accordance with the ordinary chemical and physical laws, I do not for a moment doubt, and I cordially invite the co-operation of the chemists and physicists to aid the physiologists in the explanation of this mechanism, for we stand only upon the threshold as yet.

If now we turn from the more general discussion of muscular contraction and nervous action, to the consideration of the several functions carried on in animals, by means of special arrangements of the muscular and nervous systems, we continually encounter the preponderating influence of purely physical laws. The introduction of air into the lungs of breathing animals, and its expulsion thence, is effected in a purely mechanical way, while the exchange of the carbon dioxide of the blood with the oxygen of the inspired air occurs in strict obedience to the laws of the diffusion of gases. The ordinary laws of hydraulics govern the circulation of the blood and lymph, and all the complex visible motions of the body are executed in accordance with the ordinary laws of mechanics; nor is it at all necessary for me to insist upon the purely physical nature of the operations of the organs of the special senses, conspicuously the eye and the ear. For example, so far as concerns the means by which images of external objects are formed sharply upon the retina, the eye is as purely a physical instrument as the telescope or the microscope. But I need not dwell upon this group of phenomena, because the importance of the role of the ordinary physical laws in this domain is conceded, I suppose, by the extremest of the vitalists of the present day.

We see, therefore, that, with regard to a large part of the phenomena of living beings, there are grounds for affirming either that they have already been satisfactorily explained by a reference to established chemical and physical laws, or at least that they are of such a character that it is reasonable to hope they may be thus explained at some future time. Is it possible, then, to return, as some have done of late years, to the old speculation of Des Cartes, and look upon living beings as mere machines? To do so, it will not suffice to image to yourselves ordinary machines in which fuel yields force. To satisfy the chemico-physical hypothesis of life you must suppose machines that build themselves, repair themselves, and direct, from time to time, new applications of their energy in accordance with changes in the environment; nay, more—machines that accouple themselves together, breeding little machines of the same kind that grow by and by to resemble their parents, and all this self-directed, without any engineer. But even Des Cartes required an engineer—the soul—to run his man-machine, and the logic which compelled him to this view applies just as forcibly to all the modern machine conceptions of living beings.

I have already asserted that there are whole groups of phenomena characteristic of living beings, and peculiar to them, which cannot be intelligently explained as the mere resultants of the operation of the chemical and physical forces of the universe. These phenomena I refer—I avow it without hesitation—to the operations of a vital principle, in the existence of which I believe as firmly as I believe in the existence of force, although I do not know its nature any more than I know the nature of force. If, for convenience, at any time, I compare the living body to a machine, I must compare the vital principle to the engineer—it is the director, the manager if you will, but it does not supply the force that does any part of the work. Let us consider, then, in the remainder of this discourse, the phenomena which indicate the guidance of the vital principle.

The first group of phenomena belonging to this second class are those forced upon our attention whenever we attempt to study the question of the origin of life. It has seemed to some of our contemporaries that, in accordance with the doctrine of evolution, as deduced by Mr. Herbert Spencer from the great truth of the persistence of force, life ought always to arise spontaneously out of inorganic matter whenever the necessary materials and other conditions of life are brought together. Indeed, if there be nothing more or other in life than force, I confess I do not understand how this conclusion can be logically escaped; and yet, when we come to interrogate nature, we find that, in point of fact, things do not happen so.

The sun may stream all the enormous energy of his rays upon the slime of the Nile, but he generates no monsters; nay, not even a bacterium, except in the presence and under the direction of pre-existing life. Our biological knowledge has so far advanced that it is easy for us to get together mixtures of matter, for the most part derived from pre-existing living beings, which are peculiarly well fitted to supply the materials needed for the building up of a variety of low forms of life, and the extent of our present knowledge of the conditions favorable to the development of these low forms of life is shown by the rapidity with which they do develop from a few individuals to countless millions, if only a few individuals are introduced as parents into our flasks and brood-ovens. The species to which the countless progeny belongs, depends always upon the species of the parents we introduced by design or accident, and if parents of several species are introduced we may imitate on a tiny scale the great struggle for existence, and witness the survival of the fittest. Never, however, has the spontaneous generation, out of inorganic matter, of a single living form been yet observed.

Speculative considerations have, indeed, from time to time led certain enthusiasts to desire earnestly that it might be observed; and when we consider on the one hand the influence of pre-existing bias, and on the other the intricacy of some of the experimental processes in question, it is by no means necessary to charge dishonesty upon those who, from time to time, have actually fancied that their desires have been realized to the extent of the spontaneous generation of bacteria at least. When we consider the immense development of the trade in canned food, which could not exist for a single summer's day, if these experimenters were not mistaken, it will be seen how little need there was for renewed scientific experiment to refute their conclusions; but it is a noteworthy fact that among those who have contributed most by exact research to recent scientific demonstrations of the truth, that life never arises except from pre-existing life, are to be found some of the most earnest and eloquent advocates not merely of the doctrines of evolution, but of its supposed corollary, the chemico-physical hypothesis of life.

I sympathize heartily with those who, recognizing that the supposition of the spontaneous origin of life on our globe is flatly contradicted by the facts of science, have endeavored to escape the difficulty by imagining the earliest parent living forms to have been brought to our earth on the surface of meteoric stones or other cosmical bodies. This hypothesis, put forward originally on purely theoretical grounds, has recently acquired a certain degree of support from the published observations of Hahn and Weinland,[39] who believe they have recognized the remains of humble coralline forms in thin sections of meteoric stones collected in Hungary. Yet these observations, if indeed they should prove to be correct, would rather afford indications of the existence of life in other worlds than ours, than show that living forms could survive the high temperature to which such cosmical masses must be exposed during their transit through our atmosphere; and even should we find reasons for ultimately adopting this hypothesis, we should not have solved the problem of the origin of life, but only removed it entirely beyond the domain of further scientific investigation.

If, however, we reject this view, and still mean to support the chemico-physical hypothesis of life, we shall have to resort to a still more improbable supposition. We shall have to suppose that although in the present order of things life can only arise out of pre-existing life, the order of things was at some past time so far different that life could then arise out of inorganic matter; a supposition which implies an instability in the course of nature that is contradicted by all the teachings of science.

I willingly admit that, in view of our present scientific notions of the cosmogony, it is impossible to believe that life always existed upon this planet. I willingly admit that life on the earth must have had a beginning in time. But we do not know how it began. Let us honestly confess our ignorance. I declare to you I think the old Hebrew belief, that life began by a creative act of the Universal Mind, has quite as good claims to be regarded a scientific hypothesis as the speculation that inorganic matter ever became living by virtue of its own forces merely.

If we turn now to the consideration of the processes of growth, we shall find additional reasons for believing in the existence of a vital principle. Let us consider first, in the most general way, the conditions under which those strictly chemical processes occur, to which I have already alluded, and by which the inorganic atoms are combined into organic matter. I repeat it, I do not for a moment question that the actual force by which these processes are compelled exists in the solar rays, and that it is, after all, the solar energy thus stored up in the vegetable protoplasm and its products that supplies, by its subsequent liberation, all the force manifested by living beings. Yet, let me beg you to observe that in all the myriads of years during which the solar energy has streamed upon the earth, that energy has never, on any occasion that we know of, determined the combination of inorganic atoms into organic matter, except within the substance of already living protoplasm. The water and carbon dioxide and ammonia in the atmosphere and in the soil, come into contact with each other, within the substance of porous inorganic clods on the surface of the soil, much as they do in the substance of protoplasm, and the equal sun warms both alike; but in the clod they remain water, carbon dioxide, and ammonia; in the protoplasm, provided only that it is living protoplasm, they combine into starch or oil, or even into protoplasm itself. The essential condition, then, of this storing up of the solar energy for the subsequent use of living beings is the presence of life, and in these fundamental operations the mighty force of the sun acts, in the fullest sense of the words, the part of the servant of life.

The view thus suggested, that we have here to do with something more than the mere operation of the inorganic forces, is still further strengthened when we come to consider more in detail the phenomena of the growth of living beings, whether plants or animals. The better we become acquainted with these phenomena the more fully we become convinced that we have to do with processes for which the inorganic world affords no parallel.

Linnæus, indeed, declared, "lapides crescunt," using the very same phrase which he applied also to plants and animals.[40] But it is impossible to maintain this assertion without adopting the most superficial view of the growth of living beings, and defining the process to consist merely in increase of size. That this should have appeared reasonable, in the time of Linnæus, need excite no surprise; but it seems strange to find so astute a thinker as Mr. Herbert Spencer repeating the old fallacy in the first chapter of his Inductions of Biology, and declaring: "Crystals grow, and often far more rapidly than living bodies."[41] Then, after instancing the formation of geological strata by the deposit of detritus from water, as well as the formation of crystals in solutions, as examples of growth in the inorganic world, he asks: "Is not the growth of an organism a substantially similar process?" and adds: "Around a plant there exist certain elements that are like the elements which form its substance, and its increase in size is effected by continually integrating these surrounding-like elements with itself; nor does the animal fundamentally differ in this respect from the plant or the crystal."

Now, as opposed to this, I must express my belief that the more we know of the actual details of the process of growth in plants and animals the more clearly it will be seen that this process does differ so fundamentally from that by which a crystal is formed and increases in size, or from any increase in size of inorganic bodies, that the same scientific term cannot, with any propriety, be applied to both, however long popular usage may have given to both a common name. When inorganic bodies increase in size the additional atoms are deposited on their external surfaces; or, if a fluid, after penetrating the interstices of some porous body, deposits there any material held in solution, the mass, indeed, is increased thereby, but not the size. When, however, vegetable protoplasm grows, it does not merely integrate with itself certain elements around it like the elements which form its substance; the needed elements exist in compounds quite unlike itself, and it combines them together into protoplasm in all parts of its mass, so that it grows by a process of intussusception wholly unlike anything that occurs in the inorganic world. In the case of animal protoplasm, the mode of growth by intussusception is the same, but the capability of combining together mere inorganic elements into its own substance is lost; and, besides these, a certain amount of pre-existing vegetable or animal protoplasm must be present in the food, or growth will not go on.

In both cases, when the growth has proceeded to a certain extent—within certain definite limits—a new characteristic phenomenon occurs in a growing mass of vegetable or animal protoplasm; it multiplies by division, its whole mass participating in the act, in accordance with one or other of a few definite methods. This process is repeated again and again. The progeny may separate, without modification, as independent forms, or, as in the case of the more complex organisms, they may cohere together, and the process culminates by groups of them undergoing certain definite and peculiar transformations, after which further multiplication becomes rare or ceases altogether, and the growth of the complex organism is thus limited.

I cannot, of course, attempt this evening to describe all the known details of the process of growth which I have thus hastily sketched; to give you a really satisfactory account of them would require a series of lectures. But I do not hesitate to say that the more fully you know these details the more unscientific you will think the attempt to class them as in any way similar to the circumstance that inorganic crystalline compounds seem "each to have a size that is not usually exceeded without a tendency arising to form new crystals, rather than to increase the old." It is, at the best, a waste of words to attempt to explain complex phenomena by comparing them to simpler ones which are fundamentally unlike them.

I have but now referred to a process by which, in the growth of the more complex living beings, the small primitive protoplasmic mass, out of which each individual arises, subdivides and produces a numerous brood of protoplasmic masses, at first closely resembling the parent mass, but after a time differing from it more and more, and finally undergoing transformations into definite and peculiar forms. This process, which does not take place in any disorderly manner, but in a very characteristic and definite way in each individual form, is designated by the term development. In point of fact, so far as it consists in the mere growth and multiplication of the individual elements that compose the organism, and the increase in size of the organism itself on account of these processes, it is properly designated by the term growth. In so far, however, as the individual elements are differentiated, and the wonderful architecture of the living being, with its organs and systems, is completed thereby, it is properly designated by the term development.

Nothing like the process of development as thus defined exists in the inorganic world, and in all the attempts at such a comparison that it has been my fortune to meet, the most fundamental facts of the development of living beings have been persistently ignored. Among these fundamental facts I invite your attention especially to the circumstance that there is something in the miscroscopic mass of protoplasm, out of which, even in the case of the highest and most complex living beings, each individual arises, that goes even further in determining the direction in which the individual shall develop than the pabulum, or environment, or all the mighty chemical and physical forces that are brought into play as the process goes on. In a word, the individual developes after the pattern of its parent, or not even all the solar energy can compel it to develop it at all.

We are thus brought face to face with the facts of sexual generation, and especially of heredity, with all their wide bearings on the great biological questions of natural selection and the origin of species. Into the details of these large questions the limits of the hour will not permit me to enter. Could I take time to do so, I am satisfied that at every step I should be able to collect for you additional evidence of the existence of a vital principle. Still I regret this the less because most of you, I think, are so familiar with the modern literature of these subjects, and especially with the admirable writings of Mr. Darwin, that I feel sure, if I can succeed in giving you a clear outline of my views, much that I should say, had I time, will suggest itself to your own minds. In a general way, however, when we study, in the history of life upon this globe, the double phenomena of long continued persistence of type, and of slow variation continually occurring, we will find that almost all biologists, whatever their theory of life, explain these phenomena on the one hand by heredity, on the other by the sensibility of the organism to the influence of the environment.

Both heredity and the influence of the environment may be very conveniently studied in those simplest organisms in which each individual consists of a single minute mass of naked protoplasm, as in certain rhizopods, for example, the amœba. These tiny creatures produce a progeny which preserves the parental type as closely as is done by the offspring of the higher animals. Their sensibility to the influence of the environment is manifested in several ways. They grow, that is they appropriate materials from the environment, in the way I have already specified; they manifest automatic movements, that is, on encountering food, obstacles, or other disturbing external circumstances, movements result the direction and energy of which are in no wise determined by the character or force of the external influences, or as they may be conveniently termed the stimuli by which these movements are provoked; and finally, simultaneously with the process of growth, a certain metamorphosis, or metabolism, of the protoplasm is continually going on resulting in the formation of excrementitious substances which are continually being excreted.

The processes of growth and metabolism exhibit different degrees of intensity in accordance with variations of the environment, and whatever physical theory of the mode in which the protoplasmic motions are produced we may adopt, the mechanical force manifested can only be supposed to proceed from the decomposition of a part of the protoplasm itself into simpler compounds, that is, from a particular kind of metabolism. Hence you will I think, be quite prepared to hear me speak of all the circumstances in the environment that so act upon living protoplasm as to increase its growth or metabolism, as stimuli, and of the property of living protoplasm by which all its responses to stimuli are guided, as irritability, instead of limiting these terms to the phenomena of automatic movement only, as was formerly done. This irritability of living protoplasm determines the direction in which its internal forces shall be manifested. Speaking of it as I do, perhaps you would wish me to call it sensibility rather than irritability, and I do not know that I should object very strenuously to any one who wished to do this. But however you may name it, it is this vital property of all living protoplasm that produces the sensibility to changes in the environment which has been the main factor in the gradual evolution, during the ages, of the highest and most complex from the simplest and lowest living forms.

Against this view it has been urged with much ingenuity that protoplasm is the material substratum of life, and life merely a property of protoplasm; that is, if the words have any meaning at all, that life is the resultant only of the forces inherent in the inorganic atoms of which the protoplasm is built up. Now, in the first place, no one has ever yet been able to show, by any conceivable synthesis, how the forces known to belong to the several kinds of inorganic atoms of which protoplasm is composed, could by their combination, produce the characteristic phenomena of living protoplasm, namely, the phenomena of irritability, as I have just described them. But, in the second place, this speculation appears to be pretty flatly contradicted by the circumstance that, although protoplasm can only be formed within the substance of previously existing living protoplasm, it can continue to exist, it does continue to exist as protoplasm after it has ceased to live. Not merely can it persist for a time without chemical change as dead protoplasm, it can subsequently serve as food and be reconverted into living protoplasm once more. Bear in mind, however, that this change can only be effected within the substance of the living protoplasm of the animal that assimilates this food. It is not effected by the chemistry of digestion, that merely makes peptone of the protoplasm; merely makes it soluble enough to pass into the substance of the protoplasmic masses that are to appropriate it. These considerations, then, would seem to show that the material, protoplasm, cannot be rightly believed to be of itself the cause and essence of life.

If I should pause here, it seems to me that I should have brought forward adequate reasons for believing in the existence of a vital principle. But I cannot pause here. Beyond and above all this there is another great group of phenomena peculiar to living beings—a group of phenomena concerning which, in my own individuality, I have knowledge at least as positive as any I possess of the existence of force, and which I am led, by a logic quite as convincing as that by which any general proposition with regard to the external world is proven, to believe exists in like kind and degree in the case of my fellow-man. I refer to the phenomena of the perceiving, emotional, willful, reasoning human mind. Into the argument that makes it highly probable that a similar but less and less perfect mind exists in the animal world, and identifies with mind the sensibility of the lowest animal forms, and even that of vegetable protoplasm, I will not attempt to enter to-night. Mr. Herbert Spencer himself has presented this view with so much ingenuity, that, without committing myself to an approval of all his details, I must content myself by referring you to his writings for one of the best discussions of this matter. It will be sufficient for my present purpose to close this discourse by the presentation of a few considerations in relation to mind as it exists in man.

For myself I know mind only as a manifestation of life, if indeed it is not the essence of life. But the old doctrine of Epicurus, handed down to us in the poem of Lucretius, that in some way or fashion mind is produced by the clashing together of the atoms, has been boldly revived of late years, and transmuted into a form more plausible to modern thought, although just as unsupported by any actual knowledge of facts.

No one has done this more boldly or more cleverly than Mr. Herbert Spencer has done in his First Principles, and of course you are all familiar with the ingenious argument, in favor of this view, which runs through that masterly work. It would be, from many points of view, profitable, but it would be a very laborious task to attempt the critical discussion of his argument. It must suffice, for my present purpose, to point out that two of the fundamental assumptions upon which that argument is based are wholly undemonstrated. The first assumption is, that mind is itself a force;[42] the second, that mind cannot be conscious of itself, but only of the external world.[43]

If I could bring myself to believe that mind is, in any proper sense of the word, a force, and that such popular metaphorical expressions as mental force or mental energy accurately described the phenomena, I should certainly expect to find at least some shadow of proof for Mr. Herbert Spencer's assertion, that mental operations fall within the great generalization of the correlation and equivalence of the forces. On the contrary, however, you will find, on reading his lucid periods, that his whole argument relates to those physical conditions in the organs of sense and in the muscular and nervous systems, which are the antecedents of perception—which are, in fact, the things really perceived—and in no sense constitute the perceiving mind. Between strictly mental phenomena and the physical forces no one has as yet even attempted to establish a numerical equivalent; nay, more, the correlation of thought with the physical forces is not only undemonstrated, it is utterly unthinkable. You can conceive several different ways, it matters not whether true or false, in which the motions we know as heat might be converted into those we know as light, and so on with the other physical forces; but you cannot represent mentally any intelligible scheme by which any of the physical forces can be converted into the simplest or most elementary thought.

As to the question of self-consciousness, it seems as if the great philosopher were reasoning in a circle. He first assumes that the fundamental condition of all consciousness is the antithesis between subject and object,—which is true only with regard to consciousness of perception, the form of consciousness by which we become acquainted with the non ego,—and then he concludes that there can be no consciousness of the ego because it cannot fulfil these conditions. That is, in a word, he denies consciousness of the ego, because it is not consciousness of the non ego. Really it appears to me that, as against such a philosophy as this it is not amiss to appeal to "the unsophisticated sense of mankind," of which Mr. Mansel speaks.[44] But there is fortunately a better philosophy than this; a philosophy which recognizes the validity of the mind's self-consciousness as at least fully equal to the validity of its consciousness of the conditions of the body by which it obtains a knowledge of the external world. By this self-consciousness I know, with a certainty which no doubt can ever disturb, that I have a mind; and by rightly applying my reasoning powers to the data of my self-consciousness, I can learn much that will be useful to me with regard to my mental processes and the methods of employing them. But here I have to stop. I can learn nothing, whether by consciousness or by reasoning, with regard to the real nature of my conscious mind, and however much it may long for immortality, neither philosophy nor science afford any foundation of proof upon which it might build its hopes.

I have already said that I know mind only as a manifestation of life. Its operations are intimately connected with the chemical and physical phenomena of living beings, and it exercises over them a certain directing influence, the nature of which we do not understand. The obedience of our voluntary muscular actions to the mandates of the guiding will is a familiar illustration of this directing influence. On the other hand, all the knowledge of the external world on which the mind exerts its reasoning power reaches it through the organs of sense and the nervous system. Indeed, our studies of the phenomena of sensation compel us to conclude that what our mind really perceives, when it takes cognizance of the external world, is merely the ever-changing panorama of our own cerebral states. It should be anticipated, therefore, that disturbed or morbid conditions of the brain would lead to irregular or disorderly mental operations; and the circumstance that this really happens, affords no better proof of the materiality of thought than is afforded by the circumstances of our ordinary normal thought.

So, too, since the cerebral changes, which the mind perceives, are themselves of a purely chemico-physical nature, it should be anticipated that, like the metabolic processes in other tissues, they would be accompanied by an increased excretion of characteristic waste-products, by evolution of heat and by afflux of blood. Experimental investigation has been directed to each of these points, and some important observations have no doubt been made; but much of the testimony is conflicting, and our knowledge is still so incomplete that further inquiry in each direction is greatly to be desired.

This is particularly the case with regard to the chemical questions connected with the metabolism of the brain. In the first place our knowledge of the chemical composition of brain-substance is still in its infancy. The view that its characteristic ingredient is the phosphorized nitrogenous body described in 1865 by Liebreich under the name of protagon has been strongly controverted by Diaconow, Hoppe-Seyler, and Thudicum, while recently it has been reaffirmed by Gamgee, and Blankenhorn.[45] But even should this view turn out to be well founded, we have yet everything to learn with regard to the transformations protagon undergoes during functional activity, and the nature of the resulting waste products.

Long before Liebreich announced the existence of protagon, however, the attention of the physiological chemists had been directed to the prominence of phosphorous as an element in the composition of the cerebral substance, and it had been suggested that a part of the phosphoric acid excreted in the urine might be derived from the metabolism of the brain. As early as 1846 Bence Jones[46] had observed an excess of phosphatic salts in the urine during certain brain diseases, notably acute inflammations, and an observation published in 1853 by Mosler[47] appeared to indicate that a similar excess followed intellectual activity.

Byasson [1868] in his essay on the relation between cerebral activity and the composition of the urine,[48] reports a number of urinary analyses which support the view that the excretion of alkaline phosphates by the kidneys is habitually increased during mental work. This opinion has also received a certain degree of support from the more recent papers of Zuelzer[49] and Struebling;[50] nevertheless it is impossible to study the detailed observations upon which it is based without feeling how meagre and unsatisfactory the evidence relied upon really is. It is at best only sufficient to indicate the importance of further inquiry, and to suggest the necessity of avoiding certain obvious errors of method which complicate and obscure the results of the investigations hitherto made.

The opinion that mental effort is accompanied by an increase in the temperature of the brain was first propounded by Lombard in 1867. Using a delicate thermo-electric apparatus of his own contrivance, he observed during mental effort a rise of the surface temperature of the head, which sometimes amounted to as much as one-twentieth of a degree centigrade.[51] Subsequent and more elaborate investigations confirmed him in this conclusion, which has also been supported by observations made with thermo-piles by Schiff and Bert, as well as by the use of surface thermometers in the hands of Broca and L. C. Gray of Brooklyn.[52] Gray claimed to have observed a maximum rise of as much as two and a half degrees Fahrenheit. These physicians and some others have also investigated the relative temperature of the two sides of the head, of different regions on each side, the variations produced in certain regions by voluntary muscular movements, and those resulting from localized brain diseases.[53]

To attempt any discussion of these interesting studies, and their conflicting results, would lead me altogether beyond my prescribed limits. It is enough for my present purpose to point out that the recent investigations of François Frank[54] would seem to indicate that the variations of temperature actually observed are chiefly due to changes in the cerebral circulation. Plunging suitable sounds, connected with a thermo-electric apparatus, into the brains of animals to different depths, Frank found that the deeper parts of the brain are always warmer than its superficial layers. The superficial layers are continually cooled by radiation, and their temperature is a degree, or more than a degree centigrade, lower than that of the deeper parts. Even these, however, are .1° to .2° centigrade cooler than the blood in the thoracic aorta, and it will therefore readily be understood that a relaxation in the muscular coats of the cerebral vessels, permitting the more rapid circulation of a larger quantity of blood, would be promptly followed by an increase in the temperature of the superficial parts of the brain. None of the observers I have cited have reported a surface temperature of the head during mental effort that is too high to be accounted for in this way; and if, as I willingly concede is probable, there is really an increased heat-production in the brain itself, it is wholly masked by the more considerable change due to afflux of blood.

Now a consideration of the phenomena of blushing, and certain well known sensations in the head, might lead us to expect that emotional and mental conditions would prove to be attended by increased activity in the circulation of the blood in the brain; yet many difficulties have hitherto been encountered in the attempt to demonstrate experimentally that this is true. Mosso of Turin supposed that he had succeeded in doing this with his plethysmograph.[55] The instrument is essentially a cylinder of water, into which the arm is introduced and so fastened in place by a caoutchouc membrane that the slightest increase or diminution in the volume of the arm will cause the rise or fall of the water, through a tube connected at one end with the interior of the cylinder and at the other with a suitable recording apparatus. The pen or pencil of this apparatus inscribes a curve that rises or falls with the fluid in the tube. Among the curious observations made with this instrument, Mosso reports that the mental operations and emotions of the persons he experimented on were accompanied by a fall of the curve, which he regarded as proof that more blood goes to the brain and less to the arm during emotion, or mental action, than at other times. But the following year these observations were repeated with great care, and with an improved plethysmograph by Basch, of Vienna,[56] who failed to verify them. Most of the phlegmatic Germans on whom he experimented did sums in their heads, and otherwise exerted their minds, without producing the slightest modification of the curve, and none of them appear to have been as emotional as Dr. Pagliani, of whom Mosso relates that, his arm being in the plethysmograph, when the revered Prof. Ludwig entered the room the curve fell as if he had received an electric shock. Basch has cautiously investigated the causes of the varying quantity of blood in the arm in these experiments, and has clearly shown how many general and local conditions concur in producing the result. Especially has he emphasized the effect of variations in the abdominal circulation, which appear to exercise a much more considerable influence upon the size of the arm than any changes that occur in the brain.

In subsequent works Mosso has stated that during mental effort, such, for example, as is required to multiply small numbers in the head, the radial pulse, as recorded by the sphygmograph, is shown to become somewhat more frequent, and the recording lever does not rise so high as at other times.[57] Thanhoffer, who has pointed out that in these observations the influence of respiration on the pulse was neglected, concluded, nevertheless, from his own sphygmographic observations, that after due allowance is made for this complicating influence, it must be conceded that cerebral activity does exercise a certain effect upon the pulse, and in the direction stated.[58] Eugène Gley, in a recently published essay, claims to have obtained similar results, and states that at the same time the sphygmographic trace of the carotid artery shows a higher upstroke of the recording lever, and other indications of dilatation of the vessel.[59] While these observations are not sufficiently numerous, or free from objections, to be accepted without question as proof that an increased supply of blood to the brain invariably accompanies mental effort, they are certainly sufficient to encourage further labor in this interesting field.

But if the arguments in favor of the purely material nature of our mental operations that have been based upon the imperfect results of the three lines of investigation I have just referred to must be rejected as utterly fallacious, what shall we say of the logic that attempts to draw a similar conclusion from the results of those inquiries into the phenomena of personal equation which aim at determining the time that must be allowed for the mental operation involved?[60] Do we, then, indeed need the beautiful experiments of Hirsch and Donders[61] to prove that thought occupies time? Whence, indeed, do we derive our primitive conceptions of time save from our consciousness of the succession of thought? And how could even the shortest time be occupied by even an infinite number of thoughts if each thought did not occupy at least some time, however brief?

I have thus, gentlemen, attempted to show that we are logically compelled to invoke the existence of a vital principle in order to account for certain important groups of phenomena occurring in living beings which cannot possibly be explained by the chemical and physical forces of the universe. These phenomena form a series, at one end of which we find the mere irritability or sensibility of the humblest mass of living protoplasm; at the other the reasoning faculty of the human mind. From the one extreme of this series to the other I recognize the manifestations of the vital principle. I willingly confess that I know nothing of the ultimate nature of this principle, except that it must be very different from the chemical and physical forces whose operations I have learned to recognize in the organic as well as in the inorganic world; nevertheless I am compelled by my study of the phenomena to conclude that it exists. I know that Mr. Huxley, only last summer, declared in the International Medical Congress at London, that the doctrine of a vital principle is the "asylum ignorantiæ of physiologists;"[62]

but this ancient sarcasm has now been applied to so many things that it has long since lost whatever sting it may once have possessed, when it was fresh and new. And I also know that one of the chief characteristics of true science is the sharpness with which it enables us to discriminate between that which we have proven and really know and that which we have not proven and do not know. Better far is it, and a thousand times more in accord with the simple honesty of science, to acknowledge frankly the truth that phenomena occur in living beings which the inorganic forces do not explain, than to mistake our wishes for discoveries, to convert conjectures into dogmas, or, worst of all, to transform an undemonstrated hypothesis into a superstitious, aggressive, and intolerant creed.

Nor will the soundness of the conclusions, at which the present generation shall arrive as to this matter, be without its practical effect upon methods of biological research, and the consequent future progress of biological science. It is not a mere metaphysical subtlety, but a subject of practical importance that I have asked you to consider to-night. For if the chemico-physical hypothesis of life be true, the only road of progress in biology lies through the chemical and physical laboratories. Now, I have already this evening more than once indicated how highly I esteem the class of biological work that has already been done in these laboratories, and I have endeavored to show how large is the unexplored biological field that can be explored only in this manner. But in addition to all that we can ever hope to do in this direction—and I insist upon its importance—I insist also upon the importance of other lines of work: I insist upon the importance of the systematic study of the phenomena of growth and development, of generation and heredity, of sensibility and mind. All that can thus be learned we need to know, and not merely for its own sake. This knowledge is indispensable to the right interpretation of the succession of life upon the globe in the past, and the successful direction of the interference of the human will with the future succession of life upon the globe in accordance with human necessities. We shall make slow progress in this direction if we confine our efforts to the application of chemistry and physics to those phenomena of living beings that can be thus explained. The other phenomena, not thus explicable, must also be studied in detail, arranged into orderly groups, and made the basis of such inductions as our knowledge of them may warrant. It is only by pursuing this method that we can hope ultimately to acquire, with regard to the phenomena of living beings, that power to predict, which is the criterion of true science, and that power to control, which we so sorely need.

NOTES.

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[1] George F. Barker—*Some Modern Aspects of the Life Question.* Address as President of the Amer. Ass. for the Advancement of Science. Boston meeting, August, 1880. Proceedings, Vol. XXIX, Part I, p. 23.

[2] Galen—*Quod animi mores corporis temperamenta sequantur*, Cap. 3. [Kühn's Edit., T. IV, p. 772.]

[3] St. George Mivart—The Cat. London, 1881, p. 387.

[4] First taught by J. R. Mayer—*Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel: Ein Beitrag zur Naturkunde.* Heilbronn, 1845.

[5] See, for example, M. Foster—*Text Book of Physiology*, 2d Edit., London, 1878, p. 355.

[6] Barker—*op. cit., supra.*

[7] See H. Senator—*Unters. über die Wärmebildung und den Stoffwechsel,* Archiv. für Anat. Phys. und wiss. Med., 1872, S. 1.

[8] Foster—p. 368, *op. cit., supra.*

[9] L. Landois—*Lehrb. der Phys. des Menschen*, Vienna, 1879, S. 402.

[10] L. Hermann—*Handb. der Phys.*, Bd. I, Th. 1, S. 242.

[11] Emil Du Bois-Reymond—*Unters. über thierische Elektricität*, Berlin, 1848–60, and *Gesammelte Abhandl. zur allgemeinen Muskel-und Nervenphysik*, Leipsic, 1875–77.

[12] Du Bois-Reymond—*Ges. Abhandl.*, Bd. II, S. 243.

[13] Bernstein—*Unters. über den Erregungsvorgang in Nerven-und Muskelsysteme*, Heidelberg, 1871; also Du Bois-Reymond's Archiv, 1875, S. 526; Hermann in Pflüger's Archiv, Bd. X, 1875, S. 48.

[14] L. HERMANN—*Weitere Unters. zur Phys. der Muskeln und Nerven*, Berlin, 1867; also *Handb. der Phys.*, Bd. I, Th. 1, Leipsic, 1879, S. 192 *et seq.*

[15] HERMANNN—*Handb. der Phys.*, Bd. I, Th. 1, S. 215.

[16] ENGELMANN—Pflüger's Archiv, Bd. XV, 1877, S. 116 *et seq.*

[17] DU BOIS-REYMOND—*Ges. Abhandl.*, Bd. II, S. 319 *et seq.*

[18] MAYER—Müller's Archiv, 1854, S. 214; AMICI (1858)—Translation in Virchow's Archiv, Bd. XVI, 1859, S. 414.

[19] SCHWANN—in *Müller's Handb. der Phys.*, 1837, Bd. II, S. 59.

[20] C. B. RADCLIFFE—*Dynamics of Nerve and Muscle*, London, 1871.

[21] MATTEUCCI—*Lectures on the Physical Phenomena of Living Beings*, (translated by J. Pereira,) London, 1847, p. 333.

[22] ARTHUR GAMGEE—*A Text Book of the Phys. Chemistry of the Animal Body*, Vol. I, London, 1881, p. 418.

[23] L. HERMANN—*Grundriss der Phys. des Menschen*, 5te Aufl., 1874, S. 231.

[24] DU BOIS-REYMOND—*Ges. Abh.*, Bd. II, S. 320.

[25] FOSTER—*op. cit.*, p. 79 *et seq.*

[26] C. A. HAUSEN—*Novi profectus in historia electricitatis*, Leipsic, 1743. I cite from DU BOIS-REYMOND—*Unters. über thierische Elektricität*, Bd. II, Berlin, 1849, Th. 1, S. 211.

[27] DU BOIS-REYMOND—*Ges. Abh.*, Bd. II, S. 250.

[28] BERNSTEIN—*op. cit.*, *supra.*

[29] HERMANN—*loc. cit.*, note [14], *supra;* also *Handb. der Phys.*, Bd. II, Th. 1, Leipsic, 1879, S. 144 *et seq.*

[30] See especially DU BOIS-REYMOND—*Unters.*, Bd. II, Th. 1, S. 289, and PFLÜGER—*Unters. über die Physiologie des Electrotonus*, Berlin, 1859: An excellent summary of the observations (with the literature) is given by HERMANN—*Handb. der Physiologie*, Bd. II, Th. 1, S. 157 *et seq.*

[31] RADCLIFFE—p. 74 *et seq.*, *op. cit.*, *supra.*

[32] A. VON HALLER—*Elementa Physiologiæ*, Lib. X, Sect. VIII, § 15, T. IV, Lausanne, 1762, p. 380. He cites as authority the essay of LE CAT, crowned by the Berlin Academy in 1753. [We have in the S. G. O. Library the Berlin edition of 1765, *Traité de l'existence, etc., du fluide des nerfs, etc.*]

[33] BARKER—p. 8, *op. cit., supra.*

[34] CLAUDE BERNARD—*Leçons sur la Phys. et la Path. du système nerveux*, Paris, 1858, T. I, p. 157 and p. 224.

[35] Translation of a lecture given by E. Du Bois-Reymond at the Royal Institution, London, in Appendix No. 1 of H. BENCE JONES' *Croonian Lectures on Matter and Force*, London, 1868, p. 130.

[36] HERBERT SPENCER—*The Principles of Psychology*, Vol. I, New York, 1871, p. 95. Compare also his *Principles of Biology*, Vol. II, New York, 1867, p. 346 *et seq.*

[37] FOSTER—p. 79, *op. cit., supra.*

[38] A. GAMGEE—p. 447, *op. cit., supra.*

[39] O. HAHN—*Die Meteorite und ihre Organismen*, Tubingen, 1881. I cite the Jour. of the Royal Mic. Society, October, 1881, p. 723.

[40] "Lapides crescunt, Vegetabilia crescunt et vivunt, Animalia crescunt, vivunt et sentiunt." This phrase occcurs in the first edition of the *Systema Naturæ*, Leyden, 1735. I cite the reprint of FÉE, Paris, 1830, p. 3, as well as the second Stockholm edition, 1740, p. 76. The expression is replaced in the later editions by more guarded language.

[41] HERBERT SPENCER—*The Principles of Biology*, Vol. I, New York, 1866, p. 107.

[42] HERBERT SPENCER—*First Principles*, Amer. Ed., New York, 1864, p. 274.

[43] HERBERT SPENCER—*op. cit.*, p. 65 *et seq.*

[44] As cited by Mr. HERBERT SPENCER, *loc. cit.*, last note.

[45] GAMGEE—p. 425 *et seq., op. cit., supra.*

[46] HENRY BENCE JONES—*On the variations in the alkaline and earthy phosphates in disease*, Phil. Trans. for 1846, p. 449.

[47] MOSLER—*Beitraege zur Kentniss der Urinabsonderung*, etc., Inaug. Diss., cited in Canstatt's Jahresbericht, 1853, Bd. I, S. 134.

[48] H. BYASSON—*Essai sur la relation qui existe à l'état physiologique entre l'activité cérébrale et la composition des urines*, Paris, 1868.

[40] W. ZUELZER—*Ueber das Verhältniss der Phosphorsaüre zum Stickstoff im Urin*, Virchow's Archiv, Bd. 66, 1876, S. 223.

[50] STRUEBLING—*Ueber die Phosphorsaüre im Urin*, Archiv. für exp. Path. und Pharm., Bd. VI, 1876–7, S. 266.

[51] J. S. LOMBARD—*Experiments on the relation of heat to mental work*, The New York Medical Journal, Vol. V, 1867, p. 199.

[52] J. S. LOMBARD—*Experimental researches on the temperature of the head*, Proc. of the Royal Society of London, Vol. 27, 1878, p. 166; IDEM—*The regional temperature of the head*, London, 1879; IDEM—*Experimental researches on the temperature of the head*, London, 1881. MORITZ SCHIFF—*Recherches sur l'échauffement des nerfs et les centres nerveux à la suite des irritations sensorielles et sensibles*, Archives de Physiol. norm. et path., T. III, 1870, p. 5 *et seq.* BERT—*Communication to the Société de Biologie*, read Jan. 18, 1879, in Gazette Hebdomadaire, Jan. 24, 1879, p. 63. BROCA—*Communication to the French Association for the Advancement of the Sciences*, at the Havre meeting of 1877, in Gaz. Hebd., Sept. 7, 1877, p. 577; also Gaz. Méd. de Paris, 1877, p. 457; IDEM in London Med. Record, Jan. 15, 1880. L. C. GRAY—*Cerebral Thermometry*, The New York Med. Jour., Vol. 28, 1878, p. 31; also Chicago Jour. of Nervous and Mental Diseases, Vol. VI, 1879, p. 65.

[53] See, besides the papers cited in the last note, C. K. MILLS in The New York Med. Record, Vol. 14, 1878, p. 477, and Vol. 16, 1879, p. 130; MARAGLIANO and SEPPELLI—*Studies on cerebral thermometry in the insane*, translated by J. Workman, The Alienist and Neurologist, St. Louis, Jan., 1880, p. 44 *et seq.*; R. W. AMIDON—*The effect of willed muscular movements on the temperature of the head*, Archives of Medicine, April, 1880, p. 117.

[54] FRANÇOIS FRANK—*Communication to the Société de Biologie*, May 29, 1880, in Gaz. Hebd., June 11, 1880, p. 392.

[55] ANGELO MOSSO—*Sopra un nuovo metodo per scrivere i movimenti dei vasi sanguini nell'uomo*, Atti della Reale Accademia della Scienza di Torino, T. XI, Nov. 14, 1875. I have not obtained access to the original, but find an abstract in the Archives de Phys. norm. et path., 1876, p. 175. See also BARKER, p. 12, *op. cit., supra.*

[56] BASCH—*Die volumetrische Bestimmung des Blutdrucks am Menschen*, Stricker's Med. Jahrb., 1876, S. 431. See also ROLLET in HERMANN'S *Handb. der Phys.*, Bd. IV, Th. 1, Leipsic, 1880, S. 306.

[57] MOSSO—*Die Diagnostic des Pulses in Berzug auf die localen Veränderungen desselben*, Leipsic, 1879; also by the same, *Sulla circolazione del sangue nel cervello dell'uomo*, Rome, 1880.

[58] THANHOFFER—*Der Einfluss der Gehirnthätigkeit auf den Puls*, Pflüger's Archiv., Bd. XIX, 1879, S. 254.

[59] EUGÈNE GLEY—*Essai critique sur les conditions physiologiques de la pensée.*

État du pouls carotidien pendant le travail intellectuel, Archives de Phys. norm. et path., Sept.-Oct., 1881, p. 741.

[60] BARKER—p. 11, *op. cit.*, *supra.*

[61] HIRSCH—*Détermination télégraphique de la difference de longitude entre les observatoires de Genève et de Neuchatel*, Genève et Bale, 1864. DONDERS—in Reichert and Du Bois-Reymond's Archiv., 1868, p. 657.

[62] T. H. HUXLEY—*The connection of the Biological Sciences with Medicine*, The Popular Science Monthly, October, 1881, p. 800.

At the conclusion of the reading the thanks of the Society were voted to the President for his able and instructive address.

208TH MEETING. (11TH ANNUAL MEETING,) DECEMBER 17, 1881.

The President in the chair.

Forty-four members present.

The minutes of the last annual meeting were read and adopted.

The Secretary, Mr. THEODORE GILL, read the list of members who had been elected since the last annual meeting.

The Treasurer read to the Society his report upon the receipts, expenditures, and remaining funds of the Society for the year now about to close. He also read the list of members whose dues had been paid.

The Chair then reported to the Society a resolution of the General Committee, which is as follows:

Resolved, That the President be requested to ask the Society to appoint a committee to audit the Treasurer's report, and to communicate the result of their audit to the Society at its next meeting.

In accordance with this request, and also with that of the Treasurer, it was moved and carried that the Chair appoint a committee of three for the purpose named in the resolution.

The Chair appointed a Committee of Audit, consisting of Messrs. John Jay Knox, G. K. Gilbert, and Robert Fletcher.

Mr. THORNTON A. JENKINS then offered the following resolution:

Resolved, That all persons who have resigned membership in the Society, or failed in their duties as provided for in the rules of the

Society, shall be dropped from the succeeding published list of members.

By a vote of the Society this resolution was referred to the General Committee.

The Society then proceeded to ballot for officers for the ensuing year, and the following officers were elected:

President,	WILLIAM B. TAYLOR.	
Vice-Presidents,	J. E. HILGARD.	J. C. WELLING.
	J. J. WOODWARD.	J. K. BARNES.
Treasurer,	CLEVELAND ABBE.	
Secretaries,	THEODORE N. GILL.	MARCUS BAKER.

MEMBERS OF THE GENERAL COMMITTEE.

J. S. BILLINGS.	GARRICK MALLERY.
C. E. DUTTON.	SIMON NEWCOMB.
J. R. EASTMAN.	J. W. POWELL.
E. B. ELLIOTT.	C. A. SCHOTT.
WILLIAM HARKNESS.	

The rough minutes of the meeting were then read and approved, and the Society adjourned.

209TH MEETING. JANUARY 14, 1882.

The President, WM. B. TAYLOR, in the chair.

Upon taking the chair President-elect TAYLOR offered a few remarks, and thanked the Society for the honor conferred upon him.

The minutes of the 207th meeting—the 208th being the annual meeting—were then read and approved.

A communication by Mr. BENJ. ALVORD was read, entitled

CURIOUS FALLACY AS TO THE THEORY OF GRAVITATION.

Some years since I noticed in a text book on astronomy, used in one of the most celebrated colleges in the United States, a pretended demonstration that the attraction of gravitation *must* vary inversely as the square of the distances. It was continued in several editions down to about 1850, when that portion was omitted. I always supposed that the author copied it from some old authority; that he was not guilty of inventing it, abused as it was.

In "Hind's Dictionary of Arts and Sciences" (one volume, folio, London, 1769, copy in the Congressional Library) it is found under the article "Attraction."

The first named author announced that "Gravity at different distances from the east *must* vary inversely as the square of the distances." He proceeded substantially as follows:

"The total amount of attraction exerted by the earth upon bodies exterior to it is the same as though that force was all concentrated in the centre. But a force or influence which proceeds in right lines from a point in every direction is diminished as the square of the distance is increased. For, let the centre of the earth be the vertex of a pyramid, cut said pyramid by two parallel bases at different distances from the vertex, making two similar pyramids. *Whatever the nature of gravity, its influence at the distance of each base must be equally diffused over the base. Therefore its intensity or force will be as much less at the greater base, as contrasted with its influence at the nearer and lesser base, as the surface of the latter is to the surface of the former.* But the surfaces of these bases are to each other as the squares of their distances from the vertex. Therefore the force of gravity varies inversely as the square of the distances.—Q. E. D."

Actually he placed Q. E. D. to it as if it was a mathematica, demonstration!

He afterwards said:

"The intensity of light at different distances from the radiant varies inversely as the square of the distances. This proposition is proved in the same manner as that respecting gravity, the reasoning in which applies to *all* emanations from a centre."

Subsequently, when he got to refer to the laws of Kepler, he said:

"They, therefore, became known as *facts* before they were demonstrated mathematically. The glory of this achievement was reserved for Newton, who proved that they were necessary results of the law of universal gravitation."

This sentence would have astonished Newton! It places the cart before the horse. From the empirical laws of Kepler the theory of gravitation was mathematically derived by Newton. Not the reverse. What a confusion of ideas that Kepler's laws could both be demonstrated mathematically and observed as facts? How it belittles the labors of Newton, who should have made his discovery (*de novo* from his own breast) by a geometrical process and not from the observed facts!

But my principal object in referring to this curious fallacy was to give an attempt of my own to show its fallacy by a "*reductio ad absurdum.*"

I can prove by an entirely similar process, with equal plausibility, that the force of gravity must vary inversely *as the cubes of the distances.* Instead of a pyramid take a cone. Let the centre of the earth be the vertex of a cone. Place two spheres or molecules of different sizes,* tangent to the cone, at different distances from the vertex. *Whatever the nature of gravity, its influence at the distance of each sphere must be equally diffused throughout the solid contents or volume of each sphere. Therefore its intensity or force will be as much less at the greater sphere, as contrasted with its influence at the nearer and smaller sphere, as the volume of the latter is to the volume of the former.* But these volumes or solid contents vary as the cubes of their radii, or as the cubes of their distances from the vertex. Therefore the force of gravity varies inversely as the cubes of the distances.

The oracular "Q. E. D." could have been placed to this fallacy with full as much propriety as in the former case, for I have used nearly identical words. Of course they are both pure assumptions. Neither are mathematically true, and the one destroys the other, as they are contradictory. But the first is true as arrived at by severe induction from the observed facts.

If I was a professor of logic, I should give these as specious examples of the danger of false premises, and of the ease with which they could be manufactured.

Indeed, the authors first named would imply that there could in the science of mechanics be no central forces, no empirical laws. Indeed, they would reduce the whole planetary system, the whole cosmos, to a geometrical necessity; and they would lose that interesting exposition in physical astronomy as to the wisdom and beneficence exhibited in the planetary system as it exists.

In the well-known discussion of central forces by Poisson, the equation of the curve when referred to co-ordinate axes is ascer-

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*The word molecules, being now a favorite word with the physicists, might suit the casuist a little better.

tained, and the change of one constant in the equation causes a change in the nature of the curve. If the law varied *directly as the distance*, the orbits of the planets would be ellipses as now, (but the sun would be at the centre, and not at one foci,) and they would all revolve in the same period about the sun, and on the surface of any planet no attraction towards its centre would exist. This curious result would follow: that any object projected into the air would immediately be carried from the earth, and would perpetually revolve as a satellite, like the moon, around it. All terrestrial objects would be unsettled and float about in the air in the utmost disorder.

If, on the contrary, the law varied inversely *as the cube of the distance*, (according to that precious second fallacy above set forth,) each planet would describe a spiral orbit, (if at first projected towards the sun,) continually winding and winding towards the sun; or, if perchance projected at first *from* it, would move in a spiral curve, causing it to recede farther and farther from the sun; and the eye of Omniscience alone could trace its final wanderings. What a contrast, all these suppositions, to the order, stability, beauty, and beneficence of our planetary system as it exists!

The next communication was by Mr. M. H. DOOLITTLE

ON THE GEOMETRICAL PROBLEM TO DETERMINE A CIRCLE EQUALLY DISTANT FROM FOUR POINTS.

"Describe a circumference equally distant from four given points; the distance from a point to the circumference being measured on a radius or radius produced. In general there are four solutions." (Chauvenet's Geometry, problem 110.)

These four solutions were undoubtedly obtained in accordance with the conception of three given points all either inside or outside of the required circumference. Three other solutions may be obtained from the conception of two given points inside and two outside. Mr. Marcus Baker has suggested that a distance may properly be measured from a given point through the centre of the circle to the opposite side of the circumference. This interpretation increases the number of solutions to fourteen.

This communication gave rise to a brief discussion, participated in by Messrs. HARKNESS, NEWCOMB, and BAKER, the latter pointing out that the problem appears among the exercises of Rouché and Comberousse's Traité de géométrie élémentaire, (2d ed., p. 113, Ex. 124,) a source from which Prof. Chauvenet drew many of his exercises. In Chauvenet's Geometry this problem appears as Exercise 110, page 308, with the statement that there are in general *four* solutions. This statement does not occur in the French work cited, and, therefore, the error appears to be due to Chauvenet himself, a thing somewhat noteworthy, as Chauvenet's works are in general very accurate.

Mr. Alvord then remarked

ON SOME OF THE PROPERTIES OF STEINER'S "POWER-CIRCLE."

After the consideration of this communication the report of the Auditing Committee, appointed at the 208th meeting, was called for, and, in the absence of the chairman, Mr. Knox, was presented by Mr. Fletcher. The following is the report:

Washington, *January* 13, 1882.

Mr. President and Gentlemen
of the Philosophical Society of Washington:

We, your committee, appointed at the annual meeting, December 17th, 1881, to audit the report of the Treasurer for the years 1880 and 1881, have the honor to submit the following report:

We have examined the statement of receipts of dues from members and of interest on bonds, and find the former to be $1,175 and the latter $125, as appears in the Treasurer's statements of accounts for the years 1880 and 1881.

We have examined the vouchers for disbursements for the same period, and find them correct.

We have compared the return checks with the vouchers and with the entries in the bank book, and find them correct.

We have examined the bank book, and found the balance as set forth to be correct, said balance, deducting the amount of two checks not yet returned, being $320.16, with Messrs. Riggs & Co.

The bonds referred to in the statements of assets were exhibited to us by the Treasurer, and consist of $1,000 U. S. 4½s and $500 4 per cent. bonds.

All of which is respectfully submitted.

Jno. Jay Knox.
Robert Fletcher.
G. K. Gilbert.

The report was adopted, and the committee discharged.

The President, Mr. Taylor, then offered a brief communication

ON THE TOTAL LUNAR ECLIPSE OF JUNE 11, 1881.

This was noteworthy for the bright illumination of the moon's disk, which occurred during totality. The features of the moon's surface could be seen almost as distinctly during total eclipse as during full moon. This phenomenon was attributed to the refraction caused by the earth's atmosphere. To an observer stationed upon the moon a bright circle of sunlight would be visible surrounding the earth, and to the light from this source was attributed the illumination of the moon's disk seen during total lunar eclipses.

This communication was discussed by Mr. Harkness.

Mr. Dall then presented a brief communication

ON SOME PECULIAR FEATURES OF MOLLUSKS FOUND AT GREAT DEPTHS.

While considerable difficulty was experienced in separating some of the forms by their shells alone, yet, when their anatomy was examined, some very striking differences were presented. Among the dredgings off the Atlantic coast and in the Gulf of Mexico by the *Blake* were found mollusks claimed to be representatives of two new families having a dentition simulating that of the Docoglossa. One related to the Fissurellidæ and the other referable to the order Rhipidoglossa.

This communication was discussed by Messrs. Gill and Alvord, after which the Society adjourned.

210th Meeting. January 28, 1882.

President Wm. B. Taylor in the chair.

Thirty-nine members and visitors present.

Mr. Ferrel presented to the Society a communication entitled

ON THE CONDITIONS DETERMINING TEMPERATURE,

but, from lack of time, did not complete its presentation, and asked for a continuance at some future meeting.

Mr. L. F. WARD then read a paper entitled

ON THE ORGANIC COMPOUNDS IN THEIR RELATIONS TO LIFE.

This paper was briefly discussed by Messrs. ANTISELL and ELLIOTT, after which the Society adjourned.

211TH MEETING. FEBRUARY 11, 1882.

President WM. B. TAYLOR in the chair.

Mr. GILBERT presented to the Society a communication

ON ERRORS OF BAROMETRIC OBSERVATIONS PRODUCED BY WIND.

This communication will be published in full in the Report of the Geological Survey.

This communication was discussed by Messrs. BAKER, MASON, and ANTISELL, after which the Society adjourned.

212TH MEETING. FEBRUARY 25, 1882.

President WM. B. TAYLOR in the chair.

Thirty members and visitors present.

Mr. FERREL presented to the Society the concluding portion of a communication offered to the Society at its 210th meeting, January 28th,

ON THE CONDITIONS DETERMINING TEMPERATURE.

The usual formula for the rate of cooling of a heated body in vacuo, first given by Pouillet as determined from the experiments of Dulong and Petit, is of the form:

$$\delta h = Bf(\mu^{\tau} - \mu^{\tau'})$$

In which

B = the units of heat radiated by a unit of lamp-black surface in a unit of time;

f = the radiating power of the body, lamp-black being unity;

τ = the temperature of the cooling body;

τ' = the temperature of the enclosure;

μ = a constant, of which the value is 1.0077;

δh = the heat lost in a unit of time for each unit of surface.

The first part of the second member, $Bf\mu^{\tau}$, expresses the amount of heat radiated by the body, and the second, $Bf\mu^{\tau'}$, the amount of heat received from the enclosure; the radiating and absorbing powers being usually assumed to be the same, f is common to both.

In applying this formula to bodies in space, protected from the rays of the sun, τ' would represent the temperature of space, by which is meant the temperature at which a body would stand by the heat received from the stars. In applying it to bodies on the earth's surface it may be regarded as the temperature of an imaginary enclosure, from which as much heat would be received as from all surrounding objects, the earth's surface, and the atmosphere, &c., not including the sun, and hence it represents the shade temperature.

If we now suppose the body to be exposed to the direct rays of the sun, the amount of heat thus received must be added to that received from space, or from terrestrial surroundings, that is, to $Bf\mu^{\tau'}$, and the preceding formula then becomes

$$\delta h = -K\rho f + Bf(\mu^{\tau} - \mu^{\tau'}) \tag{1}$$

In which

K = the units of heat received from the sun on a unit of surface;

ρ = the ratio between the surface receiving rays, projected on a plane perpendicular to the rays, and the whole radiating surface.

As the body receives the rays from one direction and upon one side only, and radiates from all sides, the average amount of heat, $K\rho f$, received over the whole surface and absorbed, must be compared with the amount lost by radiation, and hence the factor f must come in, since only the heat absorbed affects temperature, the absorbing and radiating power here, as usual, being assumed to be the same.

In the case of a spherical body, as the bulb of a thermometer, the value of ρ becomes $\frac{1}{4}$, since the projected receiving surface of the sphere is one-fourth of the whole radiating surface of the sphere. In the case of a long cylinder, in which the radiation from the ends could be neglected in comparison with the whole, the value of ρ becomes $\frac{1}{\pi}$, if the side of the cylinder is exposed perpendicularly to the sun's rays. In the case of a thin disk, with its surface perpendicular to the sun's rays, neglecting the radiation from the edge, the value of ρ would be $\frac{1}{2}$. In the case of such a disk, in which the radiation is from one side only, which would be approximately so in the case of such a disk with the opposite side of polished silver, the value of ρ would be unity.

The amount of heat, K, received from the sun through the atmosphere at the earth's surface is usually expressed by

$$K = Ap^{\varepsilon} \tag{2}$$

In which

A = the heat received from the sun on a unit of surface at the top of the atmosphere;

ε = the secant of the zenith distance of the sun;

p = a constant for all zenith distances, but differing in different states of the atmosphere, but always less than unity.

In the case of a static equilibrium of temperature, which was the only case considered, δh vanishes, and the preceding equations, (1) and (2), give

$$\rho A p^{\varepsilon} = B(\mu^{\tau} - \mu^{\tau'}) \tag{3}$$

This equation expresses the condition which determines the static temperature, τ, of a body, and it is seen that this depends upon the solar constant A; the form of the body, upon which the value of τ depends; upon the value of p, or the state of the atmosphere; upon the zenith distance, which determines ε; upon the radiating constant, B; and upon the shade temperature, τ'.

Putting for the unit of heat the amount required to raise the temperature of a cubic centimetre or gram of water one degree centigrade, and the square centimetre, second, and degree centigrade, for the units of surface, time, and temperature, respeotively, the value of B was determined by the author, from the experiments of Mr. J. P. Nichol on the rate of cooling of a blackened copper ball in vacuum, surrounded by an enclosure of blackened surface, (Proc. Royal Soc. Edin., 1869–70, p. 207,) to be .01808. This value was considered more reliable than that of Pouillet from the experiments of Dulong and Petit, since the latter were made on the rate of cooling of mercury in a glass bulb, and the results had to be reduced to those which would have been obtained with a blackened surface; and the value of the radiating power, f, for glass, which was used in this reduction, Pouillet states, was somewhat hypothetical, and so it left some doubt with regard to the true value of the constant. Pouillet's value of B for the minute-unit was 1.146, and this reduced to the second-unit is .01910. The value $\mu = 1.0077$ required no change to satisfy the results of Mr. Nichol's experiments.

The value of A, deduced from the experiments of Pouillet and Herschel with the actinometer, is .03046 for the mean distance of the sun, both sets of experiments, when reduced to the sun's mean distance, giving very nearly the same value. At the time of the earth's perihelion this is about one-thirtieth greater, and at aphelion as much less.

Pouillet's value of p for clear weather is about 0.75, but others make it considerably less. It can hardly be regarded as a constant, but only as a sort of average of values for clear weather, which may differ very much at different times. According to Tyndal, who maintains that the absorption power of the atmosphere in clear weather depends almost entirely upon the amount of aqueous vapor in it, the value of this constant, even in clear weather, must depend very much upon the hygrometric state of the atmosphere.

With the preceding numerical values of the constants of A and B, the preceding equation gives

$$(4) \qquad \mu^{\tau - \tau'} = \frac{1.685\, \rho p^{\varepsilon}}{\mu^{\tau'}} + 1$$

for determining the value of $\tau - \tau'$, for any zenith distance of the sun, of which the secant is ε, where the value of p and the shade temperature τ' are known. But since the value of B was determined for a vacuum, this formula is only applicable where the radiating body is in a vacuum, and cannot be applied in cases where the body receives or loses heat by conduction or convection.

The first term of the second number of the preceding equation depends upon K, the heat received from the sun, and, therefore, vanishes where the body is in the shade, and we then have $\tau - \tau' = 0$. Hence the temperature of all bodies having the same surroundings must cool down to the same temperature, τ'. This is a necessary consequence of the equality of the absorbing and radiating powers of bodies.

The author had been able to find but few observations of the value of $\tau - \tau'$ to compare with the theoretical value given by the preceding formula. Hooker states that from a multitude of desultory observations made on the Himalaya Mountains at an elevation of 7,400 feet, he concluded that the average effect of the sun's rays on a black-bulb thermometer was 125.7° or 67° (37.2° C.) above the temperature of the air. The shade temperature was, therefore, 14.8° C. With this value of τ', and the value $\rho = \frac{1}{4}$ for the spherical bulb, we get $\tau - \tau' = 41.6°$ at the top of the atmosphere where $p = 1$. The value of p for that altitude, and also the value of ε for the observations, are not accurately known. At the elevation of 7,400 feet, Pouillet's value of $p = .75$ would have to be considerably increased, but the effect of the exponent ε would perhaps bring the value of p^ε equal to about .75. With this value of p^ε the formula gives $\tau - \tau' = 32.4°$, five degrees too small for the observed value.

Again, at the height of 13,100 feet, he found in January, at 9 a. m., the temperature of the black bulb 98° with a difference of 68.2°, and at 10 a. m., 114° with a difference of 81.4°. From the average of these we get $\tau' = -0.4°$ C. and $\tau - \tau' = 41.6°$ C. The preceding formula gives $\tau - \tau' = 45.7°$ C. at the top of the atmosphere where $p = 1$. At the elevation of 13,100 feet the value of p^ε should not be very much less than unity—perhaps about as much less as would reduce the theoretical value 45.7° down to the observed value 41.6°.

It should be remarked here that the theory requires that the two thermometers should have exactly the same surroundings. If the one thermometer is in a vacuum surrounded by a glass bulb and the other outside, this condition is not perfectly fulfilled, and the indication of the thermometer outside in the shade might vary a little from one in the shade within the bulb, unless this bulb is so situated as to have the same temperature as the external shade thermometer.

If, in place of a black-bulb thermometer, we had a thin disk with a blackened side exposed perpendicularly to the sun's rays, and the opposite side of polished silver of which the radiating power is extremely small, we should have in this case the value of $\rho = 1$ very nearly, and with this value of ρ the formula would give, in the first of the examples above, for the top of the atmosphere, $\tau - \tau' = 106.6°$ C., which, added to the shade temperature, 14.8°, would give $\tau = 121.4°$ C. This enormously high temperature is not inconsistent with observation, for water has been made to boil from the effect of the direct rays of the sun at the earth's surface, where the theoretical condition of our formula, that no heat shall be lost by conduction, was not perfectly fulfilled.

A portion of the earth's surface, where the soil is dry and sandy, having little conductivity for heat and exposed to the vertical rays of the sun, would be a case similar to that of an isolated disk radiating sensibly from one side only, and the temperature of such a surface, so exposed, should stand at a very high temperature, but of course not nearly up to the theoretical temperature, since much heat would be conveyed away by the conduction and convection of the air, and also some conducted down into the earth. The temperature of sandy soils is often observed to be as high as 150° F. and upwards, and the preceding theory explains these very high temperatures and the great differences of temperature of different bodies under the same circumstances.

From equations (2) and (3), with the given values of A and B, we get

$$(5) \qquad K = .07232\, \mu^{\tau'}(\mu^{\tau - \tau'} - 1)$$

This is an actinometric formula, giving the amount of heat received from the sun, in absolute heat units, from the observation of the sunshine and shade temperatures. So far as the author's reading extends no such formula has ever been given, but $\tau - \tau'$ has been regarded as a measure of the sun's relative intensity under different circumstances. The formula not only gives the absolute instead of the relative amount of heat received, but it shows that $\tau - \tau'$ is not proportional to K, and consequently not a correct measure of the relative intensities of the sun's rays. With an observed value $\tau - \tau' = 35°$ and $\tau' = 30°$ the formula gives $K =$ .02806; but with the same value of $\tau - \tau'$, and with the value of $\tau' = 0°$, it gives $K = .02229$. Hence the value of K is not proportional to $\tau - \tau'$, and differs considerably when the value of $\tau - \tau'$, under different circumstances, is the same. Both these values of K are less than the value of $A = .03046$, as they should be by equation (2). The greater the altitude the more nearly should the value of p approximate to that of unity, and the more nearly should the value of K approximate to that of A.

If the value of p, according to Tyndal, as has been stated, depends upon the hygrometric state of the atmosphere, then the value of K, as given by the preceding formula, for any observed values of τ and τ', must give the diathermancy, and consequently the hygrometric state of the atmosphere in clear weather, not only for the point of observation, but generally throughout the whole extent of the atmosphere through which the rays pass, for the greater the value of K the greater the diathermanancy of the air, and hence the less the amount of aqueous vapor in it.

This was briefly discussed by Messrs. HARKNESS, H. FARQUHAR, and TAYLOR.

Mr. ANTISELL then began the presentation of a communication

ON THE BUILDING UP OF ORGANIC MATTER,

which was unfinished when the hour of adjournment arrived, and its completion went over to the next meeting.

213TH MEETING. MARCH 11, 1882.

President WM. B. TAYLOR in the chair.

Thirty-seven members and visitors present.

Mr. ANTISELL then presented to the Society the remainder of his communication

ON THE BUILDING UP OF ORGANIC MATTER,

the presentation of which was begun at the last meeting.

A brief discussion of this paper—the session having been prolonged for this purpose—followed, and was taken part in by Messrs. GILL and WARD, who took exceptions to some of the conclusions arrived at in the communication.

214TH MEETING. MARCH 25, 1882.

President WM. B. TAYLOR in the chair.

Thirty-six members and visitors present.

The President announced to the Society the death, at 3 p. m. this day, of pneumonia, after an illness of two days, of Mrs. Joseph Henry, widow of the first president of the Society.

Mr. A. B. Johnson then presented to the Society a communication

ON SOME PECULIAR RAVAGES OF TEREDO NAVALIS.

This communication was discussed by Messrs. Antisell, Dall, Gill, Harkness, and White.

Mr. Antisell called attention to the fact that the existence of the Teredo, as well as that of other destructive mollusks brought to our harbors by shipping, along our entire coast is well known, and that, in view of this fact, it is a matter of surprise that provision was not made for guarding against this danger. To this it was answered by Mr. Johnson that the wharf was a temporary one, being only needed for three months, and that, although the presence and destructive powers of the Teredo were recognized by the Board, it did not appear that in any previous case the destructive action of the Teredo was so rapid as to render special precaution necessary in this case. Upon a question from Mr. Harkness it was asserted by Mr. Johnson that a pile, examined on September 15 by divers, and found sound—chips cut by divers from the pile under water were found unbored by the Teredo—broke down on September 19, thus indicating a destruction of a pile in four days.

The accuracy of the observation of September 15, that the chips were unbored, was questioned by Mr. Dall, who asserted that the Teredo in its youngest stage attacks the wood, and that the hole made is at first very minute, and is gradually enlarged and deepened as the mollusk grows. So that a pile which appears sound on the surface may, in fact, already be seriously injured by Teredo borings. In San Francisco Bay the work of destruction of piles by the Teredo, and their renewal goes on continually, and it is estimated that a complete renewal of all the piles in the bay occurs every seven years. The mollusk works and breeds the year round in waters above a temperature of 60° F. It attacks the hard woods, as lignum vitæ, quite as readily as softer woods, but the destruction in such case is less rapid. Such woods, however, as palmetto, consisting of bundles of tough fibres interspersed with soft or spongy material, are only slightly, if at all, injured.

Mr. Gill called attention to the fact that the Dutch Commissioners, appointed in consequence of the great ravages of the Teredo on the coast of Holland in about 1859, found creosote the best preventive. They further found that the activity of the Teredo was, to a certain extent, dependent upon meteorological conditions since the years 1720, 1755, 1782, 1820, and 1850, were seasons of great drought, and consequent increase of salinity of the sea-water along the coast, and in those years the destruction caused by the Teredo was unusually great.

Respecting the geological age of the Teredo, Mr. WHITE exhibited to the Society fossilized wood from the cretaceous formation showing Teredo borings.

Mr. BILLINGS then presented to the Society a communication

ON THE VENTILATION OF THE HOUSE OF REPRESENTATIVES,

which was unfinished when the hour of adjournment arrived, and went over to the next meeting.

Adjourned.

215TH MEETING. APRIL 8, 1882.

President WM. B. TAYLOR in the Chair.

Forty-eight members and visitors present.

Mr. BILLINGS then continued the presentation of the communication begun at the last meeting

ON THE VENTILATION OF THE HOUSE OF REPRESENTATIVES,

of which the following is an abstract:

The difficulties to be overcome, and the means used for this purpose were explained, and plans and sections of the Hall of the House of Representatives at the Capitol, in Washington, were shown. The amount of fresh air required is about one foot per second per person, if an approach to perfect ventilation is desired. The imperfect form of ventilation by dilution requires from forty to fifty feet per minute. When a hall is occupied only one or two hours, the cubic space is important, but in long sessions it is the supply rather than the space that must be looked to.

To produce the requisite movement of the large amount of air used, special force must be supplied. This may be propulsion—the plenum method, or by aspiration—the vacuum method, or a combination of the two. The effect of wind and rain on aspirating systems was alluded to. In the majority of such halls the plenum system, by means of a fan, is used. The difficulty in introducing this large amount of air into a hall depends partly on the necessity for avoiding unpleasant currents, and partly on the cost of heating and supplying power. The question of cost, however, in such halls as are referred to, is usually a minor consideration, but if the tastes of individuals as to temperature are to be consulted—that is, if each man is to have his air at the temperature which suits himself—the cost becomes a serious matter.

The effects of various positions of fresh air inlets were pointed out, and stated to depend largely upon the tendency of air to adhere to surfaces over which it passes, as shown by the investigations of Savart and others. The difference between the upward and downward system were pointed out.

The various modes of heating were described, more especially with reference to their effect upon the air, and the influence of moisture was discussed. Probably the importance of moistening the air is less than has been supposed, and the methods employed for this purpose have been beneficial only indirectly.

The system of heating and ventilation of the Hall of the House was then described, and compared with that of the English Houses of Parliament, the Chamber of Deputies at Versailles, and the Grand Opera House at Vienna, and Frankfort on Main.

The great importance of skilled superintendence was pointed out, and the necessity for continuous records was insisted on.

Remarks upon this communication were made by Messrs. ANTISELL, ELLIOTT, MUSSEY, and POWELL.

Mr. HILGARD then presented a communication

ON SIEMENS' DEEP SEA THERMOMETER AND CARRÉ'S ICE MACHINE.

Remarks on this communication were made by Messrs. ANTISELL, DALL, DUTTON, and E. J. FARQUHAR, after which the Society adjourned.

216TH MEETING. APRIL 22, 1882.

President WM. B. TAYLOR in the chair.

Thirty-six members and visitors present.

The Secretary read a list of names of persons who had been elected to, and had accepted membership in, the Philosophical Society, viz: Ezra Westcott Clark, Henry Flagg French, Henry Allen Hazen, Charles Hugo Kummel, Israel Cook Russell, William Wirt Upton, Albert Lowry Webster.

Mr. Ferrel then presented to the Society a communication

ON SOLAR RADIATION AT SHERMAN, WYOMING.

The next communication was by Mr. C. A. White

ON ARTESIAN WELLS ON THE GREAT PLAINS.

This communication has been essentially reproduced with the title, "Artesian Wells upon the Great Plains," (subscribed C. A. White,) in the American Review for August, 1882, No. 135, pp. 187–196.

Mr. Antisell called attention to previous attempts on the part of the Government to obtain water on the great plains by boring artesian wells. During the surveys and explorations of the 39th parallel, for the purpose of ascertaining the feasibility of building a railroad to the Pacific Ocean, special attention was given to the matter of obtaining water by means of artesian wells, and at that time he reached the same conclusion essentially as that now presented by Mr. White. Mr Antisell's published report upon this subject may be found in volume 7 of the Pacific Railroad Reports published in 1854.

Mr. Mussey called attention to boring now in progress along the line of the Southern Pacific Railroad in New Mexico; boring being in progress at the expense of the railroad company for the purpose of supplying water for locomotive purposes.

Mr. Gilbert considered the argument conclusive as to the failure of artesian wells on the great plains to be of any practical value for irrigating purposes, but for some other uses, such as stock raising, farm uses, etc. Some wells in favorable localities had proved a success, and others would also undoubtedly prove successful. Geological prophecy is generally, however, to be made with great caution, and to be received with caution equally great, a proposition which was supported by citing several cases in the experience of himself and others.

On the close of this discussion Mr. ELLIOTT presented a communication

ON THE CREDIT OF THE UNITED STATES, PAST, PRESENT AND PROSPECTIVE.

This communication will be published in another form.

Remarks upon this paper were made by Messrs. GILL and W. B. TAYLOR, after which the Society adjourned.

217TH MEETING. MAY 6, 1882.

President WM. B. TAYLOR in the chair.

Twenty-eight members and visitors.

The President announced to the Society the death of two of its members, Mr. WILLIAM J. TWINING, Major U. S. Engineers and Commissioner of the District of Columbia, and Mr. JOHN RODGERS, Senior Rear Admiral U. S. Navy and Superintendent U. S. Naval Observatory. He further announced to the Society that the proposition for a federation of the Anthropological, Biological, and Philosophical Societies had been discussed by the General Committee, but that thus far no action had been taken.

The first communication was by Mr. ELLIOTT COUES,

ON THE POSSIBILITIES OF PROTOPLASM.

The following is an abstract of this communication, which has been published at greater length under the title—"Biogen: a Speculation on the Origin and Nature of Life. Abridged from a paper on the 'Possibilities of Protoplasm,' read before the Philosophical Society of Washington, May 6th, 1882. By Dr. ELLIOTT COUES. Washington: Judd & Detweiler, printers and publishers. 1882." (8vo., pp. 27.)

Referring to previous papers on the subject of Life, by Mr. WOODWARD and Mr. WARD, the speaker opposed any purely chemico-physical theory, and adhered to the doctrine of the actual existence of a "vital principle." Granting that all substances, including protoplasm, have been evolved from nebulous matter; that evolution to the protoplasmic state is necessary for any manifestation of life, and even that life necessarily appears in matter thus elaborated, it does not follow that the result of the processes by which matter is fitted to receive life is the *cause* of the vitality manifested. For all that is known to the contrary protoplasm and vitality are simply concomitant; or if there is any causal relation between them, vital force is the cause of the peculiar properties of protoplasm, not the result of those properties. There really exists a potency or principle called "vital," in virtue of which the chemical substance called protoplasm manifests vitality, that is to say, *is alive*, and in the absence of which no protoplasmic or other molecular aggregation of matter can be alive. The chemico-physical theory simply restates abiogenesis or "spontaneous generation," of which we know nothing scientifically. The grave doubt that "life is a property of protoplasm" will persistently intrude until some one shows what is the chemico-physical difference between living and dead protoplasm; none being known.

Noting that chemistry and physics had combined to manufacture an egg which would do everything to be expected of an egg, except to hatch, the speaker summed his charge thus: The atheistic physicist, denying mind in nature, declares that matter alone exists. Matter in motion is all there is; the cosmos being matter in motion in virtue of material forces alone. This is simply to invent a kind of perpetual motion machine, and leave out even the inventor; for such a machine invented itself and set itself going. Then the materialistic chemist takes this self-started machine and declares it has laid an egg that will hatch. On any such theory a God is not only superfluous but impossible. Yet the result of the alleged self-evolution of self-created matter through chemical elements to organic compounds has been the creation of a protoplasmic soul so constituted that it must believe in a God; and if matter be that God, matter contradicts itself, for the constitution of the human soul requires that its God must be other than its protoplasmic self; while if matter be not that God, there must be some other.

The speaker argued for the existence of the soul as something apart from and unlike matter, defining "soul" as that quantity of spirit which any living body may or does possess. No idea can attach to the term "spirit" from which all conceptions of matter are not absolutely excluded. Spirit is immaterial, self-conscious force; life consists in the animation of matter by spirit.

The substance of mind and the substance of matter were noted as equally hypothetical. To the former was given the name

Biogen, or "soul-stuff," and it was defined as spirit in combination with the minimum of matter necessary to its manifestation. The analogy between biogen and luminiferous æther, or the hypothetical substance of light, was discussed. The drift of the speaker's speculation on the vital principle as an ens realissimum was toward a restatement, in scientific terms, of the old *anima mundi* theory. Modern materialistic and atheistic notions about life were denounced as every one of them disguises of the monstrously absurd statement that a self-created atom of matter could lay an egg that would hatch.

The whole matter being beyond the scrutiny of the physical senses is remote from the scope of exact science; but it is irrational and unscientific to deny it, as is virtually done when science excludes it from any share in life-phenomena, by presuming to explain life upon purely material considerations. No chemico-physical theory of life is tenable that does not satisfactorily explain the chemico-physical difference between, for example, a live amœba and a dead one; an explanation which has never yet been, and probably cannot be, given.

A general discussion of the points involved in this paper followed. Mr. Powell pointed out what he regarded as a fundamental and fatal error in the reasoning, viz., that the axiom that the whole equals the sum of all its parts, had been assumed throughout to be true *qualitatively* as well as quantitively. Furthermore, he maintained that logical consistency required that those who believe in force should believe also in the vital principle, and *vice versa*. As for himself, however, there was neither force nor vital principle, but only matter in motion. Three relations are always to be borne in mind, viz., quantity, quality, and succession, whereas the physicist falls into error by considering only the quantitive relation.

So much of the support of the views of Mr. Coues as might be derived from the common consensus of mankind was criticised by Mr. Gill as unsound, since the common consensus of mankind has often been found at fault; the supposed flatness of the earth, the motion of the sun around the earth, etc., are examples where this criterion fails. Paraphrasing an eminent philosopher's dictum, he thought there was a tendency of biologists ignorant of philosophy and philosophers ignorant of biology to make a distinction between organic and inorganic matter, and call in a "vital force." He likened living and dead protoplasm to an electric battery in action and at rest, and maintained that life is a property of matter, and that it cannot be conceived of separated from matter.

Mr. HARKNESS avowed his belief in force, and hence in vital force, and further in a little religion, and was, therefore, moved to make inquiry concerning the chemical difference between living and dead matter.

Mr. WARD pointed out that very diverse views were held upon this subject by two classes of thinkers who do not come into intellectual contact. Furthermore, while not asserting that a belief in vital force was a superstition, attention was drawn to the fact that infantile races attribute all phenomena to supernatural agencies, and that, with increasing knowledge, there is a decrease in the number of these appeals to supernatural agencies.

The corner stone of modern science, said Mr. DOOLITTLE, is *measure.* We must have a biometer. What electrical science would be without ohms, astronomy without graduated circles, chemistry without the balance, such is biology without a *measure.* Is there more life in two mice than in one mouse? In a horse than in a mouse? Until we can answer these questions substantial progress in biology is not to be expected.

The term automatic, as used here, he considered a confession of biologic ignorance. Automatic motion, as used in the discussion, seemed to mean simply motion which cannot be relegated to any known law.

After some further desultory discussion the Society adjourned.

218TH MEETING. MAY 20, 1882.

President WM. B. TAYLOR in the chair.

Thirty-two members and visitors present.

A series of resolutions concerning the death of Admiral JOHN RODGERS, a member of this Society, which resolutions had been adopted by the General Committee, were read by the Secretary; after which Prof. CHARLES W. SHIELDS, of Princeton College, read to the Society a communication

ON THE PHILOSOPHICAL ORDER OF THE SCIENCES.

This communication has been published by Scribner's Sons in a volume entitled "The Order of the Sciences. An Essay on the Philosophical Classification and Organization of Human Knowledge." By Charles W. Shields, Professor in Princeton College. 103 pp., 12mo. New York, Charles Scribner's Sons, 1882.

This communication was discussed by Messrs. WARD, POWELL, ANTISELL, TAYLOR, ALVORD, and BAKER.

219TH MEETING. JUNE 3, 1882.

President WM. B. TAYLOR in the chair.

Twenty-two members and visitors present.

The first communication offered was by Mr. ALVORD

ON THE COMPASS PLANT.

This communication has been published with the title "On the Compass Plant," by Benjamin Alvord, in the American Naturalist for August, 1882, No. 16, pp. 625–635.

Remarks were made on the exhibition of polarity in other vegetable types by Messrs. HENRY FARQUHAR and THEODORE GILL.

Mr. E. B. ELLIOTT next presented to the Society a communication

ON SOME FORMULÆ RELATING TO GOVERNMENT SECURITIES.

Mr. C. H. KUMMELL then presented a communication

ON COMPOSITION OF ERROR FROM SINGLE CAUSES OF ERROR.

This was unfinished when the hour of adjournment arrived, and its completion went over to the next meeting.

Adjourned.

220TH MEETING. JUNE 17, 1882.

President WM. B. TAYLOR in the chair.

Twenty-three members and visitors present.

Mr. C. H. KUMMELL continued his communication

ON COMPOSITION OF ERROR FROM SINGLE CAUSES OF ERROR.

which was begun at the last meeting.

This paper is expected to appear in full in the Astronomische Nachrichten.

Remarks upon this paper were made by Messrs. E. B. ELLIOTT and W. B. TAYLOR.

Mr. MARCUS BAKER then presented the following communication

ON A GEOMETRICAL QUESTION RELATING TO SPHERES.

On January 17, 1882, Mr. Doolittle called the attention of the Society to the geometrical problem *To determine a circle equally distant from four given points in a plane*, and showed that the statement in Chauvenet's Geometry, (p. 308, Ex. 110,) that this problem admits of *four* solutions is erroneous, there being in general *fourteen* solutions. The extension of this problem to spheres and five points in space is nearly as simple as for the case of circles and four points in a plane.

Let it be proposed to solve the following:

PROBLEM.—*To determine a sphere equally distant from five given points.*

The distance to a sphere, considered here, is to be measured along a diameter, produced if necessary, and hence for any position we have two distances, one a maximum, the other a minimum.

Solution.—Case I. Through any four of five given points, *a*, *b*, *c*, *d*, *e*, as, for example, *b*, *c*, *d*, *e*, describe a sphere; the fifth point, *a*, will in general fall within or without this sphere, of which call the radius R and centre C; also, let α be the distance from the centre of this sphere to the point *a*. Then two spheres described with centre C and radii $\frac{1}{2}(R \pm \alpha)$ fulfil the condition of being equidistant from the five points.

Every distinct group of four of the five given points in like manner gives two solutions; hence of this kind there are in all *ten* solutions.

Case II. Through any three of the five given points, *a*, *b*, *c*, *d*, *e*, as *a*, *b*, *c*, pass the circumference of a circle; from the centre of the circle erect a perpendicular. This perpendicular is the locus of all points equidistant from points *a*, *b*, *c*. Join the points *d* and *e* by a line; bisect this line by a plane perpendicular thereto. This plane is the locus of all points equidistant from *d* and *e*. The intersection of these two loci is the centre of two spheres equidistant from the five points.

Every distinct group of three of the five given points in like manner gives two solutions; hence of this kind there are in all *twenty* solutions.

Therefore, in general there are *thirty* spheres equally distant from five given points.

The next communication was by Mr. H. A. HAZEN

ON THE RETARDATION OF STORM CENTRES AT ELEVATED STATIONS, AND HIGH WIND AS A PROBABLE CAUSE.

In the absence of Mr. Hazen the following abstract was read by the Secretary, Mr. Baker:

In his tenth paper, published in the January, 1879, number of the American Journal of Science, Prof. Elias Loomis advanced certain evidence, based on barometric observations, to show that apparently the progress of a storm centre was much more rapid at the surface of the earth than at elevations above it. It is the purpose of this article to put forth certain facts which, it is hoped, will tend to elucidate the subject.

Not long since, before this Society, Prof. G. K. Gilbert showed that a high wind had a tendency to depress the barometer column, as determined from his discussion of certain observations made by the Signal Service at the summit and along the side of Mount Washington, New Hampshire. If now a wind can produce such a depression, it would seem as if the wind accompanying a storm and continuing its force at a high station some time after the passage of the storm centre at the base, might cause the apparent retardation.

It is very desirable that special experiments be made, under natural conditions, directly testing the influence of high winds on the barometer column.*

It seems possible to indirectly ascertain such influence from a barometric computation of the height of a mountain by means of observations taken during different wind velocities. Table I gives such a computation of the height of Mount Washington from observations at the base and summit in May, 1872 and 1873.

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*Direct experiments have been made, using a blower for the air current, and an air-tight receiver for the barometer, at short distances, a condition of things, however, which can never occur in nature.

Table I.

Mean amount to be added to the true difference of elevation between the summit and base of Mount Washington in order to give the computed difference, arranged according to the force of the wind.

	Wind Force in Miles per Hour.													
	0 to 10.		11 to 20.		21 to 30.		31 to 40.		41 to 50.		51 to 60.		Above 61.	
	Cases.	Am't.	C.	A.	C.	A.	C.	A.	C.	A.	C.	A.	C.	A.
		′		′		′		′		′		′		′
May, 1872	77	−27.1	25	−18.6	30	−3.1	43	+13.8	65	+10.5	32	+33.9	50	+51.4
May, 1873	104	−43.5	134	−22.0	183	+4.1	135	+15.6	99	+34.9	61	+52.4	27	+80.1

In the above table, for May, 1872, all winds under 10 and above 40 are included, and in May, 1873, all the cases, except a few which were omitted because of serious errors in the observations.

The table shows this remarkable peculiarity that, though with winds above sixty-one miles per hour, the mean computed difference in height is too great by sixty-six feet; with winds under ten miles per hour the mean difference is too small by thirty-five feet. We conclude, then, that some other cause must produce the results, or must act in conjunction with the wind. Taking the wind above sixty-one miles per hour I have found ten cases in which the height was too small by about fifteen feet, also a great number of cases in which, though the wind continued strong from the same direction, yet the computed height continually became less, showing that the wind does not produce a direct effect upon the indications of the barometer. On projecting the curves of pressure we find that there is a uniformity in the occurrence of small and large differences of elevation with the maxima and minima of pressure, the least being found when the pressure is high, and the greatest when it is low.

Grouping a second time, then, with respect to the maxima and minima of pressure, we have Table II.

Table II.

Mean amounts to be added to the true difference of height between the summit and base of Mount Washington to obtain the computed difference.

Date.	Locality.	Maxima of Pressure.		Minima of Pressure.	
		Cases.	Amount.	Cases.	Amount.
			′		′
May, 1872	Mt. W. and base	81	— 32.5	70	+ 57.4
May, 1873	Mt. W. and base	102	— 61.6	137	+ 67.3
Jan., Feb., Mar., Oct., Nov., Dec., 1880.	Mt. W. and mean of B. and P.	119	— 29.1	120	+ 127.0

As the first two horizontal rows of figures apply only to observations for the month of May, and as it would be very desirable to have results for the colder months when the fluctuations are much increased, I have added a third set of figures for the summit of Mount Washington, compared with the mean of Burlington and Portland as the base, and computed the difference of elevation from observations taken at 7 a. m., 3 p. m., and 11 p. m., Washington time, during January, February, March, October, November, and December, 1880.

It is evident from Table II that during the prevalence of relatively high pressure, elevations computed barometrically will, in general, be too small, and, on the other hand, when the pressure is low, the computed heights will be too great. This also explains the coincidence of too great computed heights with high winds, for the reason that the highest winds always occur with relatively low pressure; on the contrary, when the wind is light, the pressure is generally high.

May not this retardation be due to the effect of varying temperature? When a "low" has passed a station at sea level the temperature frequently falls steadily, thus contracting the atmosphere and causing its withdrawal from the upper regions, and a still further fall in pressure there. This process will continue until the fall caused by the low temperature is counterbalanced by the rise due to the advancing "high." The following is given as an illustration:

Observations of air-pressure and temperature at Denver and Pike's Peak, Colorado, in November, 1880.

Day.	Hour. Wash. Time.	Temp. Pike's Peak.	Mean Temp. Pike's Peak and Denver.	Pressure.	
				Pike's Peak.	Denver.
		°	°	"	"
14	7 a. m.	— 5	6	17.75	24.69
	3 p. m.	+ 2	20	17.75	24.64
	11 p. m.	6	19	17.82	24.59
15	7 a. m.	10	22	17.83	24.50
	3 p. m.	14	34	17.71	**24.28**
	11 p. m.	11	16	17.57	24.48
16	7 a. m.	1	6	17.28	24.41
	3 p. m.	— 6	1	17.18	24.44
	11 p. m.	— 14	— 6	17.22	24.58
17	7 a. m.	— 31	— 20	**17.13**	24.54
	3 p. m.	— 19	— 10	17.25	24.49
	11 p. m.	— 16	— 12	17.42	24.42
18	7 a. m.	— 9	— 6	17.48	24.33
	3 p. m.	— 4	7	17.41	24.23
	11 p. m.	— 5	6	17.32	24.08

From these observations we see that, although the air-pressure was at a minimum at Denver, November 15, 3 p. m., yet, owing to the extraordinary cold, the pressure continued to fall at Pike's Peak, (which is 8,840 feet above Denver,) and did not reach its lowest point until forty hours afterward, or November 17, 7 a. m. Extending the same reasoning to the diurnal range of air-pressure we shall find a satisfactory solution of the retardation. From hourly observations at the summit and base of Mount Washington I find that while the morning maximum occurs at 8:30 a. m. at the base, it does not occur till noon at the summit, during this part of the day the temperature is rising rapidly; and hence we may suppose that it produces the continued rise in air-pressure at the summit overbalancing the diurnal range; in like manner the afternoon minimum occurs at 6 p. m. at the summit, or two hours later than at the base, as the temperature begins falling at 2 p. m. This may account for the difference at the two stations. On comparing the night maximum and morning minimum I find little or no retardation; this is what we might expect from the fact that at this time there is little or no change in temperature.

The President, Mr. TAYLOR, called the attention of the Society to the remarkable halo witnessed by many people in Washington last Thursday, June 15, saying that in some respects it was remarkable, and presented some theoretical difficulties. While it had been seen by a number of those present, none had made any scientific observations of it or taken any measurements. A number of other halos were mentioned which, like this, occurred between 10 and 11 a. m., and it was thought worth while to consider whether halos appeared oftener at those hours than at others, and if so, why.

221ST MEETING. OCTOBER 7, 1882.

The President in the chair.

Forty-one members present.

The consideration of the minutes of the last meeting was postponed.

The PRESIDENT welcomed the members to a renewal of the meetings of the Society after the summer vacation.

He also announced that vacancies had been created in the Committee by the resignation of Dr. J. J. Woodward, a Vice-President of the Society, on account of prolonged illness, and of Mr. Marcus Baker, one of the Secretaries, by reason of assignment to duty in California. The General Committee had elected Mr. E. B. Elliott a vice-president in place of Dr. Woodward, and Dr. J. S. Billings a secretary in place of Mr. Baker. The vacancies resulting therefrom in the membership of the Committee had been supplied by the election of Dr. D. L. Huntington, U. S. A., and Prof. C. V. Riley.

Mr. A. S. CHRISTIE made a communication

ON A SYSTEM OF STANDARD TIME.

A prime meridian (say Greenwich) time would, in general, give the hours of the local natural day dissymmetrical with respect to the zenith of the clock face and the zero point of the hour numbers. Turning the dial plate until the prime meridian hour of local mean noon comes to the zenith, eliminates the first mentioned element of dissymmetry, and is a partial adaptation of prime meridian time to local convenience. The second element of dissymmetry is inherent in the nature of numbers, and cannot be eliminated whilst they are retained; for symmetry demands that the zero point shall be either *everywhere* or *nowhere*, neither of which conditions can be satisfied by the symbols now in use. Rejecting them, therefore, and adopting a series of hour symbols having no absolute numerical, but only an ordinal, significance, is another and final step in the adaptation of prime meridian time (such only as to the hour-zero) to general use.

A consideration of what symbols to adopt will immediately suggest, that an abandonment of the artificial, and a return to the simplicity of nature, constitutes the real and complete solution of the problem. That problem may now be stated: To avoid the discordance of local time on different meridians (a discordance which cannot be removed) by the adoption of the same standard time on all meridians, so that the hour and fraction of the hour shall be the same at the same instant everywhere; which standard time shall be marred by no dissymmetry with respect to the globe, alien in no land, essentially local everywhere, cosmopolitan and impartial as the sun himself.

The mere statement of the problem is almost sufficient. The system of time must consist in simply telling *where the sun is* with respect to our terrestrial meridians—the answer in every case must be the same in all quarters of the globe. To limit the geographical knowledge necessary, insure uniformity, and afford hour-zeros, twenty-four equi-distant meridians should be agreed upon as such hour zeros, and named from some country through which, or city near which, they pass. Regard now the dial plate of the clock as the earth, the north pole at center, and meridians, twenty-four of which are actually drawn, radiating to the circumference. (Mr. Henry Farquhar suggests that the dial plate be an actual planisphere.) Bring the local meridian to the zenith and let the hour-hand, revolving once each day, point to the mean sun. The time read from such a chronometer will be the natural, or sun time, proposed in this paper. Space here forbids details with respect to the theory itself, or mention of the objections urged against its practicability; but it may be said in conclusion, in answer to an objection raised by Prof. Coffin, that the longitude of any place is given at once by the clock face at meridian transit of the mean sun, without any subtraction whatever.

Mr. Henry Farquhar urged some objections to the device of reckoning time by meridians an hour apart, as not being sufficiently local to avoid a longitude correction in tables of sunrise and other astronomical events, nor sufficiently universal to escape confusion at points nearly 30 minutes from the standard meridians. He thought the need of a universal standard time, already greatly increased by railway and telegraph communication, would become still more strongly felt in the future. Inconvenience resulting from the occurrence of the 24th hour during daylight at any place, could be obviated by numbering hours beyond 24 and retaining the same day. It would not be suitable to reckon time everywhere from Greenwich midnight, since that would involve a change of day at local 10 A. M. in Sydney, (nearly noon in New Zealand) or, if the hours after 10 A. M. were counted as 25, 26, etc. of the previous day, a discrepancy in date between Australia and Europe. Hours might be reckoned from midnight at 6h. east of Greenwich, noon at 6h. west; though 5¼h. west, a meridian passing near Cumberland, Maryland, would be preferable. The longitude of a place would be the time of mean noon at that place, and count from the last-named meridian westward, from 6h. to 30h., and not from 0h. to 24h. The longitude of Washington, then, would be 23h. 53.2m., that of San Francisco, 26h. 54.6m., Honolulu, 29h. 16.4m., Auckland, 7h. 5.7m., Calcutta, 12h. 51.7m., and Greenwich, 18h. 45.0m. The 6h. meridian would pass through Bering Straits and be the line adopted for the change of date.

East of British India the day would be understood to change at 24h., which hour would arrive at some time less than 6h. after midnight. For the rest of the world, the hours would run above 24, and be diminished by 24 at the time indicated by local custom and convenience for a change of day. In Washington, for example, the conventional day might change at 36h., the hours of next day counting on from 12h., or at 39h. and count on from 15h., according as it was preferred to have the change near midnight or about 3h. after midnight. At Greenwich the hour nearest midnight would be 31h. or 7h.

Mr. Farquhar also showed a proposed form of clock-face, in which the hours were numbered from 0 to 42 in two circuits, 24 being opposite 0, and so on. Such a clock would do for all meridians, but might easily be arranged to have any desired noon-time at the top.

Mr. Coffin remarked that he had failed to appreciate the importance of standard time to the extent to which it had been frequently advocated. If we examine the several departments, in which such time is supposed to be needed, we can better determine in what way a requirement of that kind can be best supplied.

In navigation the time of the prime meridian is a necessity; and this is furnished directly by chronometers regulated to that time, while from astronomical observations the corresponding local time may be found; and both are involved in all questions of longitude. No further standard time is needed in this department.

The use of an astronomical ephemeris also requires the time of the meridian for which it is prepared. A prime meridian common to all nations is a desideratum. But at present the maritime nations of Great Britain and the United States reckon longitudes from Greenwich, while on some of the nautical charts of Russia, Germany, and Spain, longitudes are given from Greenwich as well as from the prime meridian of each respective country. Besides this use of the meridian of Greenwich more general than of any other meridian, the meridian of 180° E. or W. from Greenwich passes near Behring Strait and through an extensive unoccupied region of the Pacific Ocean, where it will be most convenient to have the change of day, which is one less on the east side of such meridian than on the west. Indeed, the change of longitude from east to west, or the reverse, necessarily requires a change of the local day. *Where* the change is made, is arbitrary. For instance, the longitude 175° E. is equivalent to 185° W.; but October 7 in the first case is October 6 in the second. If such noting of the day, which is as much a part of the expression of the local time as are the hours and minutes, is attended to, we have the simple rule, common in navigation and the use of an ephemeris, "To the local time add the longitude if west, subtract it if east, to obtain the corresponding time of the prime meridian;" and this rule includes the day as well as its parts.

Sir John Herschel and others have proposed that longitudes should be reckoned westerly from 0 to 360°. This would complicate the expression for the local day, and congruity would require that the change of day should be at the prime meridian, which would cause great inconvenience and even confusion.

There are some observations of terrestrial phenomena, which it is desirable to have made simultaneously in the same continent or in all parts of the world. This was notably the case in the magnetic crusade some forty years ago, when certain instants of Göttingen times were specified; but the observers had no difficulty, each for himself, in determining and using his corresponding local time. And in meteorological observations, if times are prescribed in the time of any specific meridian, the observers, if of sufficient intelligence to make valuable observations, can readily convert these times into their local times, or the reverse. The constant difference of longitude, expressed in time, is all that each one requires for the purpose.

The great call for a standard time has been made with regard to railroads. A *uniform* time for each road, or connecting system of roads, is needed for regulating the times of starting and the arrival of trains, which each road can best determine for itself, and the time-tables and clocks at the several stations may be reserved for the employés of such roads only. If the time-tables published for information of the travelling public are given in the local time of each place, or a column of constants for the reduction of the published times to the local times is given, the needs of the traveller seem to be sufficiently provided for. A local time differing but little from local mean-solar time is needed to meet the wants of the social and industrial interests of the country, and if it be exactly the mean-solar time, it varies from place to place directly with the longitude.

An essential is that each time-table for railroads should state distinctly what time is used. A neglect of this has and will produce uncertainty and confusion. In a leading railroad guide I found, at a place which I visited ,three time-tables for the same road, without any statement that one of them was in New York time, the others in time of other places.

The suggestion that the dials of clocks should indicate an entire day of twenty-four hours instead of a half day of twelve hours is valuable to a certain extent. This is done in astronomical clocks, and in the astronomical mode of noting time. It would be an improvement in chronometers for nautical use, but sufficient if the dial be marked into the two periods of twelve hours each, into which common, universal use divides the day.

It would seem to be impracticable to change materially the use of local-mean time, now common throughout the country; nor is such change desirable or needed.

It is only within forty years that mean time has been substituted for apparent time in many of our cities, though its advantages had long been recognized by astronomers and time regulators; and within twenty years that the sun's rising and setting have been stated in mean, instead of apparent, time in the popular almanacs of the day.

The subject-matter was further discussed by Messrs. Doolittle, Elliott, Riley, Hilgard, Gilbert, and Mussey.

Mr. G. Brown Goode then read a paper

ON THE FISHERIES OF THE WORLD.

This has been essentially printed in the "Cyclopædia of Political Seience, Political Economy," etc., edited by John J. Lawlor, published at Chicago, vol. 2, pp. 211–231, (Art. "Fisheries,") 1883.

222d Meeting. October 21, 1882.

The President in the Chair.

Twenty-two members were present.

The minutes of the last meeting were read and adopted.

Mr. S. C. Busey read a paper

ON THE INFLUENCE OF THE CONSTANT USE OF HIGH-HEELED SHOES UPON THE HEALTH AND FORM OF THE FEMALE, AND UPON THE RELATION OF THE PELVIC ORGANS.

(The paper will appear in full in vol. 7, Gynecological Transactions.)

[Abstract.]

The foot and its coverings is not a new subject. Far more attention, however, has been given to the style and display of the covering than to the comfort and physical well-being of the foot. From this point the author gave a historical resumé of the different coverings for the feet which had been used as far back as the ancient Egyptians. The heel at first was designed to make short men look tall, and like other parts had undergone many changes to suit the whims of fashion and taste. During the reign of Louis XVI this objectionable style began to disappear, but has been again revived, and is perhaps more general now than at any previous time. Then followed a brief summary of the causes that produced deviations of form, with special reference to the effect of the constant use of French high-heeled shoes. Diagrams were exhibited showing the distortions of the feet caused by them, and the consequent changes in the joint-flexures and spinal curves. He claimed that the primary deflection took place at the base of the line of gravitation, and above this point there were greater or lesser alterations of the flexures and curves along the bony framework. Special attention was directed to the increased obliquity of the pelvis, and to the probable corresponding change in the position of the womb and other pelvic organs, which might be an important factor in the causation of some of the disorders of the female reproductive organs.

The subject-matter was discussed by various members.

A communication was submitted by Mr. Theodore Gill

ON THE CLASSIFICATION OF THE INSECTIVOROUS MAMMALS.

In 1875 the author published a "Synopsis of Insectivorous Mammals" in the Bulletin of the United States Geological Survey of the Territories, under Hayden, (vol. 1, No. 2; 2d series, 1875, pp. 91–120,) and proposed several modifications in the classification. The principal of those modifications were (1) the union of the typical Insectivora and Dermoptera (*Galeopithecus*) is one order, as had been long before proposed by Frederic Cuvier and Wagner, but their distinction as two suborders; (2) the distribution of the true insectivores under two groups characterized by their molar dentition, and the complete subordination of the form of the body, and (3) the combination of families into super-families, and (4) the subdivision of several into subfamilies. The scheme thus promulgated has met with gratifying and unexpected favor, and has been essentially adopted by Messrs. Coues, Jordan, Dallas, Trouessart, and Dobson. Surgeon-Major Dobson's opinion is especially weighty, as he has undertaken a monograph of the order, and his opportunities for investigation have been unequalled. Since the publication of the Synopsis, in 1875, several forms have been made or become known which compel the recognition of new subordinate groups in the order; and Major Dobson has also proposed to raise the Solenodontinæ from the rank of a subfamily of Centetidæ to that of a family by the side of the latter. The assessment of the comparative value of different groups is a difficult and delicate task, and much can be said for as well as against any given proposition. The Solenodonts are doubtless as distinct from their nearest of kin as are some of the generally admitted families of mammals, and therefore it will be quite proper to recognize the family value of the type. But there are other groups of Insectivora which have been associated together in the same families which are equally or more entitled to the same distinction. Indeed, the only subfamilies of the "Synopsis of Insectivorous Mammals" which do not contrast more seem to be the Gymnurinæ and Erinaceinæ. If the Solenodontidæ are to be differentiated with family rank from the Centetidæ, so should the others. We would then have the following families:

SUBORDER DERMOPTERA.

1. Galeopithecidæ.

SUBORDER BESTIÆ.

DILAMBDODONTA.—Bestiæ with broad molar teeth surmounted by W-shaped ridges.

TUPAIOIDEA.

2. Tupaiidæ.
3. Macroscelididæ = Macroscelidinæ.
4. Rhynchocyonidæ = Rhynchocyoninæ.

ERINACEOIDEA.

5. Erinaceidæ, with the two subfamilies Gymnurinæ and Erinaceinæ.

SORICOIDEA.

6. Talpidæ = Talpinæ.
7. Myogalidæ = Myogalinæ.
8. Soricidæ.

ZALAMBDODONTA.—Bestiæ with narrow molar teeth having V-shaped ridges.

CENTETOIDA.

9. Centetidæ = Centetinæ.

10. Oryzoryctidæ = Oryzoryctinæ, *Dobson*, Mon. Insect., pp. 2, 71. 1882.

11. Solenodontidæ, *Dobson*, Mon. Insect., pp. 3, 87. 1882.

12. Potamogalidæ.

13. Geogalidæ = Geogalinæ, *Dobson*, Mon. Insect., p. 2. 1882.

CHRYSOCHLOROIDEA.

14. Chrysochloridæ.

The "Monograph of the Insectivora," by Surgeon-Major Dobson, will fill a long-felt want, and exceptionally well represent the present condition of our knowledge respecting the existing representatives of the order.

223D MEETING. NOVEMBER 4, 1882.

The President in the Chair.

Forty-five members present.

The minutes of the last meeting were read and approved.

A communication was made by Mr. G. K. GILBERT:

ON A GRAPHIC TABLE FOR COMPUTATION.

[Abstract.]

On Nov. 17th, 1881, a new method of barometric hypsometry was presented to the Society, and this has since been published in the Second Annual Report of the Geological Survey. It involves a new formula. In the application of that formula an approximate value of the required altitude is first obtained, to which a correction is then added. For the determination of this correction a table was prepared, to be entered with two arguments. Although this table was spread out on six octavo pages, and although the deduced correction is small, it was nevertheless found impracticable to avoid a double interpolation. To escape this inconvenience the graphic table was afterwards devised.

The graphic table consists of three super-imposed sets of lines. In each of two sets the lines are straight, parallel, and equidistant, and those of one set intersect those of the other at right angles.

These represent values of the two arguments. The lines of the third set are curved, and each one represents a value of the correction. In use, the straight lines representing the values of the two arguments are traced to their intersection, and from the relation of this point of intersection to the curved lines the correction is deduced.

This method is theoretically applicable to the tabulation of any quantity which is the function of two variables, but is practically useful only when the quantity to be determined is either expressible by a small number of digits, or else is subject to only a small range of variation.

A second graphic table was exhibited, having for its object the computation of altitude from horizontal distances and vertical angles as data. On this, successive values of computed altitude are indicated by parallel, equidistant, straight lines. Vertical angles are indicated by the directions of lines radiating from a point, but the intervals of these lines are not equal. Distances are measurable along these radial lines, but are not indicated in the drawing. The scale of distances is identical with that of the map, including the points whose altitudes are to be computed. The lines are drawn on tracing-linen.

For the use of this table it is postulated that the points whose altitudes are to be computed are correctly placed upon a map, and that the same map indicates a point from which the elevation or depression angles of the various points were measured. The transparent linen bearing the table is placed over the map and connected with it by a pin passing through the common origin of the radial lines, and also through the indicated position of the station from which the angles were measured. About this point as a centre the table is then moved until the radial line, indicating the vertical angle of one of the points, is brought immediately over the representation of that point upon the map, The position of that point among the parallel lines then indicates the desired altitude.

The use of this device is limited to a special case, but that case is one of frequent recurrence in the preparation of contour maps, and it is hoped that the device will lead to an economy of time.

The principle involved in the application of a *transparent* graphic table permits of the extension of the graphic table to cases involving three arguments. Two sets of lines could be drawn on a lower sheet, and two other sets on an upper transparent sheet, and these sets could be so constructed that one of them would represent a function of three variables represented by the other three.

The paper was discussed by Mr. HARKNESS and Mr. H. A. HAZEN, Mr. HARKNESS pointed out that the construction of a two-argument computation table by means of curved lines was not novel.

224TH MEETING. NOVEMBER 18, 1882.

The President in the Chair.

Forty members present.

The minutes of the last meeting were read and adopted.

Mr. E. B. ELLIOTT spoke

ON SURVIVORSHIPS, WITH TABLES AND FORMULAS OF CONSTRUCTION.

(No abstract has been furnished.)

Mr. H. A. HAZEN submitted a paper

ON THE COMING WINTER OF 1882–'83.

The following is an abstract:

It has been a great desideratum, and one which has called out the efforts of many men, to determine in advance the probable character of a season. A prominent meteorologist has inferred that the coming winter is to be a very severe one, because, as he says, "every one knows that a cold and wet summer is invariably followed by a cold and stormy winter." In order to obtain probable sequences in the weather, if we could in any way determine the mean temperature or pressure over an extensive region, it would seem as though results would be far more satisfactory than those from a single station. The following plan has been adopted for ascertaining such mean results:

We may draw isobars or any isometeorologic lines upon a map of a country; then we may rule a large number of squares upon glass or some transparent substance; and after that, by placing these squares upon the map, we may at a glance interpolate the exact pressure or temperature in each square, and a mean of all the squares would give a mean for the whole country.

Such results have been determined for the United States east of the 97th meridian for each month since July, 1873. (These were exhibited graphically before the Society.) We find a singular result on comparing these figures with similar figures for the single station of Providence, R. I., (observations at this station, from 1832 to 1876, were kindly furnished the author by the Smithsonian Institution,) namely, a striking uniformity in the values; and we may conclude that, as far as mean monthly temperatures are concerned, we may consider those at any one station fairly comparable with the same over an extensive region.

In the accompanying table each summer, and the following winter, at Providence, R. I., have been considered as cold, cool, mean, warm, or hot; and an effort has been made to establish the character of the winter that follows a summer having any one of the above characteristics:

Year.	Summer.	Winter following.	Year.	Summer.	Winter following.
1832	cold	warm	1857	cold	hot
1833	cool	warm	1858	cold	hot
1834	warm	cold	1859	mean	hot
1835	mean	cold	1860	cool	hot
1836	cold	cold	1861	cool	warm
1837	cold	mean	1862	cold	warm
1838	hot	cold	1863	cold	hot
1839	mean	cool	1864	cold	warm
1840	warm	mean	1865	mean	hot
1841	mean	hot	1866	warm	warm
1842	mean	mean	1867	mean	mean
1843	mean	mean	1868	mean	cold
1844	mean	warm	1869	cool	warm
1845	cool	cool	1870	hot	hot
1846	cold	hot	1871	mean	cold
1847	mean	hot	1872	hot	cold
1848	warm	cool	1873	mean	mean
1849	mean	hot	1874	mean	cold
1850	mean	hot	1875	cold	mean
1851	mean	cool	1876	warm	cold
1852	warm	warm	1877	warm	hot
1853	warm	cool	1878	warm	cool
1854	warm	cool	1879	mean	hot
1855	hot	cold	1880	hot	cold
1856	hot	cold	1881	warm	hot

On examining this table we find that of the eight cold summers three were followed by a hot winter, three by a warm winter, one by a mean winter, and one by a cold winter, which gives one out of eight cold summers followed by a cold winter, and six by a hot or warm winter. Taking all the cases, in forty-eight per cent. of them any summer was followed by a winter of an opposite character; in forty-two per cent. the summers or winters were mean, and in only ten per cent. of the cases were the summers followed by winters of the same character.

Making a similar comparison at Fort Snelling, Minnesota, we find, out of the sixty-eight summers and winters on record at that station, that fifty-two, or seventy-six per cent., were followed by a season of the opposite character; ten, or fifteen per cent., by a season of the same character; and six, or nine per cent., were doubtful.

We may also infer the character of the coming season for the United States by noting the movement of the permanent winter area of high pressure in respect to the Rocky mountains. It would seem as though these tended to ward off the cold if the high area settles down to the west of the range.

The winter of 1877–'78 was warm, for during every month of that season the high pressure was west of the Rockies, and the cold waves were effectually barred from the Eastern States. In December of 1877 the high pressure was spread over a vast extent of territory west of the range, and the temperature in the east rose to 7.2 degrees above the average.

The winter months of 1880–'81 were cold. During that time the high pressure was well to the east of the Rockies, and the temperature in the east fell below the average from two to six degrees. The winter of 1881–'82 was warm, as the following tabulated form shows, the plus sign indicating so many degrees above the average.

Month.	Temperature.	Position of high pressure.
1881, September	+4°.6	Normal.
October	+3°.8	Normal.
November	+2°.2	Strong west of range.
December	+7°.7	Strong west of range.
1882, January	+2°.7	Strong west of range.
February	+5°.6	Strong west of range.

It is now too early to determine exactly what the weather of the winter of 1882–'83 will be, but the indications are that it will be a medium rather than a severe one, as some have predicted. The past summer having been cold and stormy, a warm winter ought to follow; and the high pressure during last September was slightly west of the Rockies, while during October it was so far to the West and North as to rest over the Cascade range in Oregon. If it continues west of the Rocky-Mountain range a severe winter is not probable.

Mr. Henry Farquhar commenced a communication on

EXPERIMENTS IN BINARY ARITHMETIC.

The meeting was adjourned at the usual hour, (10 o'clock,) with the understanding that the unfinished communication should be taken up at a subsequent meeting.

225th Meeting. December 2, 1882.

The President in the Chair.

Fifty members present.

The minutes of the last meeting were read and adopted.

In accordance with the by-laws of the Society, the President, Mr. William B. Taylor, delivered the annual address.

ANNUAL ADDRESS

ON PHYSICS AND OCCULT QUALITIES,

By William B. Taylor.

"Vis abdita quædam."

Lucretius. (*De R. N.*, lib. v. 1232.)

1. *The Dynamic and Kinematic Theories of Force.*

From the remarkable success of scientific investigation in assailing the domain of darkness,—in continually bringing the phenomena of nature more and more under the recognized empire of certain necessary laws and principles, the induction seems natural that outstanding mysteries—the ultimate constitution of matter, the nature and genesis of life and of mind itself—must in time yield to the same persistent siege of searching analysis, and be reduced to subjection under the same government, as simple servitors of an all-embracing mechanical philosophy.

In recent years, a still further induction has been ventured upon by some, to wit, that even the fundamental laws themselves of all physical action must, when properly formulated, be interpreted by simple mechanics;—all properties of matter resolved into mass or inertia, and finite extension or form,—all potentiality of matter into varying modes of motion. And it has been strongly maintained by this class of physicists, that until such consummation, the mind must still be held in thrall of mysterious unimaginable powers, the helpless devotee of "occult qualities" which science in the past has so laboriously and successfully endeavored to relegate to the shadowy limitary of metaphysics. This form of speculative doctrine, (premonitions of which may be traced back several hundred years,) may now be regarded as having attained the importance and cohesion of a school, numbering in its following a few quite eminent disciples, who agree in denying the real existence of any inherent "forces" in matter, and in holding such a designation to be merely a convenient but provisional ideal abstraction. While on the other hand the large majority of scientific thinkers (perhaps comprising most of those who have reached the conservatism of middle age) still adhere to the older conception of primeval "force" as an essential hypostasis of the operations of nature. And thus the battle so long waged (and so long practically decided) between realism and nominalism in the field of mind, bids fair to be revived (though under quite other auspices) in the field of matter. These two modes of thought may be conveniently designated the *dynamic* and the *kinematic* theories of physics. In the terminology of the *Philosophie Positive*, the dynamic theory still lingers in the shaded vale of "metaphysics," while the kinematic theory has reached the sunny hill of "positivism."* An attempt to examine and compare these divergent lines of interpretation may be a not unprofitable exercise.

The Cohesion of Matter.—Among the earliest of our experiences is the perception that the bodies around us possess in varying degrees a quality of "hardness;" and the child who gathers a rounded pebble on the beach, (if perchance inspired by its inquisitive instinct to see what the interior looks like,) discovers that to break the pebble requires the heavy and repeated strokes of a stone much larger than itself. Whence this remarkable tenacity of coherence? Whence the striking physical difference between the pebble and an equivalent mass of very fine sand?

From a large variety of facts observed in the actions of solution, of fusion, of evaporation, of the very existence of a kinetic temperature in bodies, in the phenomena of crystallization, of isomorphism, of definite and unvarying numerical mass-ratios in chemical combinations, of polymerism or serial groupings in multiple proportion, of isomerism, of allotropy, and of other more recondite habitudes of matter, the general conviction has been reached (by what has been called "a consilience of inductions") that all substance is a collection of constituent molecules of probably uniform magnitudes held together by some powerful agency. A few it is true have asserted their superiority to such popular weakness as the admission of the atomic theory; but as their vague suggestion of some continuous or colloidal form of substance has not even pretended to interpret any of the classes of phenomena just alluded to, such dis-

*AUGUSTE COMTE, in his *Positive Philosophy*, maintains that "Forces are only motions produced or tending to be produced. - - - We hear too much still of the old metaphysical language about *forces* and the like; and it would be wise to suit our terms to our positive philosophy." (Harriet Martineau's Translation. London, 1853: book I, chap. 4.) Even *inertia* is treated as a metaphysical fiction.

sent may be summarily dismissed as the mere exhibition of an unprofitable mental captiousness.*

The kinematist repudiating any attractive force in nature would explain the strong cohesion of matter by the hypothetical external pressure of a hypothetical surrounding fluid. The Plumian professor of astronomy and physics in the University of Cambridge—James Challis—(a successor of Roger Cotes and of George B. Airy) has declared "the fundamental and only admissible idea of *force* is that of pressure, exerted either actively by the æther against the surfaces of the atoms, or as re-action of the atoms on the æther by resistance to that pressure."† And the professor of physics in the University of Edinburgh—Peter G. Tait—having also relegated the source of all material energy to the action of the highly attenuated matter diffused through space, thinks it probable that "force" has no existence, excepting as a convenient expression of a mere rate of transference of kinetic energy.‡

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* "The existence of atoms is itself an hypothesis, and *not* a probable one. - - - All dogmatic assertion upon such points is to be regarded with distrust." (*A Manual of Inorganic Chemistry*, By CHARLES W. ELIOT and FRANK H. STORER. 2d edition, revised, New York, 1868: chap. XXV, p. 605.) And yet these negative dogmatists have not shown themselves capable even of *thinking* of so elementary a fact in their science as "polymerism" apart from the terms of the atomic conception. As Prof. J. CLERK MAXWELL has well observed, "The theory that bodies apparently homogeneous and continuous are so in reality, is in its extreme form a theory incapable of development. To explain the properties of any substance by this theory is impossible." (*Encyclopædia Britannica.* 9th ed., 1875: art. "Atom," vol. III, p. 38.) The objection to atomism sometimes urged—that since magnitude is admitted abstractly or mathematically to be infinitely divisible, therefore any finite particle of matter must also be *physically* so conceived,—betrays so strange a confusion of ideas as to merit no serious answer. Yet so illustrious a mathematician and philosopher as LEONARD EULER was guilty of this gross paralogism. (*Letters to a German Princess.* May 3, 1761: vol. II, let. 9.)

† *Principles of Mathematics and Physics.* By JAMES CHALLIS. 8vo. Cambridge, 1869: hyp. V, p. 358.

‡ In an evening lecture on "Force" delivered September 8, 1876, at Glasgow, (during the session of the British Association,) Prof. TAIT announced that "there is probably no such *thing* as force at all! That it is in fact merely a convenient expression for a certain *rate.*" And referring to the corpuscular hypothesis of force, he thought "The most singular thing about it is that if it be true, it will probably lead us to regard all

It is very certain, however, that the hypothetical fluid of cohesion-pressure must be something entirely different in constitution from the luminiferous æther, since any mode of action which could be imagined for compressing together the elements of matter, would necessarily be incompatible with the transmission of solar radiation having the quality and properties of the vibrations actually observed. The fantastic scheme of Le Sage (in which cohesion is effected by the quaquaversal impacts of infinitesimal corpuscles flying swiftly in all directions, and whose various sizes determine the differing collocations of chemical unions,)—notwithstanding the approval of Prof. Tait,*—scarcely requires a "serious consideration."† Nor has any form of impact, of pressure, or of undulation, yet been proffered by the ingenuity of the kinematist—either at all adequate to the maintenance of the known conditions of matter, or indeed in itself at all conformable with any known modes of action.

The dynamist having searched in vain for any plausible co-ordination of the indisputable facts of cohesion with an intelligible mechanical agency, simply acquiesces in the result, and without invoking the unknown or the irrelevant, accepts this established property as ultimate and inexplicable.

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kinds of energy as ultimately kinetic." (*Nature.* Sept. 21, 1876: vol. XIV, pp. 459, 463.)

The climax of kinematism however has been reached by the inventor and apostle of the "fourth state of matter,"—WILLIAM CROOKES, who is disposed to dismiss matter itself to the same limbo—of changing position: "From this point of view then matter is but *a mode of motion;* at the absolute zero of temperature the inter-molecular movement would stop, and although *something* [?] retaining the properties of inertia and weight would remain, *matter*—as we know it—would cease to exist." (*Nature.* June 17, 1880: vol. XXII, p. 153.) This seems to touch the sublime "secret" of GEORGE WILLIAM FREDERICK HEGEL, in which "nought is everything, and everything is nought."—*Seyn und Nichts ist dasselbe.*

* *Lectures on some recent advances in Physical Science.* By P. G. TAIT. 12mo. London, 1876: lect. XII, p. 299.

† "The hypothesis of Le Sage - - - is too grotesque to need serious consideration; and besides will render no account of the phenomenon of elasticity." Sir JOHN F. W. HERSCHEL, "On the Origin of Force." (*Fortnightly Review.* July 1, 1865: vol. I, p. 438. Also, *Familiar Lectures on Scientific Subjects.* 12mo. London, 1866: art. XII. pp. 466, 467.)

The Elasticity of Matter.—To select another illustration, the child throwing his rounded marble downward on a stone pavement finds te his surprise that it rebounds like his play-ball, and that he may, without stooping, catch it in his hand. What explanation is to be given of this direct and sudden reversal of movement? To this familiar quality of matter, we give the name of "elasticity." But by what more simple formula of mechanics shall we represent to ourselves this property *elasticity?* Kinematists abjuring alike objective "qualities" and subjective "abstractions" have been severely taxed in their attempts either to ignore the attribute or to reduce the phenomenon to some phase of molecular vibration.

Some few—consistent in their rejection of all quality from material substance—have boldly denied the existence of elasticity; or rather have ventured to affirm that perfectly hard or inelastic atoms or masses would on collision alike rebound, precisely as though they were elastic.* This startling conclusion—apparently necessitated by their fundamental assumption "the conservation of motion"—requires for the intelligent student of rational mechanics, no discussion.

Other kinematists have resolutely endeavored to explain the resilience of colliding bodies as the special resultant of composite motions. One of the most earnest of these has been the Italian astronomer and physicist Angelo Secchi, who in an elaborate essay on the ultimate identity of all the physical forces as simple modes of motion, remarks: "It is evident that this 'elastic force' can be admitted only as a secondary force derived from another antecedent in an aggregate of atoms, that is in a compound molecule; and that it cannot be admitted as pertaining to the elementary atoms. Indeed, elasticity in its ordinary acceptation requires a void space within the molecule to allow the form to be changed by compression and afterward restored; while on the contrary it is the necessary condition of real atoms—by conception—to be impenetrable [in-

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* This thesis was maintained by JOHN HERAPATH, in his work on *Mathematical Physics.* 8vo. 2 vols. London, 1847: (vol. I, pp. 106–137.) As stated by NEWTON however, "Bodies which are either absolutely hard, or so soft as to be void of elasticity will not rebound from one another. Impenetrability makes them only stop. If two equal bodies meet directly *in vacuo*, they will by the laws of motion stop where they meet, and lose all their motion and remain in rest, unless they be elastic and receive new motion from their spring." (*Optics.* 2d edition, 1717: book III, Qu. 31.)

compressible] and not an aggregation of other solid particles. Hence they cannot be supposed to have any internal voids in which their parts could be contracted or dilated. - - - We believe we are able to show that it is by no means a necessary position to accept this elastic property as a primitive force, but that the apparent repulsion of these atoms and their rebound originates solely from their proper motion, and for this it is sufficient simply to suppose them to be *in rotation*."* He then proceeds to develop his theory of mechanical elasticity from the co-operation of the projectile motion of bodies with the internal rotations of their constituent molecules; citing in support of his assumption, the mathematical researches of Poinsot.† In this important foundation of his system however, the zealous physicist has built upon an entirely mistaken apprehension of true mechanical principles, and hence of course upon a strange misapprehension of the actual discussion by Poinsot. This eminent mathematician who has investigated so thoroughly the theory of rotatory movements has shown that in the collision of inelastic bodies, endowed with rotation, the velocity of deflection may in special cases exceed the velocity of incidence, in other special cases may be just equal to it, and lastly in general will fall short of it, being in many cases entirely destroyed. Thus a rotating inelastic body has two points between the center of inertia and that of percussion, which on impact with a fixed resistance in the line of their direction will produce a resilience of higher velocity than that of collision,—of course by the conversion and absorption of so much of the rotary motion. There are other two points from the direction of whose impact will result a velocity just equal to that of the original motion of the body;—in the one case absorbing one-third of the rotary motion, in the other case absorbing two-thirds of it. If the impact be in the line of the center of inertia, the whole of the translatory motion is arrested without affecting the rotary motion. [In the case of two equal inelastic spheres rotating with equal and opposite velocities on parallel transverse axes and meeting at a point on their equators, the bodies

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* *L' Unitá delle Forze Fisiche;* Saggio de filosofia naturale. Del P. ANGELO SECCHI. 12mo. Rome, 1864: chap. I, sect. 6, pp. 36, 37.

† Father SECCHI'S reference in a foot-note is to "*Questions dynamiques sur la percussion des corps:* pag. 21 e 29, dell' edizione a parte, ed anche il Giornale di Liouville, - - - a pag. 36."

would lose entirely their travelling motion, still retaining their rotations. So also if their axes were equally inclined so as to bring the points of impact on corresponding circles of latitude; the limiting case of which would be an impact on their poles of motion in the line of their common axes of rotation.] Lastly if a rotating inelastic body should meet a fixed resistance in the line of the center of percussion, not only the translatory—but the rotary velocity as well—would be entirely destroyed.* If we conceive a molecule as consisting of a congeries of atoms having an orbital revolution (analogous to a solar system), a very similar analysis will apply to the cases of collision.

It is very clear then that the device of storing up additional kinetic energy in the form of internal rotation (or revolution) fails utterly to reproduce the phenomena of motion exhibited by elasticity. The resulting effects cannot be admitted as at all analogous; since the internal kinetic energy assumed is either wholly or largely absorbed and exhausted by a single collision, and a second impact can never reproduce the effects of a first one; while *elastic* force remains perpetual and unimpaired by constant action.

Elasticity accordingly, equally with cohesion, is a fact of nature, a property of matter, which can neither be interpreted by any form of motion, nor resolved into any mechanical concept.† Those therefore who would formulate the elements of things devoid of

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* LOUIS POINSOT. The latter portion of a series of mathematical discussions under the general title—*Questions dynamiques sur la Percussion des Corps*; published in Liouville's *Journal de Mathematiques* for 1857: vol. II, pp. 281-308.

† "Elasticity without an action *e distanti*—even between the adjoining particles— is inconceivable. What is meant by elasticity? Surely such a constitution of the assemblage of particles as makes them recede from each other." Prof. JOHN ROBISON. (*A System of Mechanical Philosophy*. 8vo. 4 vols. Edinburgh, 1882: vol. III, p. 139.)

"An alteration of the form of a solid body is called a *strain*. In solid bodies strain is accompanied with an internal force or *stress*; those bodies in which the stress depends simply on the strain are called 'elastic,' and the property of exerting stress when strained is called elasticity. - - - The general fact that strains or changes of configuration are accompanied by stresses or internal forces, and that thereby energy is stored up in the system so strained, remains an ultimate fact which has not yet been explained as the result of any more fundamental principle." Prof. J. CLERK MAXWELL. (*Matter and Motion*. 1876: chap. V, arts. 83, 84; pp. 70, 71.

quality, have on their own declaration no right to the use of either term in considering any physical problem.

Were the examination to stop here, it might appear that the only difference between the dynamist and the kinematist is that the former—failing to find any satisfactory explanation of certain habitudes of matter, despairs of deeper insight and accordingly seeking no further, accepts the conclusion that these are insoluble; while the kinematist more hopeful, has an abiding faith that the same processes which have so successfully (or at least so largely) deciphered the riddles of light, of heat, of gaseous constitution, may be expected in time to resolve these other enigmas though they be not yet expounded. It is necessary therefore to go back still further and examine the character of this induction, by a cursory review of the postulates of the mechanical theory of light, of heat, and of the kinetics of discrete molecules.

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2. *The Theory of Molecular Kinetics.*

In the last century both light and heat were generally regarded as material emanations; the former, of radiant corpuscles, the latter, of a peculiarly rare and penetrating fluid. Earlier kinetic hypotheses of these so-called "imponderables"—however ingenious—were not supported by a sufficient induction from observed facts to justly entitle them to unqualified acceptance. And the doubts and difficulties suggested by the speculations of Newton were a striking illustration of his recognized sagacity; notwithstanding the occasional censures of modern popular lecturers, trumpeting their own superior wisdom.

The Vibratory Theory of Heat.—The fluid or "caloric" theory of heat (though often questioned or opposed) was first decisively overthrown at the close of the century by Benjamin Thompson, an expatriated American, better known as Count Rumford, whose experiments unescapably demonstrated the resolution of heat into an intestine motion, by the fact of its interminable generation in friction through the agency of continued motion.* It was not how-

* *Phil. Trans. Roy. Soc.* 1798: vol. LXXXIII, pp. 80–102. This admirable memoir read before the Royal Society of London, January 25, 1798, (in which RUMFORD—from the fact "that the source of heat generated

ever until about the middle of the present century that the conception attained a scientific definiteness and currency through the accurate determination of the kinetic or dynamic value of heat.

The Undulatory Theory of Light.—Nearly simultaneously with the work of Rumford in the field of heat, the investigations of Dr. Thomas Young, at the beginning of this century, relative especially to the interference of two luminous rays in particular cases, in like manner overthrew the theory of corpuscular emission in the field of light, by demonstrating a destruction or obliteration—quite intelligible as a conflict of wave motion, but entirely inadmissable and unthinkable as a mutual extermination of conflicting substance.* Through the refined labors of Young,—admirably assisted and re-enforced by the able efforts of his skillful and worthy rival Fresnel,—the varied and complex phenomena of dioptrics were more and more fully brought under the dominion of a rational kinetics. And thus it resulted that the new doctrine of insensible motion obtained from the scientific world a much more rapid and general acceptance in its application to light than in its application to heat. So that it was not unusual some forty or fifty

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by friction in these experiments appeared evidently to be inexhaustible," argued that this product "cannot possibly be a material substance:") may be said to furnish the first rough approximation to the mechanical equivalent of heat. The author estimated the heat produced by a one-horse power as equivalent to that obtained from the burning of nine wax candles, each three-quarters of an inch in diameter; or to the combustion of a little more than one-third of a pound of wax in two and a half hours. This essay also presents the first suggestion of the mechanical correlation of animal power with heat motion.

Dr. Young held that Rumford's experiments "appear to afford an unanswerable confutation of the whole of this doctrine:—[that of a 'caloric' fluid.] - - - If heat is not a substance, it must be a quality; and this quality can only be motion." (*Lectures on Natural Philosophy.* 1807: lect. 52: vol. I, pp. 653, 654.)

"The hypothesis of caloric" says Prof. J. Clerk Maxwell "or the theory that heat is a kind of matter is rendered untenable—first by the proof given by Rumford that heat can be *generated* at the expense of mechanical work; and secondly by the measurements of Hirn, which show that when heat does work in an engine, a portion of the heat *disappears.*" (*Theory of Heat.* 1872: chap. VIII, p. 147.)

*"*Phil. Trans. Roy. Soc.* A memoir read July 1, 1802: vol. XCII. p. 387; and one read November 24, 1803: vol. XCIV. pp. 1–16.

years ago, to find our college professors zealously inculcating the undulatory theory of light, while still maintaining the hypothesis of a "caloric" for heat.

William Herschel had found, at the beginning of the century, that the solar spectrum, as produced by an ordinary glass prism, manifested a heating power slight at the violet end, but gradually increasing to the red end, and extending a considerable distance beyond the less refrangible limit of visible rays, near which limit the maximum effect was reached.*

Johann Wilhelm Ritter, of Jena, a year later found that the chemical action of the solar spectrum, as exhibited in the darkening of silver chloride, increased toward the violet extremity, attaining a maximum beyond the most refrangible limit of luminous dispersion.† Hence, it came to be generally believed that the solar rays comprise three essentially distinct and independent kinds of energy, representing three different forms of wave-motion. This appeared the more probable from the entirely dissimilar orders of effect observed (as interpreted by the impressions of our senses), in calorific energy, in optical luminosity, and in chemical agency.

It was shown however by Alexandre Edmond Becquerel that the so-called chemical rays were not distinguishable by their refrangibility, and that photographic effects could be obtained with suitable re-agents from any region of the spectrum.‡ And finally, by the researches of Dr. John W. Draper, it was fully established that Herschel's results depended on the great distortion (as well as unequal absorption) inseparable from every prismatic or refractive spectrum, and that Ritter's results depended on a very limited and insufficient induction. And thus it has slowly come be recognized that in every normal spectrum, freed from distortion or selective absorption, (and equally freed from selective generalization), the three classes of effects, thermal, photic, and actinic, are equably or proportionally distributed; that as these several activities are equally amenable to polarization, to interference, and to spectral irradiation and absorption, there is in fact but a single form of

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**Phil. Trans. Roy. Soc.* 1800: vol. XC, pp. 291, 318, 439, 440.

† Gilbert's *Annalen der Physik.* 1801: vol. VII, p. 527. Nicholson's *Journal of Natural Philosophy*, [etc.] August, 1803: vol. V, p. 255.

‡ *Annales de Chemie et de Physique.* April, 1849: vol. XXV, pp. 447–474.

ætherial undulation, the differences of whose manifestations depend entirely upon the nature of the body, organic or inorganic, on which it falls.*

Molecular Thermo-dynamics.—Passing from the wave theory of radiation to the related subject of the internal re-actions of bodies, the application of thermo-kinetics to the facts of temperature has taught us that the molecules of all bodies are in a state of very rapid though minute movement, and that this movement, while being constantly transferred and expended, (and thus ever tending to the absolute zero,) is yet incessantly maintained in varying quantity by repeated re-enforcements from natural and artificial sources of heat, and by mutual interchanges. In the case of solid bodies, whose constituent molecules are held together by what we must call (in default of any names as yet invented by the kinematist) the qualities of *cohesion* and *adhesion*,—their mutual contact being resisted and prevented by what we must for the present call a repellant quality, the temperature motion is in the nature of an oscillation or rather irregular reverberation within the narrow limits of opposite resistances, by which the relative mean position of the particles and the stability of the body are preserved. By the term "cohesion" is designated simply the observed fact of a resistance to divellent or tensile stress; by the term "adhesion" is designated the observed fact of resistance to torsional or shearing stress.

When the energy of the molecular movements is increased until the modulus of "adhesion" is equalled, the point of melting is reached, and the molecules instead of being restored to their antecedent positions are carried irregularly from the influence of neighbor to neighbor, and thus become fluent by being deflected among each other in all possible directions. In this "liquid" condition of

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**Am. Jour. Sci.* Jan. and Feb., 1873: vol. v, pp. 25–38, and 91–98. Dr. DRAPER'S results (so far as the refrangibility of radiant heat is concerned) have recently been confirmed by the refined investigations of Prof. S. P. LANGLEY, by means of his "actinic balance." (*Proceed. Am. Acad.* Jan., 1881: vol. XVI, p. 342; *Am. Jour. Sci.* March, 1881: vol. XXI, p. 187; *Nature.* Oct. 12, 1882: vol. XXVI, p. 588.)

"A ray of specified wave-length and specified plane of polarization, cannot be a combination of several different things, such as a light-ray, a heat-ray, and an actinic ray. It must be one and the same thing, which has luminous, thermal, and actinic effects." J. CLERK MAXWELL. (*Theory of Heat.* 1872: chap. XVI, p. 218.)

the mass, adjacent molecules although entirely freed from the adhesion which constitutes rigidity, yet (as has been shown by Joseph Henry) preserve their mutual cohesion practically unimpaired: * and hence devious as may be their wanderings, no portion of their excursions can be called a free path.

If the rapidity of the mean internal motion be still further accelerated until the momentum of the molecules is equal to their modulus of "cohesion," the temperature of evaporation is reached, and the molecules are impelled from their restraining bonds into a free flight, which so long as undisturbed, continues (by the first law of motion) in an indefinite straight path in the direction of impulse. The strength of these two bonds—*adhesion* and *cohesion*—differing very widely in different substances, is thus measured by the amount of kinetic energy absorbed in overcoming them,—the so-called "latent heat" of fusion and of evaporation. In the case of ice, the strength of the molecular adhesion is considerably less than the sixth part of that of the cohesion.

We thus perceive how the most solid bodies—even at low temperatures—are exposed to surface evaporation without the opportunity of passing through the liquid state; since external molecules from the great irregularity of their short oscillations, must occasionally by the composition of motions from concurrent or immediately successive shocks, acquire a velocity transcending the bonds of cohesion, and thus escape entirely from the mass.

We accordingly learn by the kinetic theory of gases that the discrete or isolated molecules are flying about in all directions in straight lines until by encounters with other molecules (or with material barriers) their course is deflected. During the brief period of encounter (the disturbance of mutual encroachment), the trajectory becomes a minute hyperbola. From the infinite variety of possible impacts we also learn that each molecule must necessarily be constantly changing within very wide limits the direction, the velocity, and the length of its free excursions;—even when a perfect equilibrium of temperature imports that the mean kinetic energy of the entire system is constant and uniform.

It is important for us to bear in mind that this wondrous theater of continual intestine commotion does not present an example of a

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**Proceed. Am. Phil. Soc.* April 5, & May 17, 1844: vol. IV, pp. 56, 57; and 84, 85.

mechanical "perpetual motion:" the average velocity of any appreciable volume of gaseous molecules subsists only so long as no work is effected. By whatever amount any considerable number of flying particles impart motion to slower groups, or to a solid mass, by this amount do they reduce their own speed, and thus represent a diminished temperature. By whatever amount they receive any average increase of velocity from repeated impacts or from compression within a contracted inclosure, by this amount do they represent an elevation of temperature, at the expense of the bodies from which such additional energy is derived.

The Kinetic interpretation of the Laws of Gases.—It has been shown by Clausius that the number of collisions of a molecule in a given time is proportional to the mean velocity of all the molecules, to their number in a given volume, and to the square of the distance between the centers of two molecules when at nearest approach,* or at what has been called their dynamic contact. By the mathematical investigations of Krönig, Clausius, Loschmidt, and Maxwell, the foundations of a molecular physics have been successfully established; and the laws of gaseous action thus far experimentally ascertained, have been found to result deductively as the necessary consequences of the kinetic theory.

Thus the kinetic energy of any volume of molecules (which represents the temperature of the gas) being the product of molecular weight or mass by the mean square of the velocity, it follows that the relative rates of *effusion* and *diffusion* must both be inversely as the square roots of the masses,—that is of the gaseous densities;—the law of Graham.

It also follows that in the case of diffusion, by reason of the proportional retardations due to more numerous collisions from the presence of other gas, the coefficient must be lower than in the case of effusion.

In any mixture of gases, since from the mutual encounters of molecules of different mass, the average kinetic energy will be the same for all masses, or the mean squares of the velocities will be inversely as the respective masses, it follows that in different in-

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*"It is to Clausius that we owe the first definite conception of the free path of a molecule and of the mean distance travelled by a molecule between successive encounters." JAMES CLERK MAXWELL. (*Encyclopæd. Brit.* 1875: vol. III, p. 41.)

closures at the same temperature (*i. e.*, the same energy)—for equal pressures there must be the same number of impacts on any given area, or in other words that the same volume must contain the same number of molecules whether light or heavy :—the law of Avogadro and of Ampère.

And conversely, under the same conditions of pressure (or surface impacts) and of temperature (or kinetic energy), the number of molecules being the same, and the masses of the molecules being the only variable,—the densities of different gases must be proportional to their molecular weights or the masses of their individual molecules :—the law of Gay-Lussac.

Since the sum of the moving forces or the expanding power of the molecular excursions is directly proportional to their kinetic energy, it follows that the volume of a true gas under uniform pressure must be proportional to this energy, that is to the absolute temperature :—the law of Charles and of Dalton.

Since the same kinetic energy of the molecules must exert the same impulse, or the temperatures being constant, they must have a definite mean momentum, and each molecule must execute on an average the same number of impacts with the same energy, it follows that the pressure is directly proportional to the number of molecules ; or in other words that the volume of a true gas at any given temperature is inversely proportional to the pressure :—the law of Boyle and Mariotte. Or combining the last two laws, the volume of a gas multiplied by its pressure is directly proportional to the square of the mean molecular velocity, or the absolute temperature. The slight departure from the law of Boyle and Mariotte observed in most gases when compressed (the internal pressure being somewhat in defect,) indicates a small range of attraction between the molecules when brought close together.*

In addition to the external kinetic energy of the molecule due to its velocity of translation, it possesses an internal kinetic energy due to oscillation or rotation of its parts (its constituent atoms) ; and this internal energy—according to Clausius—tends to a constant ratio with the external energy. The amount of energy received or

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* "In the case of carbonic acid and other gases which are easily liquified, this deviation is very great. In all cases, however, except that of hydrogen the pressure is less than that given by Boyle's law, showing that the *virial* is on the whole due to *attractive* forces between the molecules." JAMES CLERK MAXWELL. (*Encyclopæd. Brit.* 1875 : vol. III, p. 39.)

expended by a gas in gaining or losing one degree of temperature (which is known as its "specific heat") is proportional to this constant ratio; and hence the specific heat of a gas is inversely proportional to the molecular mass;—that is to say, to the specific gravity of the gas:—the law of Dulong and Petit.

As the entire kinetic energy—molecular and atomic, is necessarily tending constantly to a dynamic equilibrium both with regard to any connected volume constituting a system, and with regard to any kinetic energy of the circumambient æther as well, there is a continual and mutual transfer of such energy:—the theory of exchanges announced by Prevost.

Mean Length of Molecular Excursions.—By a neat application of the calculus of probabilities, Clausius has determined that of the whole number of free molecular excursions in a given time, (in any large inclosure,) those having less than the mean length will be 0.6321; or nearly double the number of those having the mean length or exceeding it. He supposes that under ordinary conditions, the mean length of a free excursion of our air molecules is about sixty times the mean distance between them.

Maxwell has pointed out that three phenomena dependent on the length of the free excursions of gaseous molecules, furnish functions from which the mean length of such paths may be estimated; first, the rate of gaseous diffusion (or the bodily transfer of matter); second, the rate of diffusion of their momentum, or the degree of gaseous "viscosity" (dependent on the transfer and equalization of motion); and third, the diffusion of their kinetic energy or temperature, (the conduction of heat). In our atmosphere, under ordinary conditions (30 inches and 60° F.) the mean length of the molecular path is thus estimated at about the 1 ÷ 300,000 of an inch, or about one-sixth of a wave-length of yellow light.

The average molecular velocity of oxygen has been estimated at 1640 feet per second;* and of nitrogen (which constitutes about three-fourths of our atmosphere) at 1754 feet per second; while hydrogen molecules having but one-sixteenth the weight or mass of those of oxygen, would have under the same conditions, four times their average velocity, or 6560 feet per second. And thus while a

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* A velocity sufficient to carry the molecule vertically about eight miles high, if subjected to no resistance excepting gravitation.

molecule of oxygen would undergo about seven thousand million collisions in one second, a molecule of hydrogen among its fellows would undergo about seventeen thousand million collisions per second. It must be observed that the more violent the collisions of the molecules, the less is their tendency toward the cohesion of the liquid, or the adhesion of the solid form.

Probable Size of Molecules.—From various considerations it has been independently estimated by Joseph Loschmidt (1865), by G. Johnstone Stoney (1868), by William Thomson (1870), and by J. Clerk Maxwell (1873), that the effective size of the molecule is probably not smaller than the thousand-millionth of an inch, nor larger than three or four times this dimension; which is about the twenty-thousandth of a medium wave-length of light. Small as this dimension is, we may reflect that by what may be called the second power of our best microscopes, it would be easily visible,—supposing that light-waves were capable of optical efficiency at this degree of subdivision and amplification.

These estimates of molecular distances and magnitudes are of course but rough approximations; but they indicate at least the order of magnitude of very real things and agencies; and accepting them as probable, we may "compare small things with great" by saying that were the planet Venus brought within a distance from our Earth about one and a half times that of the Moon, this might represent the relative mean distance of two molecules of our atmosphere; at which separation (about fifty times their own diameters), they would probably count less than twenty million to the inch. In like manner the distance of Venus from our Earth at conjunction (as during the approaching transit of next Wednesday) would be relatively comparable to the length of a mean excursion of the molecules;—some 3,000 times their diameter. While a few of their longest free excursions would be comparable to the flight of the the same planet if carried from the Earth to beyond the orbit of Neptune.

The Relation of Molecular and Atomic Motions.—Returning again from this survey of molecular kinetics to the undulatory theory of light and heat, we may say that the true physical relation of radiation to conduction was first disclosed by the analytic spectrum,—that marvellous instrumentality which physics has presented to her daughter chemistry, as the most subtile and delicate of all her re-agents. From this method of observation we have learned that each of the elements when its molecules are shocked, rings out its own peculiar series of oscillations, as if by specially adjusted tuning-forks, each responsive only to the groupings of its own established periodicities. Newton first taught us that definite refrangibility in the spectrum signifies simply definite periodicity; and he also computed the data which determine the values of these periodicities.*

The known wave-lengths of different colored light divided by their known velocity of propagation, give us the inconceivable rapidity of from 390 to 750 billions per second,† as the number of atomic impulses transmitted by the æther and appreciated by the eye. Although this compass is somewhat less than an "octave," the entire range of the visible and invisible spectrum comprises more than three octaves. This extraordinary rate of vibration, no less than its remrakable uniformity, sufficiently establishes the fact that the motions of the *molecule* ceaselessly varying in velocity, and wholly irregular in length and frequency of excursion, take no part whatever in producing ætherial undulations. It is only to the constituent parts or ultimate *atoms* of the flying molecule that the rhyth-

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* NEWTON'S *Optics.* 1704: book II, part I, obs. 6. When shortly after his election to the Royal Society, Newton in a letter to the Secretary—Henry Oldenburg, (dated January 18, 1672,) proposed to offer a communication to that Society respecting his optical analysis, he spoke of it as "being the oddest if not the most considerable detection which hath hitherto been made in the operations of nature." (BIRCH'S *History of the Royal Society.* 1757: vol. III, p. 5.) Although a century and a quarter elapsed before the spectral lines were first detected by W. H. WOLLASTON, (*Phil. Trans. Roy. Soc.* June 24, 1802: vol. XCII, p. 365;) Newton was fully aware of the necessity of employing a very small hole or luminous image for obtaining a pure spectrum, and he pointed out that a narrow slit is still better; "for if this hole be an inch or two long, and but a tenth or a twentieth part of an inch broad, or narrower, the light of the image will be as simple as before, or simpler, and the image will become much broader." (*Optics:* book I, prop. IV.) For delicate observations Newton appears to have been compelled to rely on the services of an assistant; and thus he missed the consummation of his "oddest and most considerable detection of nature's operations"—the spectroscope.

† A *billion* (as is sufficiently indicated by the term itself) is the "second power of a million;" not (as is commonly taught in school-book numeration) the *third* power of a thousand, or the *second* power of an impossible number;—a surd

mic motions generating radiant light and heat must be referred We may thus picture to ourselves the monochromatic lines of the spectrum as exhibiting a second order of occult or insensible kinetics, in quality and range as different from and as much below the kinetics of the molecule, as this differs from and is below the kinetics of tangible masses.

The Origin of Atomic Motions.—With regard to the nature and origin of the atomic motions, it appears tolerably clear that they are primarily derived from the shocks of the molecules or systems of which they are the components; and that there is at every molecular collision a transfer or exchange of energy tending to equalize the internal momentum of pulsation with the external momentum of translation. The *primum mobile* is therefore the falling together of molecules under the influence either of gravitation, or of chemical affinity. While it is difficult to realize the precise manner in which molecular and atomic motions are re-distributed during the brief instants of impact, it appears in the highest degree probable that the atoms describe *elliptical orbits*, which may become circular, but never rectilinear. Were the atomic motions mere oscillations, it would appear unavoidable that under the stress of special impacts, some of them must occasionally be detached,—as in the case of molecular evaporation. But the *ultimate* molecule is unchangeable and "indivisible:"—held together in bonds incomparably stronger than those of hardest steel. And the loss of an atom may be regarded as an impossible catastrophe. Moreover, from the utter irregularity of direction in molecular encounters, obliquity of impact on the rapidly changing atoms, would appear almost a necessity: and hence would result as necessarily—elliptical paths of excursion.

In this constant play of atoms derived from repeated collisions, we must believe that these atoms are whirled in ever varying *rotations*—simultaneously with their orbital revolutions; but as these double motions form but parts of their common fund of kinetic energy, it is not probable that any special phenomena will ever distinctly reveal such axial motions;—unless indeed it be hereafter shown that *polarity* is the resultant of concerted directions of rotational or orbital axes, or of both.

The Amplitude of Atomic Orbits.—Of the actual or relative diameters of these orbits we are as ignorant as we are of the sizes of the atoms themselves. We may assume the amplitudes of the ætherial waves at their origin, to be a faithful transcript of those of the atomic excursions which generate them: and we must conclude the latter to be—even in the velocities of the highest incandescence, extremely small fractions of the length of the resulting waves. For although the amplitude of the atomic orbit represents but the square root of the brillancy, we may reflect that this latter form of energy presents an enormous range of variation. The light from Sirius—for example, supposing it to be in time twenty years in reaching us,—has but 1 ÷ 1,315,000 part of the amplitude of terrestrial sun-light; the amplitude being inversely as the distance travelled.* And there are among the visible stars doubtless some a thousand times more distant yet than Sirius.

According to the estimates of Wollaston, and of the younger Herschel, lights may vary in brilliancy forty thousand million times, representing a difference of amplitude of two hundred thousand times. To suggest some approximate idea of the form of such ætherial waves, we may liken them to earthquake waves transmitted across the surface of the ocean at the rate of six miles in a minute, which, while leaving on the tide-gage their registered amplitude of 15 inches, have for their length 150 miles: being accurately measurable waves presenting the ratio of one inch to ten miles.†

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*As the bright sun Sirius is considerably larger that our sun, and probably intrinsically brighter as well, the figure 1,315,000 (representing its distance in units of sun-distance) would be somewhat reduced as a measure of relative wave-amplitude. If the intrinsic splendor of the two suns be the same, the distant one has about 64 times the surface, or eight times the diameter of our own. The probability of greater density in the former—from greater mass,—is offset by the probability of correspondingly higher temperature. Hence assuming the mean densities to be nearly the same, the gravitative pressure of equal gaseous masses on the photosphere of Sirius, would probably be in the neighborhood of eight times that upon our sun, or some 200 times that upon the surface of our earth.

† The earthquake which destroyed the city of Simoda, in Japan, in December, 1854, generated such a system of waves, which crossing the Pacific Ocean, over a distance of 4,500 miles, in the time of 12 hours and 36 minutes, left their record on the tide-gages of the Coast Survey, at San Francisco, as having a maximum amplitude of 18 inches. The height of the ocean wave at its origin was, of course, much greater than this. (*Smithsonian Report* for 1874: pp. 216, 217.—A Lecture "On Tides," by Prof. J. E. Hilgard, (at present Supt. of Coast Survey,) delivered before

Smallness of Atoms.—The extreme minuteness of the atoms is evidenced not alone by the necessary limitations of their orbital excursions under ordinary conditions, and by their inconceivable rapidity of oscillation, but even still more strikingly by the vast number of molecules which may be chemically combined and compacted within the volume of an elementary molecule,—still observing the law of Avogadro.

From such considerations we may infer that the dimensions of the ultimate atoms are probably as much below that of the composite molecule, as this is beneath a visible magnitude: or in other words, that were the molecule an object to be seen, the highest power of our best microscopes would utterly fail to detect its constituent atoms.

The Constancy of the Atomic Periods.—We have learned from the fixity of the spectral lines (whether luminous or dark) that what may be called the tones or pitches of these resonant particles are very accurately maintained through an enormous range of amplitude; that is, that the respective periods of the atomic orbits (infinitesimally brief as they appear to our slow-moving thoughts) are quite unaffected by their radii, or their rates of velocity. The evidence of these uniformities of period in descending temperatures is found in the stability of gaseous absorption lines under all degrees of cold producible; these lines remaining dark when taking up the motion of the incandescent back-ground, simply because the amplitude of the oscillation is not sufficient on the whole to impress our sense of vision. And although at very high temperatures both the number and the distinctness of the spectral lines may be considerably affected, their position (as long as visible) is not at all disturbed. That new lines should appear at increasing temperatures is not surprising, since in every case a certain width of atomic play is required to affect the eye. But that under such circumstances pre-existing lines should disappear,—as has been established by the researches of Dr. J. Plücker and Dr. J. W. Hittorf,*—so

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the American Institute, Jan. 27, 1871.) It is instructive to reflect that a wave line of this order (representing an ætherial undulation)—executed by the most skillful draftsman or engraver, on any scale whatever, or with any microscopic appliances, could not be distinguished by any process of direct instrumental measurement or verification from a perfectly straight line.

**Phil. Trans. Roy. Soc.* Memoir read March 3, 1864: vol. CLV, pp. 1–29.

as to produce an entirely different spectrum, is not so easily explained. The suggestion of a disruption or disassociation of the atomic flight by centrifugal force is negatived by the fact of perfect restoration of the orbit under uniform conditions. Nor does the hypothesis of a resolution of the elementary molecules into still more elementary types, (which seems to have gained some favor,) render the physical conception of the phenomena in any respect more simple. In particular cases a precise equalization of the energies of emission, and of absorption in surrounding heated gas, might effect a neutralization and complete obliteration of one or more of the lines. And it is conceivable that a certain increase of amplitude in the ætherial wave may (as in the case of its length) cease to be recognized by the optic nerves.

The law of Atomic Orbits.—The conception being thus presented to us—of a particle moving in an elliptical or circular orbit of constant period, irrespective of the length of the radius-vector, or of the velocity, (a condition so wholly unlike the gravitative orbits of planets, observing the laws of Kepler,) what is the dynamic interpretation of such a system? This problem has been anticipated by the genius of Newton, who in his Mathematical Principles of Natural Philosophy has demonstrated the *imaginary* case,—"if the periodic times are equal, (and the velocities therefore as the radii,) the *centripetal* forces will also be as the radii." * A law of force *increasing* directly with the distance (as in the extension of an india-rubber, or of a helical steel wire spring,) is undoubtedly a very remarkable one: but whatever its range of action, it will manifestly within that range, secure the atom from all possibility of detachment.

From the perfect uniformity both of chemical and of spectroscopic indications, whether in the smallest or the largest mass of molecules,—from whatever source obtained, we are forced to conclude that the molecules of any simple gas are absolutely similar. Whether we analyze a drop of petroleum or distill an insect or a

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* *Newton's Principia.* 1687: book I, sect. II, prop. 4, corol. 3. A very beautiful illustration of this orbit is presented by the conical pendulum, when the length of the suspension is very great relatively to the ranges of excursion of the ball, so that an ellipse or different circular orbits shall lie sensibly in the same plane. Another similar example is furnished by the orbits of the balls of a parabolic "governor."

plant, whether we decompose water from the Indian ocean or from Arctic snow-flake, whether we inspect with curious eye the light from sun, or star, or from remotest nebulæ at opposite confines of the heavens, we find in the spectrum of hydrogen the same fixed lines; —assuring us that these are truly the reverberations of periods incessantly repeated alike in every molecule of this particular element.* Taking this—the lightest of all known molecules, (Prout's fundamental unit of chemical equivalency,) we have within the single molecule the widely separated lines of four distinct periodicities, or atomic orbits :—the red line "C" (α) of 456 billion revolutions per second,—the greenish blue line "F" (β) of 615 billion revolutions,—the blue line near "G" (γ) of 689 billion revolutions, and the violet line "h" (δ) of 729 billion revolutions. As no form of either reciprocating or orbital movement could possibly be maintained without an equal and opposite re-action, there must necessarily exist here *at least* eight independent atoms. But it seems wholly improbable that each of these systems of motion should comprise but a single couple of atoms: and it is still more improbable that either these periods, or even the numerous additional ones disclosed in the secondary spectrum of hydrogen, represent all the atomic motions within its molecule, in view of the necessary imperfection of the optical record, and the fact that this embraces less than the third, and possibly not more than one-fourth of the whole actinic spectrum.

Physical Complexity of the Molecule.—We are therefore justified in believing that the most elementary of chemical molecules is a wonderfully complex system, comprising an unknown number of constituent units, held together by dynamic bonds whose nature we can neither guess nor conceive; and thus the atom of Newton and of Dalton has been carried downward far beyond the horizon of action at which they had imagined it—probably even to a second order of diminished magnitude.

The relations between the translatory motion of the integral gase-

* "The same kind of molecule—say that of hydrogen—has the same set of periods of vibration,—whether we procure the hydrogen from water, from coal, or from meteoric iron ; and light having the same set of periods of vibration comes to us from the Sun, from Sirius, and from Arcturus." J. Clerk Maxwell. (*Encyclopæd. Brit.* 1875: art. "Atom," vol. III, p. 48.)

ous molecule and the internal revolutions about its center of inertia present a new difficulty of conception as to the constitution and action of the ætherial medium. For while the molecule (a mere cluster of atoms) is supposed to be flying freely about without obstruction or retardation, (in order to fulfil the laws of Charles, and of Boyle and Mariotte,) the individual atoms themselves experience a very considerable resistance to their revolutions;—the precise measure of which resistance is the kinetic energy absorbed and expended by ætherial undulations. And so it results conversely, that if the motion of the æther-waves exceeds that of the molecular atoms exposed to their action, the difference of momentum is taken up by the latter, and through exchanges at molecular encounters is equalized by corresponding increments of velocity in the molecules themselves. Such is the process in all terrestrial heating by solar radiation. And this brings directly to view one important distinction between heat and light,—to wit, that while both are *radiated* in precisely the same manner, "conduction" has no existence in optical action. The only approach to any such effect in light, is found in the obscure and puzzling phenomena of fluorescence and phosphorescence, and of animal luminosity. In the case of *heat* we may have a transfer by radiation—always the result of atomic motion, by conduction—always the result of molecular motion, or by convection—always the result of mass motion.

During the time of a mean free excursion of gaseous molecules at the temperature of incandescence, the atomic periods would permit from ten to twenty thousand revolutions. But from the great amount of energy absorbed by the æther it does not appear probable that any considerable portion of such orbital movement can continue throughout the interval of a mean free path. If then it be true that in a majority of the molecular excursions the whole internal atomic motion is absorbed and destroyed, to be renewed again only by the succeeding collisions, there is a constant drain upon the molecular momentum; a condition which must alike prevail, however low may be the temperature of the gas. While there is thus a constant tendency to equalization of the orbital atomic momentum and the rectilinear molecular momentum, the total kinetic energy of the former has been estimated at not more than from two-thirds to three-fourths of the kinetic energy of the latter.

It is in the gaseous spectrum alone—that is, in the atomic motions of discrete molecules, that perfect uniformity of period, or as we may call it, perfect purity of optical tone is to be observed. With any considerable compression of a gas, that is, with any great crowding together of the molecules and shortening of their mean free excursions, whereby the increased frequency of collision is constantly disturbing the atomic orbits before their motions can be fully absorbed by the æther, there will result a momentary hastening or retarding of the normal periods, giving to the spectral lines an increased breadth or wider range of refrangibility. And when the condensation reaches that of the "liquid" or "solid" condition, preventing all free excursion, the incessant agitation of the atoms results in a universal clang or optical "noise," in which all uniformity of period seems lost, and perturbations of all possible degrees present us with the discord and confusion of a perfectly continuous spectrum.*

The Chemist has taught us that in numerous cases the normal molecule is divided into sub-molecules. Thus the relations of the compounds of arsenic, as well as of those of phosphorus, indicate the composition by half molecules of these elements; the ratios of the so-called "sesqui-salts" point to the same result; the allotropic condition of oxygen—called ozone—is formulated as having the equivalency of one and a half molecules; one molecule of aqueous vapor (and therefore of water) consists of one molecule of hydrogen and a half molecule of oxygen; two molecules of ammonia are resolved into three equal molecules of hydrogen and one of nitrogen; and a single molecule of hydrogen united with a single one of chlorine will form two molecules of hydrochloric acid,—each containing an equal division of the two constituents. Although this dichotomy of the molecule is suggestive of binary systems in some way specially linked together and at the same time susceptible of various re-arrangements, yet the fact remains that these divided molecules are still extremely complex physical systems,—apparently identical in constitution and construction, and therefore undistinguishable from each other. The Chemist however adhering too literally to the phrase of Dalton, has neglected the obvious import

*J. Clerk Maxwell has felicitously compared the atomic oscillations producing a continuous spectrum, to the clang of a bell "on which innumerable hammers are continually plying their strokes all out of time, [when] the sound will become a mere noise in which no musical note can be distinguished." (*Encyclopæd. Brit.* 1875: art. "Atom:" vol. II, p. 43.)

of the spectral lines, and speaks familiarly of the *diatomic* molecule.* It is true that the "atom" is properly a physical and not a chemical unit, since it can never be reached by any possible reactions of affinity or of decomposition. But if the term is to be still retained in chemical nomenclature, it should always be understood in its merely etymological sense of the "undivided," and not in its more popular sense of the uncompounded.

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3. *The Fallacy of Kinematic Theories.*

After this rather labored effort to approximate to some definite conception of the physical nature of the two types of invisible or elementary motion—displayed in the atomic revolutions or oscillations generating radiant undulations of the æther, and in the molecular flights and encounters generating the thermo-dynamic pressures of gaseous fluids,—let us consider what countenance these forms of motion may be supposed to lend to a kinematic theory of universal force.

It is important here to notice that by experiments on the sensible vibrations of bodies,—as of tuning-forks and pneumatic diaphragms,—translatory motions of approach and recession have been produced in light bodies. The "attractions" or "repulsions" have been shown to depend on the amplitudes of the oscillation, and the ratio of the wave-lengths to the surfaces of action; as also on the symmetrical concurrence or reversal of the phases of vibration in two confronting systems.†

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* Prof. George F. Barker in his excellent presidential address before the Chemical Section of the American Association at Buffalo, on the theme—"The Molecule and the Atom," referring to the constitution of hydrochloric acid, repeats the common view: "hence a molecule of hydrogen is composed of two atoms." (*Proceed. Am. Assoc.* August, 1876: p. 95.)

† Dr. Jules Guyot. *Des Mouvements de l'Air et des Pressions de l'Air en Mouvement.* 8vo. Paris, 1835.

Prof. Frederick Guthrie. "On Approach caused by Vibration." *L. E. D. Phil. Mag.* Nov. 1870: vol. XL, p. 354. (From his tuning-fork experiments, the author ventures the bold and startling induction: "In mechanics—in nature—there is no such thing as a pulling force.")

Prof. C. A. Bierknes of Christiania, Norway. Hydro-dynamic experi-

Irrelevancy of a Vibratory Hypothesis.—The first remark that occurs to a thoughtful student of these well-known phenomena of hydro-dynamics, (upon which narrow basis some enthusiasts have erected so wide a framework of induction,) is that between these resultant motions and any actions traceable in molecular physics,—(unless possibly in particular habitudes of electricity and magnetism,) there is not even a rough analogy. And the next and most obvious suggestion is that the absolute precedent condition of any reciprocating action whatever is the presence of the very qualities—*cohesion* and *elasticity*—for the production of which such reciprocating action is invoked. The essential powers and characteristics by which alone either atomic revolutions or molecular impacts are for an instant rendered possible, are the inherence of never-slumbering forces of attraction and repulsion. A vibratory particle (assumed by the kinematist for the avoidance of incomprehensible attributes,) is itself the most astounding—the most unrealizable in scientific thought, of all physical concepts. No atom can perform an oscillation or a revolution, or follow any other path than a straight line—excepting under the coercion of other atoms attracting and repelling. The first law of motion is that of perfect continuity both in amount and in direction. A shuttlecock rebounding in the empty air, would not be more conspicuously a dynamic solecism and impossibility than the kinematist's "vibratory particle."

Those therefore who in their backward search of causation would assign the origin of force to some incomprehensible æther action, have no more warrant from experience, induction, or reason, than those less cultured philosophers who taking "the unknown for the wonderful" habitually refer each unfamiliar phenomenon (with easy faith)—to "electricity."*

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ments on vibration. *Nature.* Aug. 18, 1881: vol. XXIV, p. 360; and Jan. 19, 1882: vol. XXV, pp. 272, 273.

Also a modification of the experiments of Prof. Bierknes, by Mr. AUGUSTUS STROH: (in air instead of in water.) *Nature.* June 8, 1882: vol. XXVI, p. 134.

* "There are not wanting those who appear very much disposed to say that the conception of *force* itself—as part and parcel of the system of the material universe—is superfluous and therefore illogical. - - - Having come to regard heat, light, electricity, as modes of motion, they seem to consider force itself as included in the same category, and think there is

Instability of a Vibratory Hypothesis.—But the kinematic embarrassment is not concluded here. Supposing the marvellous feat accomplished of effecting a rotatory resilience which should simulate in direction and amount the facts of observation, how far would such accordance justify its acceptance as the true and sufficient account of the molecular behavior, in the light of the great established principle of the conservation of energy? As a necessary corollary of this great generalization we know that every system of atomic or molecular oscillation, undulation, and impact, is directly amenable to material disturbance and to the precise mechanical equivalents of kinetic deflection, arrest, and neutralization. But as regards the fundamental qualities of atomic or molecular attractions, repulsions, and elasticities, no such disturbance, or aberration, or interference, is for an instant possible. And these fundamental qualities are persistent, and permanent, as well as unchanging. Hence the countless balls sustained in place by countless fountains, must never be permitted to decline or swerve from their required positions. Every bent spring, every loaded beam, every sustaining rope and chain and cable must therefore have expended upon it a ceaseless rain and battery of impact or of wave propulsion. Nay every solid, every liquid, must be held in its tenacious consistency by the external coercion of a never resting dynamic bombardment. In what manner is the inexhaustible supply of kinetic energy supposed to be obtained? What is its source?—and where is its escape? Why is it that the incessant and violent collisions brought into play

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'reason to believe that it depends on the diffusion of highly attenuated matter through space.'" Sir JOHN HERSCHEL. ("On the Origin of Force." *Fortnightly Review.* July 1, 1865: vol. I, p. 436. And *Familiar Lectures,* [etc.] 12mo. London, 1866: art. XII, p. 462.)

The learned physical professor in the University of Edinburgh sees "reason to believe that *force* depends upon the immediate action of highly attenuated matter diffused throughout space." (*North British Review.* February, 1864: vol. XL, p. 22,—of Am. edition. And Prof. P. G. TAIT's *Sketch of Thermo-dynamics.* 8vo. Edinburgh, 1868: chap. I, sect. 3, p. 2.)

And the no less learned physical professor in the University of Cambridge, thinking it irrational to ascribe the occult quality of *elasticity* to any sensible molecule, finds no difficulty in relegating this property to the æther. (*L. E. D. Phil. Mag.* June, 1866: vol. XXXI, pp. 468, 469. And Prof. J. CHALLIS's *Principles of Mathematics and Physics.* 8vo. Cambridge, 1869: pp. 316, 358, and 436.)

under this dynasty of percussion, do not speedily raise the temperature of all coherent bodies to a fierce and glowing heat?*

And this brings us face to face with the great radical—incommensurable difference between "force" and *energy*,—that the function of the former is attended with no expenditure, and is capable of no exhaustion. The truth of this bold asseveration has been tested again and again by every expedient which the most skillful and ingenious kinematists have been able to devise for its question, without the suspicion of impeachment; and it remains to-day, one of our strongest and best assured inductions.

On this broad platform rests the issue between kinematism and dynamism,—that the former inevitably contravenes and destroys that bulwark of modern physics—*the conservation of energy*; while the latter is its only support and its necessary foundation. Without the indestructible—unwasting—tensions of molecular attraction and repulsion, it lies beyond the scope of human ingenuity to devise or imagine a conservative system.

The fundamental—the inherent and incurable weakness of every attempt to supersede "force" by motion is betrayed in this,—the inadmissible supposition of a world held together only by the infinite expenditure of *work*, for whose existence no provision is devised, and for whose maintenance no motor can be suggested or conceived.†

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* Referring to the steady maintenance of material tensions by supposed ætherial motions or vortices, J. CLERK MAXWELL truly remarks: "No theory of the constitution of the ether has yet been invented which will account for such a system of molecular vortices being maintained for an indefinite time without their energy being gradually dissipated into that irregular agitation of the medium which in ordinary media is called heat." (*Encyclopædia Britannica.* 9th ed. 1878: art. "Ether:" vol. VIII, p. 572.)

† "Taking such a system in its entirety (where force exists not), there is no possibility of its reproduction. There is therefore a necessary and unceasing drain on the *vis viva* of such a system. Everything which constitutes an event, whatever its nature, exhausts some portion of the original stock. Such a system has no vitality. It feeds upon itself and has no restorative power." Sir JOHN HERSCHEL, ("On the origin of Force."—*Fortnightly Review.* July 1, 1865: vol. I, p. 437. And *Familiar Lectures,* [etc.] 1866: art. XII, p. 465.)

"It is remarkable" observes J. CLERK MAXWELL, "that of the three hypotheses which go some way toward a physical explanation of gravitation, every one involves a constant expenditure of work." (*Encyclopæd. Brit.* 9th ed. 1875: art. "Attraction:" vol. III, p. 65.)

It is the inversion of the sequence taught us by all sufficiently observant experience, that motion of any kind or form is ever the product of force, and can never be its parent.

Inadequacy of a Vibratory Hypothesis.—But after all this lavish exercise of creative power and ingenuity,—this prodigal expenditure of kinetic energy,—how surprising to find the notable invention wholly incompetent to produce the observed phenomena. Cohesive force (for example) apparently incapable of exerting any attractive power whatever beyond the range of a single layer of molecules, that is beyond the distance of perhaps the five hundred millionth of an inch from its center of action, yet exercises for an exceedingly small space within that distance a holding strength many thousands of times greater than the all-pervading power of gravitation. By what form of undulation, oscillation, or impulsion, shall we represent the tenacity of a steel wire sustaining a pull of 300,000 pounds to the square inch beyond the limits of perhaps the thousand-millionth of an inch between its molecules, yet exerting within that limit an insuperable repulsion, and again at double the distance another range of repulsion, so far resisting all human efforts, that the nicest and closest approximation of the severed ends of the wire shall fail to develop the attraction of an ounce or single grain?* By what form of partial differential equation, shall this sudden and absolute discontinuity of function be expounded? Nay rather, by what hallucination of metaphysical assumption have intelligent men been induced to waste useful time and ink and paper, on the chase of the *ignis-fatuus* of cohesive undulation or percussion?

The Authority of "Sensible" Impressions.—But it is insisted that "the principle of deriving fundamental conceptions from the indications of the senses does not admit of regarding any force varying with distance as an essential quality of matter, because according

* Prof. CHALLIS thinks "the ultimate atoms of glass are kept asunder by the repulsion of æthereal undulations which have their origin at individual atoms," and "it may be presumed that this atomic repulsion is attributable to undulations incomparably smaller than those which cause the sensation of light." (*Principles of Mathematics and Physics.* 1869: p. 456.) But the luminiferous vibrations are themselves *atomic*. What lower order of atom is then to be appealed to in support of this fanciful and inept hypothesis?

to that principle we must in seeking for the simplest idea of physical force have regard to the sense of *touch*."* Let us inquire then what is taught us by tactile experience with regard to the philosophy of physical contact. In the celebrated experiment by which Newton first measured the wave-lengths of light from the colored rings which yet bear his name, he found that on placing a piece of clean plate glass upon the convex surface of a large lens, a very considerable pressure was required to exhaust the series of outcoming interference fringes and to exhibit the central black spot. Professor Robison estimated that a pressure of at least one thousand pounds to the square inch was necessary to effect this approach to a mathematical contact between the two glasses.† And yet even with this very close and perfect physical contact it is shown that at the first appearance of the black spot between the glasses, they are still separated from actual or mathematical contact by the space of the 250,000th of an inch.

Material Contact not Absolute.—Supposing it were desired to directly communicate a push or a pull through the distance of seven miles, a perfectly straight steel bar (properly supported on friction rollers through that space) would probably be as efficient a *mechanical* means for the purpose as could well be suggested. And yet the blow of a suitably heavy hammer struck upon one of its ends would

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* Prof. JAMES CHALLIS. *Principles of Mathematics and Physics.* 1869: p. 358.

†*A System of Mechanical Philosophy.* By Prof. JOHN ROBISON: vol. I, sect. 241, p. 250. Dr. YOUNG remarks on this: "Hence it is obvious that whenever two pieces of glass strike each other without exerting a pressure equal to a thousand pounds on a square inch, they may effect each other's motion without actually coming into contact. Some persons might perhaps be disposed to attribute this repulsion to the elasticity of particles of air adhering to the glass, but I have found that the experiment succeeds equally well in the vacuum of an air-pump. We must therefore be contented to acknowledge our total ignorance of the intimate nature of forces of every kind." (*Lectures on Natural Philosophy.* 2 vols. 4to. London, 1807: lect. III: vol. I, p. 28.) And Prof. J. CLERK MAXWELL says to the same effect: "We have no evidence that real contact ever takes place between two bodies, and in fact when bodies are pressed against each other and in apparent contact, we may sometimes actually measure the distance between them, as when one piece of glass is laid on another, in which case a considerable pressure must be applied to bring the surfaces near enough

require very nearly two seconds for its transmission and delivery at the opposite end. Or if we reduce our steel punch to the more manageable length of (let us say) one foot, then the blow received by it from a hammer, and the blow given out by it at the other end, will be separated by the interval of the 18,000th part of a second. Assuming the actual approach of the hammer face to the end of the steel punch at the instant of impact to be the millionth of an inch, we may even compute the interval of time elapsing between the delivery of the blow by the hammer and its reception by the steel punch, at the $1 \div 216000,000000$ of a second; an interval of time real enough and long enough to permit the atoms of the iron molecules to execute from 1800 to 3200 of their normal oscillations or orbital revolutions. By thus considering what is really signified by physical contact and impact, we find it to be something quite different from what the kinematist would suggest by his appeals to "the sense of touch."

The unlucky boy when struck in the face with a ball, or wounded in his finger with his jack-knife, may well refuse to be comforted by the assurance that neither the ball which bruised his face, nor the blade which penetrated and severed the capillary vessels of his finger, ever approached within the millionth of an inch of his flesh, or probably within double that distance from it. But the philosopher who aspires to construct a theory of universal force from the inductions of experience, should at least sufficiently develop his intellectual vision to avoid accepting coarse and external resemblances as evidences of co-ordinated derivation, or adopting the unanalyzed impressions of unobservant consciousness as the revelations of axiomatic truth.

Action at a Distance.—But here our investigation is undermining the very corner-stone of the kinematic system,—the repudiation of all static energy, the alleged fundamental absurdity of any mechanical action at a distance. That "a thing can no more act *where it is not* than *when* it is not," is a plain dictum of common-sense.* Even the provisional admission of such a supposition is

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to show the black spot of Newton's rings, which indicates a distance of about a ten-thousandth of a millimeter." (*Encyclopœdia Britannica.* 9th ed. 1875: art. "Attraction:" vol. III, p. 63.)

* Prof. JAMES CROLL believes that "No principle will ever be generally received that stands in opposition to the old adage 'A thing cannot act

in violation of the canons of sound thought, and is contradictory of one of the most obvious aphorisms of logical metaphysics. Whatever our refinements as to the real nature of physical contact (it is said), this action is none the less a fact of constant and familiar occurrence, and is the actual method of kinetic transference manifested to our every-day observation. If we wish to give a billiard ball a definite motion in a specific direction, we do not whistle to the ball, or attempt to "psychologize" it; we strike it with a cue. Is it conceivable that "mere brute matter" should be more "spiritual" than man himself?

As these popular and taking propositions involve purely a question of physical fact, their truth can never be decided by any introspections of the consciousness, by any deductions from the "*ego cogito*," or by any disquisitions on "the theory of conception." As a question of fact, the final settlement of the nature of material *action* is to be reached only by the converging inductions of a critical *experience* (aided and enlightened by every expedient of refined investigation), and by the necessary inferences from such experience. It is very certain that a material body must exert its action—either at *some* distance, or at *no* distance, that is by absolute and perfect contact. Have we at present the means of intelligently probing this sharply defined issue? *

Action at no Distance.—It is a well-established principle, or rather fact, of dynamics that finite time is required for the production of

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where it is not.'" (*L. E. D. Phil. Mag.* December, 1867: vol. XXXIV, p. 450.) And George Henry Lewes is fully persuaded that "Action at a distance (unless understood in the sense of action through unspecified intermediates) is both logically and physically absurd." (*Problems of Life and Mind.* 1875: vol. II, appendix C, p. 484.)

* Dr. Oliver J. Lodge has remarked: "I venture to think that putting metaphysics entirely on one side we may prove in a perfectly simple and physical manner that it is impossible for two bodies *not* in contact to act directly on each other:" and he defends the position by the argument, that since action and re-action are equal and opposite, and since "work" done upon one body is equal to the "energy" so expended by the opposite body, "the distances must be equal but not opposite; that is, the two bodies must move over precisely the same distance and in the same sense: which practically asserts that they move together and are in contact so long as the action is going on." (*L. E. D. Phil. Mag.* January, 1881: vol. XI, pp. 36, 37.)

any finite velocity, or of any finite change in velocity. Only an infinite force could generate motion instantaneously, and this acting for any finite time would produce an infinite velocity. Now the impact of a moving body upon a body at rest, must occur in the absolute instant of contact. No motion could be transmitted *before* contact, for this would be the chimera—*actio in distans.* No motion could be transmitted *after* contact, for then the impinging body could evidently have no more motion than the body impinged upon. And no motion could be transmitted at the *instant* of contact, for this occupies but an infinitesimal of time. But if no motion could be communicated either before, or at, or after contact, it is very clearly established that no motion whatever could possibly be derived from impact pure and simple. This conclusion—applicable alike to an atom or a planet—remains equally unassailable whatever be the magnitudes of the bodies in action.

We are thus strongly reminded of Zeno's celebrated paradox as to the impossibility of motion. For while the kinematist very positively assures us that action at a distance is a metaphysical impossibility, the dynamist assures us no less positively that action at no distance is a demonstrated physical impossibility.* But if mere kinetic energy cannot be transferred excepting through a vacant

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* This position is so forcibly stated by Prof. JOSEPH BAYMA in his able Treatise on Molecular Physics, that a quotation from that work seems here especially appropriate. "Finite velocity cannot be communicated in an indivisible instant, as we have seen. - - - Nor can the demonstration be evaded by having recourse to the *multitude* of points among which the contact would be supposed to take place. For - - - if each individual point of matter only acquires an infinitesimal velocity (*vdt*), the whole multitude will acquire only an infinitesimal velocity ; that is, there will be no motion caused at all. Nor can it be said that the motion is communicated by means of a *prolonged* contact. A prolonged contact is impossible unless the velocities have become equal at the very commencement of the contact. Therefore if velocity were communicated by the contact of matter with matter, it would have to be communicated in the very first instant of the contact, not in its prolongation. - - - Therefore *distance* is a necessary condition of the action of matter upon matter. Therefore the contact between the agent and the object acted upon is not material but *virtual*, inasmuch as it is by its active power (*virtus*), not by its matter, that the agent reaches the matter of the object acted upon." (*Molecular Mechanics.* 8vo. London, 1866 : book I, prop. 3, pp. 14, 15.)

space, *à fortiori* must static "force" require distance as the indispensable condition of its action.

So much therefore for the vaunted dictum of "common-sense:" and so much for the antagonistic dictum whose "absurdity is so great that no man who has in philosophical matters a competent faculty of thinking can ever fall into it!" * And this absurd—this incomprehensible—this inconceivable proposition—that matter is capable of acting *only* where it is not, is proved by the incontestible conviction of reason to be a primary and necessary truth: and the wondrous scholastic dogma resisting it—supposed the sacred oracle of a mysterious intuition,—is but the detected impostor of a crude induction.

True meaning of Contact Action.—To confirm however the explicit deductions of mechanical theory by the verifications of actual experience, let us examine more closely the true character of that transmission of energy by impact which to the kinematist appears to furnish so simple and so obvious an explanation of "force." Taking the most elementary example of the *vis a tergo*, let us suppose two precisely similar billiard-balls—A and B—on the perfectly smooth surface of a frozen lake, B at rest, and A rolled toward it in the direct line joining their centers of inertia. The familiar result that A is brought to rest by the collision, and B continues the motion in the same direction prolonged, will be fluently explained by the kinematist as a mere case of conservation, or the persistence of motion,—which evidently passes at the instant of contact directly from A to B, like an electric charge.

Overlooking—first, the fallacy of a finite velocity passing into a body instantaneously (already controverted), there is a second difficulty, that *motion*—defined as a change of position in a body, or the occupation of successive portions of space by a body,—cannot exist out of the body, cannot therefore pass through the confines of the body. But admitting for the moment both these possibilities,—in the third place, how could the ball A part with *all* its motion to

* This inconsiderate utterance of NEWTON in his oft-quoted "third Bentley letter," (Feb. 25, 1693,) was wholly repudiated by him a quarter of a century later, when with a graver wisdom he asked the question: "Have not the small particles of bodies certain powers, virtues, or forces, by which they act at a distance?" (*Optics*. 2d edition. 1717: book III, query 31.) A recantation never cited by the kinematist.

another ball no larger than itself? The two possessing the same inertia, why did not *A* expend just half its motion on collision with *B*, giving the latter its equal share; and thus conserve the original momentum by the double mass moving conjointly with half the velocity? This very simple question—it is safe to affirm—can never be answered by any principles of the science of kinematics.

By the principles of dynamics, these three queries admit of a very satisfactory solution. At the moment of physical contact between the two balls, (there being still an assignable space between them,) their approaching surfaces commence mutually to encroach upon a powerful molecular repulsion crowding back and compressing more closely together vast multitudes of resisting layers of molecules on either side, until their combined pressure gradually absorbs and destroys the momentum of *A*, while simultaneously exerting an equal stress on the inertia of *B*. And thus by the necessary equality of action and re-action, the centers of inertia of the two balls pass successively through the same reversed phases of approach and recession during the brief finite interval of physical contact, attaining a relative velocity of separation precisely equal to that of the encounter: the deformations of the balls, or their compressions, being as the squares of the absorbed velocity, and their energy of recovery being as the square roots of the restored velocity. So far therefore from the original motion of *A* being transferred to *B* (as often loosely stated), it really passes continuously through every stage of decline to actual rest; and a new motion commencing from zero is gradually started in *B*, by the continued application of an elastic pressure, during a finite time.

To take one more example in illustration of the impossibility of action *at no distance*, let us suppose an ivory ball weighing one ounce to be centrally struck while at rest by another ivory ball weighing four ounces, and moving with a velocity of 10 feet per second. If we were to ignore the "occult" force of *elasticity*, and neglect the difficulties already exposed, kinematics would give the simple result of a common velocity of the two balls after impact, of 8 feet per second: 4×10 being equal to 5×8. But this is not what would happen. We should find instead that the four-ounce ball has its velocity reduced to 6 feet per second, while the one-ounce ball takes up a velocity of 16 feet per second;—just *double* that it should have taken were action at no distance a natural possibility: the latter ball absorbing (so to speak) the whole velocity and three-fifths more, while the former has expended two-fifths of its original velocity.

Here then is presented a new difficulty on the kinematic theory. In what possible manner can a body moving at a definite rate impart to another body *by simple impact* a velocity considerably higher than that possessed by itself? By kinematics, this question also must remain forever unanswered. By the established principles of dynamics—there being no actual or mathematical contact of the two balls,—the static energy of their combined compressions or repulsions acquired during the time of their physical contact precisely equals the kinetic energy of impact; and consequently on resilience refunds a precisely equal kinetic energy of separation;—to wit, a relative velocity of 10 feet per second.

Impossibility of Action at no Distance.—It turns out therefore when we examine very slightly beneath the surface of "sense information," that *impulsion* (so perfectly obvious and intelligible to the kinematist) is itself a very notable example of the ultra-sensible and recondite: *—that the vaunted philosophy of "the sense of touch" is no more able to escape from the dominion of the unseen, the hidden, the enigmatical, in causation, than is the dynamism which is held to be so superficial, credulous, and undiscerning.

And this mysterious but necessary principle of all dynamics reaches far back of the imagined cases of corporeal contact in collisions,—even to the intimate structure of the densest material; †

*As acutely remarked by the eminent mathematician—James Ivory: "A little reflection is sufficient to show that in reality we have no clearer notion of *impulse* as the cause of motion, than we have of *attraction*. We can as little give a satisfactory reason why motion should pass out of one body into another on their contact, as we can why one body should begin to move, or have its motion increased, when it is placed near another body. - - - If then we are apt to think that impulse is a clearer physical principle than attraction, there is really no good ground for the distinction; it has its origin in prejudice." (*Encyclopædia Britannica.* 8th ed. 1854: art. "Attraction:" vol IV, p. 220.)

"When the Newtonians were accused of introducing into philosophy an unknown cause which they termed *attraction*, they justly replied that they knew as much respecting attraction as their opponents did about impulse." Dr. William Whewell. (*History of Scientific Ideas.* 1858: book III, chap. IX, sect. 8: vol. I, p. 278.)

† There is good reason to think that absolute contact never takes place in the component parts of the hardest and most compact solid bodies." James

for it is demonstrable that the component molecules and atoms of the hardest steel are far from being in contact; that carbon molecules have room enough—even when crystal-bound in diamond—to freely execute the oscillations constituting its varying temperature by constant exchanges, and to so alter their relative excursions as to represent the changed specific gravity due to varying temperature.

The conclusion reached, we would wish to express in the most emphatic and unequivocal terms:—that in all nature we have as yet been furnished with no example of absolute contact action;—that "action at no distance" is sheer physical *impossibility;*—that in utter scorn of venerable scholastic axioms, matter is forever incapable of influencing other matter in any manner whatever or in any degree whatever—*excepting* "where it is not!" And thus the paradox of Zeno receives its solution by the thorough confutation of kinematism at every point—inductive or deductive,—theoretical or experimental.

"*Occult Qualities.*"—And now we are fully prepared to encounter the portentous arraignment of having recourse to the witch-craft of magical virtues and to the mystery of "occult qualities." What then is the precise import of this supposed obnoxious epithet *occult* as applied to material property or quality? A property whose existence is once clearly demonstrated, can scarcely with propriety be characterized as hidden, unknown, or undiscovered.* Rather are

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Ivory. (*Encyclopæd. Brit.* 8th ed: vol. IV, p. 220.) The case of simple traction by a "solid" metallic rod can be explained *only*—(as J. Clerk Maxwell has well stated)—"by the existence of internal forces in its substance" or "between the particles of which the rod is composed, that is between bodies at distances which though small must be finite," and for these tensions acting through small distances—"we are as little able to account as for the action at any distance, however great." (*A Treatise on Electricity and Magnetism.* 8vo. 2 vols. 1873: part I, chap. V, sect. 105: vol. I, p. 123.)

*Leibnitz in his memorable controversy with Newton regarding the authorship of the infinitesimal calculus, took occasion—with a somewhat amusing though ill-tempered irrelevancy, to assail his rival's *mechanical* philosophy. In a published letter he says: "His philosophy appears to me somewhat strange, and I do not believe that it can ever be established. If all bodies possess gravity, it necessarily follows (however the defenders of the system may speak, and whatever heat they may display), that gravity these terms applicable to pretended explanations—having no basis in fact or in reason—proffered in the vain hope of avoiding unexpected or undesired inductions. But if the phrase be designed to stigmatize either the absolute cause of original properties or their mode of operation, as obscure, hidden, inexplicable, then the epithet is but the expression of a necessary and universal truth, which may be accepted with entire satisfaction.

On contemplating the backward steps of efficient causation, we find them not only finite in number, but in any case even surprisingly few,—if we neglect the complications of perturbation, and the successions of iteration in time. When we arrive at the primitive efficient cause, (if we accept it as ultimate,) this is by admission and very definition—inexplicable; since any attempt to explain it, necessarily refers it to an antecedent cause, and thus denies it to be ultimate.* Or if this denial be insisted on, then the series of

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must be a scholastic *occult quality*, or the effect of a miracle. - - - Nor do I find a vacuum established by the reasons of Mr. Newton, or of his partizans, any more than his pretended 'universal gravitation,' or than his 'atoms.' No one—unless with very contracted views—can believe either in the vacuum, or in the atoms."

With equal dignity and cogency, NEWTON replied to this tirade, in a letter dated February 26, 1716, that he was not to be drawn by M. Leibnitz into a dispute which was nothing to the question in hand. "As for philosophy, he colludes in the significations of words, calling those things 'miracles' which create no wonder; and those things 'occult qualities' whose *causes* are occult, though the qualities themselves be manifest." (Raphson's *History of Fluxions*. Also the *Works of Isaac Newton*, edited by Samuel Horsley. 5 vols. quarto. London, 1779–1785: where both letters are given: vol. IV, pp. 596, 598.)

*Says ROGER COTES in his admirable Preface to the *Principia*: "Since causes naturally recede in a continued chain from the more compounded to the more simple, when the most simple is reached no further backward step is possible. Hence an ultimate cause cannot admit of any mechanical explanation; for if it could, it would by that very fact cease to be ultimate. Will you therefore banish ultimate causes by calling them 'occult?' Then those immediately depending on such must next alike be banished, and straightway those next following; until relieved from every vestige of a cause, philosophy shall indeed stand purged!" (Newton's *Principia*. Second edition. 1713. *Preface.*)

Says Sir WILLIAM HAMILTON, "As every effect is only produced by the concurrence of at least two causes, and as these concurrent or co-efficient causes in fact constitute the effect, it follows that the lower we descend in the series of causes, the more complex will be the product; and that the explanations is necessarily illimitable, and as necessarily beyond the grasp of human comprehension. Do what we will we cannot escape the inexorable logic of fact,—the certainty of conviction that the ultimate must in the nature of things be forever the unintelligible, the inexplicable, the inscrutable;—that (paradoxical as it may sound) no explanation can be accounted final until it has been pursued backward to the unexplainable.

And this furnishes an additional objection to the kinematic scheme,—that it leaves a vast domain—a phantasmagoria of inconsequent motions—still to be explained;—that however irrational or inexplicable its last postulate, it does not attain to that simplicity of inherent, inscrutable, attribute of power, which must ever be the test of final resolution.

He who supposes, therefore, "that the information of the senses is adequate (with the aid of mathematical reasoning) to explain phenomena of *all kinds*," who refuses to admit "that there are physical operations which are—and ever will be incomprehensible by us," betrays a very imperfect idea—no less of the impassable limitations of finite intellect, than of the fathomless profundity of nature's system.* He who thinks that by formally repudiating the mysterious, and confidently discarding the unknown, he thereby

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higher we ascend, it will be the more simple. - - - And as each step in the procedure carries us from the more complex to the more simple, and consequently nearer to unity, we at last arrive at that unity itself,—at that ultimate cause, which as ultimate cannot again be conceived as an effect." (*Lectures on Metaphysics:* lect. III, p. 42, of Am. edition. 8vo. Boston, 1859.)

Says Herbert Spencer, "It obviously follows that the most general truth not admitting of inclusion in any other, does not admit of interpretation. Of necessity therefore, explanation must eventually bring us down to the inexplicable. The deepest truth which we can get at must be unaccountable." (*First Principles.* 2d edition, 1869: part I, chap. 4, p. 73.)

*Prof. James Challis, in an essay "On the Fundamental Ideas of Matter and Force in Theoretical Physics," maintains that when there is no apparent contact between bodies, "it must still be concluded that the pressing body although invisible, exists,—unless we are prepared to admit that there are physical operations which are and ever will be incomprehensible by us. This admision is incompatible with the principles of the philosophy I am advocating, which assume that the information of the senses is adequate—with the aid of mathematical reasoning—to explain phenomena of all kinds." *L. E. D. Phil. Mag.* June, 1866: vol. XXXI, p. 467.)

abolishes or in the slightest degree diminishes his insuperable nescience of the ultimate,—but imitates the ostrich, and deludes himself.*

When men not yet emancipated from the realism of mediæval scholasticism began to turn their attention from the dreams of ontology to the actualities of sensible phenomena, it is scarcely to be wondered at that to every abstracted property of things around them, they gave "a local habitation and a name;" until the banished Nereids and Oreads, the Naiads and Dryads, the Sylphs and Gnomes, of poetic fable, were re-habilitated in a very pantheon of "occult qualities." When in a later age a larger observation and a more mathematical logic replaced these entities by more mechanical conceptions, it is perhaps as little surprising—in the momentum of re-action—that the term "occult quality" should become a shibboleth of aversion, of apprehension, and of opprobrium, the imputation of which should disturb the philosophy of even a Newton. But that we of the nineteenth century,—capable of understanding and of estimating at their approximate value the limits of these oscillations of intellectual kinetics, should be equally the timid servitors of a vocabulary—seems less excusable. Whether the intended reproach be applied to the *existence* of demonstrated qualities, or more critically to their *cause* and mode of action, is practically of little consequence. Let it be frankly avowed,—let it be boldly heralded, that in their *essence* all the primal qualities of matter *are* "occult;" and must of necessity forever remain so. Let it be recognized—with a fitting modesty—that this veil of Isis shall never be removed by mortal hands.†

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*The continental philosophers of the seventeenth century desired not only to abolish the fanciful qualities of bodies invented by their predecessors, but (as has been well said) "they tried also to abolish their own ignorance of the causes of the sensible qualities of matter. They would not have occult *causes*, and Leibnitz plainly confounds occult quality with occult cause. But it is needless to dwell upon the fact that the ultimate causes of all qualities are occult." *English Cyclopædia*—Division of *Arts and Sciences:* art. "Attraction:" vol. I, col. 739.)

† *Tὸν ἐμὸν πέπλον οὐδείς πω θνητὸς ἀπεκάλυφε.*—Inscription in the temple of Athene-Isis, at Sais on the Nile. "My veil no mortal ever withdrew."

"In bodies we see only their figures and colors, [etc.] - - - but their inward *substances* are not to be known either by our senses, or by any reflex

The Import of a "Mechanical" System.—It has been a fond assumption of the kinematist that his all-embracing system of *motion* as the origin and essence of phenomena, is pre-eminently the "mechanical" theory of nature as contrasted with a "mystical" or "transcendental" theory. It may be well therefore to consider what is really signified by the term "mechanical."

Underlying every possible conception of the simplest element of a "machine" are two essential postulates:—first, the necessity of a frame-work invested with the inherent qualities giving it structural consistence and endurance,—and secondly, the necessity of a store of potential energy by which it may be actuated and made operative: since it is an elementary truism that no machine can *originate* energy.

The geometrician who ambitious of placing his science on a more rational basis should announce a new system rejecting all assumptions and establishing its theorems by no propositions which had not first been mathematically demonstrated, might possibly receive the applause of the inexpert, but would not be likely to meet with approbation or encouragement from the great jury of his brother geometers. The physicist who proclaims that he undertakes to build up a system of mechanical laws on a foundation exclusively mechanical, acts in no sense and in no degree less irrationally. Probably his first requirement will be—"given a rigid body." But

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act of our minds." ISAAC NEWTON. (*Principia.* 1687: book III,—concluding "scholium.")

"In fact the causes of all phenomena are at last occult. There has however obtained a not unnatural presumption against such causes; and this presumption though often salutary has sometimes operated most disadvantageously to science." Sir WILLIAM HAMILTON. (*Discussions on Philosophy and Literature.* 8vo. London, 1852: appendix I, p. 611.)

"The first causes of phenomena lie beyond the limited scope of our perceptive and reasoning faculties. - - - Their intimate nature and prime origin are for us inscrutable mysteries." Dr. A. W. HOFFMAN. (*Introduction to Modern Chemistry.* 1865: lec. IX, p. 138.)

"Ultimate scientific ideas then are all representative of realities that cannot be comprehended. - - - Alike in the external and the internal worlds, the man of science sees himself in the midst of perpetual changes—of which he can discover neither the beginning nor the end. - - - In all directions his investigations eventually bring him face to face with an insoluble enigma; and he ever more clearly perceives it to be an insoluble enigma." HERBERT SPENCER. (*First Principles.* 2d ed. 1869: part I, chap. III: sect. 21, pp. 66, 67.)

by no construction, by no combination, by no involution or evolution of any purely "mechanical" process can he possibly obtain, or explain, or even conceive his postulate—a rigid body. The attempt is indeed *more* hopeless than to demonstrate an axiom by mathematical deduction. That which is the necessary basis and starting-point of any intelligible mechanics, can scarcely be supposed to be the product or derivative of such mechanics. A truly mechanical theory cannot dispense with an extraneous foundation. Those who would exclude potential causes from the field of mechanical science, but betray the hopeless—helpless nakedness and imbecility of their hypothetic fictions. "Later philosophers" says Isaac Newton, "banish the consideration of such a cause out of natural philosophy, feigning hypotheses for explaining all things *mechanically*, and referring other causes to 'metaphysics;' whereas the main business of natural philosophy is to argue from phenomena without feigning hypotheses, and to deduce causes from effects, till we come to the very first cause,—*which certainly is not mechanical.*" *

Give to the ambitious kinematic artist his cloud of sand,—or if he prefer the outfit, let him be furnished with an indefinite quantity of a perfectly continuous frictionless and incompressible fluid—bound up if you please in a chain of "vortex rings,"—by no motions or composition of motions—continued through the æons of eternity—could he ever manufacture therefrom either a lever, or a rope. The kinematic gospel of a *mechanical* theory of primeval motion is therefore a sophism and illusion. It is founded on a misconception of the very *essence* of a true mechanics. And the system that would proudly aspire to an architecture of a kosmos from the elements of matter disrobed and denuded of every quality but motion, would achieve as its highest triumph and product—a universe of dust and ashes.

Without *inertia* there could be neither transmission of motion, nor even continuity of motion. Without inertia, kinematics itself would be but an empty name. And *with* inertia, kinematics would be a science of purely rectilinear movement; for by no artifice could any other be producible. No curvature of motion—no resilience of motion—is possible without the domination and constraint of occult forces. Without "dynamics" there could be no such thing as a science of "kinetics." Without the ceaseless presence and action of occult forces there could be no such thing as the

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* *Ootics.* Second edition, 1717: book III, query 28.

conservation of energy; there could be no such thing as the production of energy.

Force—Real and Indispensable.—"Force" then is not a metaphorical abstraction: it is not a convenient asylum of ignorance. It is the most real,—the most fundamental,—the most inseparable of material attributes. It is the potency and faculty whereby all inorganic—no less than organic—forms are builded, and whereby alone their kaleidoscopic phenomena are revealed to our perceptions. And it is from the never resting antagonisms and reprisals of diverse forces that are made up the activity, the life, and the glory of the world in which we have our being; to whose ever changing—ever becoming—ever nascent pageantry, the poetry of antiquity has given the name—*Natura.*

In spite of every effort made to realize a favorite dream, there is no "unity of force." To the dynamics of even a single molecule, the contestation and constraint of at least two opposite resisting agencies are indispensable: and in the various play of matter, other such agencies are no less clearly manifested. Nor is the certainty of multiplicity, in the slightest degree impaired by our admitted ignorance as to the final number of primeval forces. It may be that chemical affinity, and magnetism, are like heat, and electricity,* merely derivative forms of energy; but at least this

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*It is not a little remarkable that a tendency seems lately to have arisen to assign *electricity* to the station of a primitive force; and several physicists have almost simultaneously maintained its indestructibility and inconvertibility.

Dr. O. J. Lodge, in a lecture delivered at the London Institution, December 16, 1880, says: "To the question What is electricity?—We cannot assert that it is a form of matter, neither can we deny it; on the other hand we certainly cannot assert that it is a form of energy, and I should be disposed to deny it. - - - It is as impossible to generate electricity in the sense I am trying to give the word, as it is to produce matter!" (*Nature.* January 27, 1881: vol. XXIII, p. 302.)

Mr. G. Lippman, in a memoir presented to the Académie des Sciences of France, May 2, 1881, maintains that all electrical changes have an algebraic sum of zero: or in other words, that electricity can neither be created nor destroyed: the subject of the paper being "The Conservation of Electricity." (*Comptes Rendus.* 1881: vol. XCII, p. 1049.—Also, *L. E. D. Phil. Mag.* June, 1881: vol. XI, p. 474.)

Prof. Sylvanus P. Thompson, "in Elementary Lessons in Electricity," (preface,) also maintains as an important hypothesis in the treat-has not as yet been satisfactorily made out. The craving of the intellect for unity must therefore pursue its quest beyond and above the material empire of the physical forces.

The Conception of Natural "Law."—The habitudes of forces form the ultimate goal and boundary of scientific thought: and as the ascertainment and assignment of these habitudes (which we formulate as "laws" of matter) form the *object* of all science, so are their unerring certainty and uniformity of action at once the necessary *postulates* and the sole *condition* of all science. But the formulated "law" is but our mental concept of a habitude and a constancy whose method forever eludes our widest grasp, while forever challenging our most daring speculation. What *is* a law of nature? What is there behind it—to ordain or to enforce it. Do forces conform to the canons of an implicit prescription? Or is the so-called "law" but the summary and explication of autogenous deportment? Whichever be our assumption, the marvel and the incomprehensibility alike remain.

Sir John Herschel, in a playful colloquy "On Atoms," referring to their prompt obedience to the laws of their being, pithily asks: "Do they know them? Can they remember them? How else can they *obey* them?—conform to a fixed rule! Then they must be able to apply the rule as the case arises. - - - Their movements, their interchanges, their 'hates and loves,' their 'attractions and repulsions,' their 'correlations,' are all determined on the very instant. There is no hesitation, no blundering, no trial and error. A problem of dynamics which would drive Lagrange mad is solved *instanter*. A differential equation which algebraically written out would belt the earth, is integrated in an eye-twinkle." *

When we ask ourselves what these inflexible and unfailing laws of

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ment of the subject, "the conservation of electricity;" holding "that electricity, whatever it may prove to be, is not matter and is not energy," and "that it can neither be created nor destroyed." (*Nature*. May 26, 1881: vol. XXIV, p. 78.—*Elementary Lessons*, [etc.] 12 mo. London, 1881.)

The electric and caloric fluids furnish a very striking and suggestive parallelism; and the common rotatory glass cylinder would have furnished Rumford with as pertinent a theme for his argument as his gun-boring lathe.

* *Fortnightly Review*. May 15, 1865: pp. 83, 84. Also, *Familiar Lectures on Scientific Subjects*. London, 1866: pp. 456, 458.

force really mean ?—Why they are thus and not otherwise ?—Why they are so diverse and irreducible, and each so perfectly autocratic ?—Why for example independent molecules bound in the cohesion and adhesion of the "liquid" or the "solid" condition, should exhibit an attraction for each other a thousand-fold stronger than their mutual gravitation ?—Why two atoms within a molecule should cling together with a tenacity only *increasing* with their enforced centrifugal separation, while perfectly similar atoms not thus united attract each other with a strength *decreasing* with the second power of their distance ?—Why the chemical affinity of dissimilar molecules shall attach them with a force incomparably greater than even that of their physical cohesion ?—so that a drop of water may be shattered and lifted by the sun-beam, precipitated in snow, ground beneath a glacier, re-melted and dashed to foam in tumbling cataracts, may be combined in the solid substance of a hydrated crystal or in the complex constitution of an organic being, may be tortured in the chemist's retort or forced in hissing fury through the steam-engine, may pass through protean changes more varied than fable ever fancied, and yet in all these marvellous pilgrimages shall never loosen its structure as a compounded molecule of hydrogen and oxygen :—Why these same elements—so firmly enchained that the oxygen will quit its grasp only under the decomposing enticement of a more powerful affinity, or under the dissociative violence of a molecular velocity and clash representing the temperature of highest incandescence,—are yet so averse to separate condensation that only the combination of extremest cold and pressure attainable by human artifice has succeeded in bringing the molecules of either to a momentary liquid or solid cohesion ?—we find such questionings though irresistibly suggested, as irreversibly removed outside the pale of oracle or answer. There is no mystery in the world of mind, that is not fully parallelled by mysteries as bewildering in the world of matter.

Hemmed in by the impassable limitations of a restricted experience and of a no less restricted faculty of reason, we find the finite radius of our science touching in every direction the shadowy universe of nescience; and where most we seem to know, there most we encounter the cloud-land of the unknowable. In our highest reach and proudest triumph of analytic achievement,—in that symbolical reasoning upon quantitive relation which we call *par excellence* the "mathematical,"—we find that our symbols over-step their appointed purpose, and our equations traversing the mystic region of "imaginary" expressions, transcend alike our interpretation and our comprehension.

Final Unity of Causation.—As every suggestion of an assignable limit to space or time directly impels us to "overleap all bounds," so the very definiteness of the *physical* leads us to spring in imagination beyond its frontiers, and to seek refuge in the transcendental;—not the *supernatural* as replacing or suspending the natural, but as supplementing and completing it—the ultra-natural,—in its best and highest sense the *metaphysical.* Incapable though we be of realizing in thought anything but the finite and the relative, we none the less find ourselves alike incapable of confining our thought to these; and the necessity which inexorably forbids our conception of the infinite and the absolute, no less imperiously compels our unhesitating acceptance of the unknown infinite and absolute as the unavoidable counterparts of the known finite and relative.*

Our visible material universe—to all appearance limited in extent—an islet in the boundless void,—is no less limited in duration,—at least as to any of its aspects now displayed. Nor have the falling leaf or the ageing man, the disappearance of races or the past extinction of species of genera and of orders,—more clearly inscribed upon them, the universal law and lesson of ephemeral birth development and decay, than have the starry heavens themselves. *Under the present system of dynamic law*, it is certain that as radiating and cooling bodies,

> "The stars shall fade away, the sun himself
> Grow dim with age, and nature sink in years."

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*Sir William Hamilton has well remarked (in his Essay on the "Philosophy of the Unconditioned"): "The *Infinite* and the *Absolute* (properly so called) are thus equally inconceivable to us. - - - We are thus taught the salutary lesson that the capacity of thought is not to be constituted into the measure of existence; and are warned from recognizing the domain of our knowledge as necessarily co-extensive with the horizon of our faith. And by a wonderful revelation we are thus in the very consciousness of our inability to conceive aught above the relative and finite, inspired with a belief in the existence of something unconditional beyond the sphere of all comprehensible reality." (*Discussions on Philosophy and Literature.* 8vo. London, 1852: part I, pp. 13 and 15.) This Essay—a Review of Victor Cousin's *Cours de Philosophie*,—was originally published in the *Edinburgh Review*, October, 1829: vol. I, pp. 194–221.

Nor is there known to science any natural process whereby this cosmic doom may be either averted, or repaired by ulterior reversal.* And when turning backward through precessive geneses of worlds and suns and systems, and recalling in imagination the heat continuously expended and dissipated during millions of millions of years, until all matter is volatilized and re-expanded in the uniform tenuity and diffusion of the primitive nebular chaos, we endeavor to extend our retrograde inspection for another billion of years,—lost in the dizzying retrospect, we find that we have neither scale, nor mechanical principle, nor hydrodynamical theory, whereby to gage or guess the antecedents of this nebular chaos.

And here again—behind the mystery and inconceivability of atomic forces, lies the still greater mystery and inconceivability of primæval nature. And yet majestic as the wondrous march of cosmic evolution—(by purely human standards), it has probably consumed no greater number of our fleeting years, than the revolutions executed by the slowest atoms in a single second of time! Or by whatever number this be multiplied, how brief an interval has it fulfilled in the great infinitude of panoramic time,—in the far-stretching ages of a past eternity.

While an intellectual necessity demands the continuity of causation and of sequence, and holds any cessation of these as positively unthinkable, we thus observe that on every side we are confronted

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* Of various suggestions (made from a teleological stand-point) for reversing the great law of "dissipation," and supplying to declining systems an *elixir vitæ* for their perpetual regeneration, perhaps the two most notable are those of Rankine and of Siemens.

William J. M. Rankine, in a paper "On the Re-concentration of the Mechanical Energy of the Universe," read before the British Association at its Belfast meeting, in September, 1852,—assuming a boundary to the ætherial medium, argues that the radiations dissipated outward, would at the limiting surface be all reflected inward to foci, at which exhausted suns would be re-kindled into incandescence, or "vaporized and resolved into their elements." (*Report Brit. Assoc.* 1852: part II,—abstracts, p. 12.—Or more fully in *L. E. D. Phil. Mag.* November, 1852: vol. IV, p. 358.)

Charles William Siemens, in a paper "On the Conservation of Solar Energy," read before the Royal Society, March 2, 1882, assuming gaseous products of combustion to be thrown off in a dissociated form from the equatorial regions of the revolving sun, (as from a centrifugal fan,) argues that they would be constantly indrawn at the polar regions, to be reburned and again given off,—in a perpetual circulation. (*Nature.* March 9, 1882: vol. XXV. pp. 440–444.)

and beset by barriers through which no loop-hole of escape appears. The mind thus baffled and bewildered in its backward inquest through illimitable series, in which to its dismay is found at no great distance—whether in atom, or in universe,—the chasm of a strange and incomprehensible discontinuity, the inevitable transition to an entirely different order of links from those made thinkable by experience, seems driven in the last resort to the unifying induction of a single, first, eternal, and all-powerful Cause—from which all other causes are dependent and derived.

This ultimate and highest induction of scientific thought—the Inscrutable made Absolute—is restful and satisfying. This ultimate and highest induction—as highest and ultimate, cannot be manipulated as a "working hypothesis." This ultimate and highest induction—as such—cannot be subjected to the subsequent verification of mathematical deduction. This ultimate and highest induction detracts nothing from the certainty of orderly sequence so irresistibly impressed upon us by every deepening channel of research, but gives us rational ground and guarantee of such unfailing regularity. This ultimate and highest induction accepting to the uttermost the mechanical interpretation of nature's administration,—whose ceaseless evolution seems ever opening up new vistas of an automatic teleology,—gives significance to our imperfect conception of a regulated system, (so necessarily involved in the very existence and operation of a "machine,") and accounts consistently for the unfaltering obedience and instantaneous response of all the countless atoms of the universe to the reign of "law," by positing behind such law—an Infinite LAW-GIVER.

In Richard Hooker's never trite though memorable words: "Of *Law* there can be no less acknowledged than that her seat is the bosom of God, her voice the harmony of the world: all things in heaven and earth do her homage,—the very least as feeling her care, and the greatest as not exempted from her power."

226TH MEETING. DECEMBER 16, 1882.

TWELFTH ANNUAL MEETING.

The President in the Chair.

About fifty members were present during the evening.

The President announced the usual order of exercises.

The minutes of the last annual meeting were read and approved.

The Secretary, Mr. GILL, read the list of members who had been elected since the last annual meeting.

The Treasurer read his report upon the finances and property of the Society. (See page 180.)

The Chairman appointed as Auditing Committee, Messrs. Thomas Antisell, Benjamin Alvord, and Otis T. Mason.

The Treasurer read the roll of names of members who were entitled to vote at the election of officers.

The Society then proceeded to ballot for the election of officers, with the following result: (See next page.)

The rough minutes of the meeting were read and approved; and the meeting then adjourned.

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

ELECTED DECEMBER 16, 1882.

President .... J. W. POWELL.

Vice-Presidents .... J. C. WELLING, J. E. HILGARD,
C. H. CRANE, J. S. BILLINGS.

Treasurer .... CLEVELAND ABBE.

Secretaries .... G. K. GILBERT, HENRY FARQUHAR.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

W. H. DALL, C. E. DUTTON,
J. R. EASTMAN, E. B. ELLIOTT,
R. FLETCHER, WM. HARKNESS,
D. L. HUNTINGTON, GARRICK MALLERY,
C. A. SCHOTT.

STANDING COMMITTEES.

On Communications:

J. S. BILLINGS, *Chairman.* G. K. GILBERT, HENRY FARQUHAR.

On Publications:

G. K. GILBERT, *Chairman.* HENRY FARQUHAR, CLEVELAND ABBE,
S. F. BAIRD.*

*As Secretary of the Smithsonian Institution.

ANNUAL REPORT OF THE TREASURER.

WASHINGTON, D. C., *December* 17, 1881.

To the Philosophical Society of Washington:

Owing to the change in the time of presentation of the Treasurer's report, I have the honor to present herewith my annual statement as Treasurer for the years 1880 and 1881, showing a cash balance on December 16th, in the treasury, of three hundred and twenty dollars and sixteen cents, ($320.16.)

The investments of the Society consist of—

One United States bond, No. 4569 A, (registered,) of the funded loan 1891, for $1,000, yielding 4½ per cent.;

One United States bond, No. 20031, (registered,) of the funded loan of 1907, for $500, yielding 4 per cent.

The further assets of the Society consist of unpaid dues amounting to about three hundred and thirty dollars, ($330.)

The active membership of the Society is to-day about one hundred and fifty-five, (155.)

The stock on hand of the publications of the Society is about as follows, by actual count:

	No. of copies.	Price to members.
Vol. I of the Bulletin	93	$2 00
II "	92	3 00
III "	182	1 00
IV "	190	1 00
Taylor's Memoir of Prof. Henry—		
1st edition	64	50
2d "	30	1 00
Welling's Memoir of Prof. Henry	4	50

The Library has lately received, by way of exchange, about fifty volumes, but these have not yet been catalogued and arranged.

Special copies of each communication that appears in the Bulletin of the Society are promptly printed for distribution by the author; the annual volumes of the Bulletin are sent usually to about 125 domestic and foreign recipients, selected with special view to the general dissemination of information as to the activity of the Society.

The distribution of stitched annual volumes, instead of individual signatures, gives general satisfaction, and is much more economical in time and labor. Much attention is given to collecting the scattered signatures of the first volume, and thus the stock in hand of the complete volume is being slowly replenished.

Volumes I, II, and III of the Bulletin have been stereotyped and printed (with some corrections) at the expense of the Smithsonian Institution as Volume XX of the Miscellaneous Collections. It is certainly a matter of congratulation that the Society has thus assured to it the economical, permanent, and most extensive publication of its proceedings; and the general effect of this arrangement is to offer stronger inducements to our members to publish through this medium.

The expense to the Society of the publication of the first three volumes of the Bulletin was easily borne by reason of the slow accumulation of the funds in the treasury; but the cost of publication of Volume IV has been entirely defrayed out of the income of the past year, and has required very nearly the whole of our receipts, so that the balance in the treasury is now only $320.16, as compared with two hundred and fourteen dollars and eighty-two cents, ($214.82) at the beginning of 1881. The Treasurer has therefore felt himself under the necessity of distributing this volume only to members who are not in arrears.

The actual expense of the editions of 500 copies each of the respective volumes has been very nearly as follows:

Vol.	No. of signatures.	Cost per edition.	Cost per copy.
No. I	10	$386	$0 77
II	18	686	1 37
III	12	333	67
IV	12	391	78

It is therefore probable that the steady increase in the membership and work of the Society is likely soon to so increase the extent and cost of our Bulletin as to absorb our whole income.

In view of the fact that the free use of our present admirable quarters is a privilege granted by the Surgeon-General, liable at any time to be revoked, I think it important that there should always be a very considerable annual surplus to be added to the permanently-invested fund, the income of which will at some future day enable the Society to lease appropriate quarters in some central locality.

I have the honor to remain, very respectfully,

CLEVELAND ABBE, *Treasurer.*

DR. *The Philosophical Society of Washington in account with Cleveland Abbe, Treasurer.* CR.

EXPENDITURES.

Date.	Voucher.	Check.	To whom paid.	Amount.
1880.				
Jan. 29	1	36	S. N. Griffin	$8 00
Jan. 23	2	37	E. M. Whitaker	14 25
Apr. 14	3	38	Judd & Detweiler	12 50
June 29	4	39	Judd & Detweiler	43 49
July 22	5	40	C. Abbe	10 30
July 24	6	41	T. K. Collins	194 09
July 31	7	43	M. Joyce	15 50
July 28	} 8	42	Judd & Detweiler	50 00
Aug. 4		44	Judd & Detweiler	91 33
Aug. 4	9	45	C. Abbe	20 00
Aug. 13	10	46	T. K. Collins	4 00
Aug. 31	11	47	Judd & Detweiler	13 56
Dec. 6	12	48	T. K. Collins	14 20
1881.				
Jan. 6	13	49	Judd & Detweiler	52 50
				543 72
Jan. 26	----	----	To Balance on hand	214 82
			Check	$758 54

ANNUAL DUES RECEIVED DURING 1880.

Date.	1877.	1878.	1879.	1880.	1881.	Total.
1880.						
Apr. 30	------	------	$15 00	$30 00	------	$45 00
July 24	------	------	25 00	325 00	------	350 00
Aug. 25	$5 00	$5 00	10 00	20 00	------	40 00
Oct. 27	------	------	10 00	40 00	------	50 00
Nov. 9	------	------	------	70 00	$5 00	75 00
Dec. 22	------	------	10 00	20 00	15 00	45 00
Dec. 31	------	$5 00	5 00	5 00	5 00	20 00
By total dues received						$625 00
By interest on invested funds:						
One $1,000 U. S. bond, yielding 4½ per cent					$45 00	
One $500 U. S. bond, yielding 4 per cent					20 00	
Total from invested bonds						65 00
						690 00
By Balance carried forward from 1879						68 54
By Total from all sources						$758 54

Cr. *The Philosophical Society of Washington in account with Cleveland Abbe, Treasurer.* Cr.

Expenditures.

Date.		Voucher.	Check.	To whom paid.	Amount.
1881.					
March	3	1	50	Judd & Detweiler	$29 31
March	4	2	51	Judd & Detweiler	10 00
June	6	3	52	Judd & Detweiler	10 00
Oct.	10	} 4	53	Judd & Detweiler	250 00
Nov.	18		55	Judd & Detweiler	156 20
Nov.	4	5	54	C. E. Dutton	20 65
Nov.	22	6	56	C. E. Dutton	20 00
Dec.	14	7	57	C. Abbe	5 00
Dec.	15	8	----	F. B. Mohun	3 50
					504 66
				To Balance on hand	320 16
					$824 82

Annual Dues Received during 1881.

Date.		1878.	1879.	1880.	1881.	1882.	Total.
Feb.	26	----	$5 00	$45 00	$15 00	----	$65 00
May	9	----	----	5 00	20 00	----	25 00
May	19	----	----	----	85 00	----	85 00
May	28	----	----	5 00	25 00	----	30 00
May	31	----	----	5 00	45 00	----	50 00
Sept.	30	----	5 00	25 00	105 00	----	135 00
Nov.	25	----	----	----	55 00	----	55 00
Dec.	1	$5 00	10 00	15 00	55 00	----	85 00
Dec.	15	----	----	----	15 00	$5 00	20 00
By total dues received							$550 00
By interest on invested funds:							
One $1,000 U. S. bond, 4½ per cent						$45 00	
One $500 U. S. bond, 4 per cent						15 00	
Total from invested funds							60 00
							610 00
By Balance carried forward from 1880							214 82
By total from all sources							$824 82

ANNUAL REPORT OF THE TREASURER.

WASHINGTON CITY, *Dec.* 16, 1882.

To the Philosophical Society of Washington:

I have the honor to present herewith my annual statement as Treasurer, covering the year ending with December 15, 1882, and showing a cash balance deposited with Riggs & Co. of $521.07. This balance is much larger than would have been the case had it not been decided to delay the publication of Volume V of the Bulletin.

The investment of the funds of the Society remains as in my last report, viz.:

One U. S. registered bond, $1,000, at 4½ per cent.
One U. S. registered bond, $500, at 4 per cent.

The further assets of the Society consist of unpaid annual dues to the amount of $300 for 1882, and of about $200 for 1881 and earlier years.

The number of active members is now about 150; the corresponding annual income, about 800 dollars.

The stock in hand of publications remains as about as reported by me a year ago.

An accession catalogue of the library has been recently compiled. The number of volumes at present on hand is 68; these have been presented by way of exchange; and we are especially indebted to the Royal Societies of Edinburgh, of Munich, and of New South Wales, and the Literary and Philosophical Society of Manchester for long series of volumes.

Very respectfully,

(Signed) CLEVELAND ABBE,
Treasurer.

DR. *The Philosophical Society of Washington in account with Cleveland Abbe, Treasurer, from Dec. 15, 1881, to Dec. 15, 1882.* CR.

EXPENDITURES.

Date.		Vou'r.	Check.	To whom paid.	Amount.
1881.					
Dec.	—	1	59	Judd & Detweiler	$5 40
1882.					
Jan.	30	2	60	S. J. Waldo	3 35
Jan.	18	3	61	Marcus Baker	3 50
Feb.	8	4	62	S. F. Bartlett	4 50
March	2	5	63	Marcus Baker	3 00
Feb.	28	6	64	Judd & Detweiler	21 30
June	1	7	65	Marcus Baker	6 00
May	31	8	66	Judd & Detweiler	22 95
June	15	9	67	C. Abbe, Treasurer	7 25
June	30	10	68	Judd & Detweiler	51 53
June	30	11	69	Marcus Baker	2 00
July	14	12	70	Marcus Baker	2 00
Oct.	4	13	71	Judd & Detweiler	2 50
Oct.	11	14	72	Judd & Detweiler	161 81
Oct.	14	15	73	B. F. Brown	2 00
				Total	$379 09
				Bal. on dep't with Riggs & Co.	521 07
				Total	900 16

RECEIPTS.

From what source.	Amount.	Total.
Credit by receipts as follows:		
Balance carried over from December, 1881		$320 16
Annual dues received:		
and deposited December 19, 1881	$75 00	
and deposited June 30, 1882	190 00	
and deposited July 31, 1882	175 00	
and deposited December 2, 1882	75 00	
	$515 00	
Interest on invested funds, viz.:		
One $1,000 U. S. bond, at 4½ per cent	45 00	
One $500 U. S. bond, at 4 per cent	20 00	
Total receipts		580 00
Total from all sources		$900 16

We have examined this account and find the same correct and properly vouched. December 18, 1882.

Auditors: THOMAS ANTISELL. BENJ. ALVORD. O. T. MASON.

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I. NAMES OF PERSONS.

II. SUBJECTS.