ANNUAL REPORT

OF THE

BOARD OF REGENTS

OF THE

SMITHSONIAN INSTITUTION,

SHOWING THE

OPERATIONS, EXPENDITURES, AND CONDITION OF THE INSTITUTION FOR THE YEAR 1859.

LETTER

SECRETARY OF THE SMITHSONIAN INSTITUTION,

The Annual Report of the operations, expenditures, and condition of the Smithsonian Institution for the year 1859.

JUNE 11, 1860.—Laid upon the table, and ordered to be printed.

IN THE HOUSE OF REPRESENTATIVES, *June* 11, 1860.

Resolved, That there be printed five thousand extra copies of the Report of the Smithsonian Institution for the year 1859 : three thousand for the use of the members of the House, and two thousand for the use of said Institution.

Attest : J. W. FORNEY, *Clerk.*

SMITHSONIAN INSTITUTION,
Washington, June 9, 1860.

SIR: In behalf of the Board of Regents, I have the honor to submit to the House of Representatives of the United States the Annual Report of the operations, expenditures, and condition of the Smithsonian Institution for the year 1859.

I have the honor to be, very respectfully, your obedient servant,

JOSEPH HENRY,
Secretary Smithsonian Institution.

Hon. WILLIAM PENNINGTON,
Speaker of the House of Representatives.

ANNUAL REPORT

OF THE

BOARD OF REGENTS

OF THE

SMITHSONIAN INSTITUTION,

SHOWING

THE OPERATIONS, EXPENDITURES, AND CONDITION OF THE INSTITUTION UP TO JANUARY 1, 1860, AND THE PROCEEDINGS OF THE BOARD UP TO APRIL 8, 1860.

To the Senate and House of Representatives:

In obedience to the act of Congress of August 10, 1846, establishing the Smithsonian Institution, the undersigned, in behalf of the Regents, submit to Congress, as a report of the operations, expenditures, and condition of the Institution, the following documents:

1. The Annual Report of the Secretary, giving an account of the operations of the Institution during the year 1859.

2. Report of the Executive Committee, giving a general statement of the proceeds and disposition of the Smithsonian fund, and also an account of the expenditures for the year 1859.

3. Proceedings of the Board of Regents up to April 8, 1860.

4. Appendix.

Respectfully submitted.

R. B. TANEY, *Chancellor.*
JOSEPH HENRY, *Secretary.*

OFFICERS OF THE SMITHSONIAN INSTITUTION.

JAMES BUCHANAN, *Ex officio* Presiding Officer of the Institution.
ROGER B. TANEY, Chancellor of the Institution.

JOSEPH HENRY, Secretary of the Institution.
SPENCER F. BAIRD, Assistant Secretary.
W. W. SEATON, Treasurer.
WILLIAM J. RHEES, Chief Clerk.

JAMES A. PEARCE, ⎫
A. D. BACHE,   ⎬ Executive Committee.
JOSEPH G. TOTTEN, ⎭

REGENTS OF THE INSTITUTION.

JOHN C. BRECKINRIDGE, Vice President of the United States.
ROGER B. TANEY, Chief Justice of the United States.
JAMES G. BERRET, Mayor of the City of Washington.
JAMES A. PEARCE, member of the Senate of the United States.
JAMES M. MASON, member of the Senate of the United States.
STEPHEN A. DOUGLAS, member of the Senate of the United States.
WILLIAM H. ENGLISH, member of the House of Representatives.
L. J. GARTRELL, member of the House of Representatives.
BENJAMIN STANTON, member of the House of Representatives.
GIDEON HAWLEY, citizen of New York.
—— (Vacancy occasioned by the death of Hon. Richard Rush.)
GEORGE E. BADGER, citizen of North Carolina.
CORNELIUS C. FELTON, citizen of Massachusetts.
ALEXANDER D. BACHE, citizen of Washington.
JOSEPH G. TOTTEN, citizen of Washington.

MEMBERS EX OFFICIO OF THE INSTITUTION.

JAMES BUCHANAN, President of the United States.
JOHN C. BRECKINRIDGE, Vice President of the United States.
LEWIS CASS, Secretary of State.
HOWELL COBB, Secretary of the Treasury.
JOHN B. FLOYD, Secretary of War.
ISAAC TOUCEY, Secretary of the Navy.
JOSEPH HOLT, Postmaster General.
J. S. BLACK, Attorney General.
ROGER B. TANEY, Chief Justice of the United States.
P. F. THOMAS, Commissioner of Patents.
JAMES G. BERRET, Mayor of the City of Washington.

HONORARY MEMBERS.

BENJAMIN SILLIMAN, of Connecticut.
A. B. LONGSTREET, of Mississippi.
JACOB THOMPSON, Secretary of the Interior.

PROGRAMME OF ORGANIZATION

OF THE

SMITHSONIAN INSTITUTION.

[PRESENTED IN THE FIRST ANNUAL REPORT OF THE SECRETARY, AND
ADOPTED BY THE BOARD OF REGENTS, DECEMBER 13, 1847.]

INTRODUCTION.

*General considerations which should serve as a guide in adopting a
Plan of Organization.*

1. WILL OF SMITHSON. The property is bequeathed to the United
States of America, "to found at Washington, under the name of the
SMITHSONIAN INSTITUTION, an establishment for the increase and diffu-
sion of knowledge among men."

2. The bequest is for the benefit of mankind. The government of
the United States is merely a trustee to carry out the design of the
testator.

3. The Institution is not a national establishment, as is frequently
supposed, but the establishment of an individual, and is to bear and
perpetuate his name.

4. The objects of the Institution are, first, to increase, and second,
to diffuse knowledge among men.

5. These two objects should not be confounded with one another.
The first is to enlarge the existing stock of knowledge by the addition
of new truths; and the second, to disseminate knowledge, thus in-
creased, among men.

6. The will makes no restriction in favor of any particular kind of
knowledge; hence all branches are entitled to a share of attention.

7. Knowledge can be increased by different methods of facilitating
and promoting the discovery of new truths; and can be most exten-
sively diffused among men by means of the press.

8. To effect the greatest amount of good, the organization should
be such as to enable the Institution to produce results, in the way of
increasing and diffusing knowledge, which cannot be produced either
at all or so efficiently by the existing institutions in our country.

9. The organization should also be such as can be adopted pro-
visionally, can be easily reduced to practice, receive modifications,
or be abandoned, in whole or in part, without a sacrifice of the funds.

10. In order to compensate, in some measure, for the loss of time
occasioned by the delay of eight years in establishing the Institution,

a considerable portion of the interest which has accrued should be added to the principal.

11. In proportion to the wide field of knowledge to be cultivated, the funds are small. Economy should therefore be consulted in the construction of the building; and not only the first cost of the edifice should be considered, but also the continual expense of keeping it in repair, and of the support of the establishment necessarily connected with it. There should also be but few individuals permanently supported by the Institution.

12. The plan and dimensions of the building should be determined by the plan of organization, and not the converse.

13. It should be recollected that mankind in general are to be benefited by the bequest, and that, therefore, all unnecessary expenditure on local objects would be a perversion of the trust.

14. Besides the foregoing considerations deduced immediately from the will of Smithson, regard must be had to certain requirements of the act of Congress establishing the Institution. These are, a library, a museum, and a gallery of art, with a building on a liberal scale to contain them.

SECTION I.

Plan of Organization of the Institution in accordance with the foregoing deductions from the will of Smithson.

To INCREASE KNOWLEDGE. It is proposed—

1. To stimulate men of talent to make original researches, by offering suitable rewards for memoirs containing new truths; and

2. To appropriate annually a portion of the income for particular researches, under the direction of suitable persons.

To DIFFUSE KNOWLEDGE. It is proposed—

1. To publish a series of periodical reports on the progress of the different branches of knowledge; and

2. To publish occasionally separate treatises on subjects of general interest.

DETAILS OF THE PLAN TO INCREASE KNOWLEDGE.

I.—*By stimulating researches.*

1. Facilities afforded for the production of original memoirs on all branches of knowledge.

2. The memoirs thus obtained to be published in a series of volumes, in a quarto form, and entitled Smithsonian Contributions to Knowledge.

3. No memoir on subjects of physical science to be accepted for publication which does not furnish a positive addition to human knowledge, resting on original research; and all unverified speculations to be rejected.

4. Each memoir presented to the Institution to be submitted for examination to a commission of persons of reputation for learning in

the branch to which the memoir pertains; and to be accepted for publication only in case the report of this commission is favorable.

5. The commission to be chosen by the officers of the Institution, and the name of the author, as far as practicable, concealed, unless a favorable decision be made.

6. The volumes of the memoirs to be exchanged for the transactions of literary and scientific societies, and copies to be given to all the colleges and principal libraries in this country. One part of the remaining copies may be offered for sale; and the other carefully preserved, to form complete sets of the work, to supply the demand from new institutions.

7. An abstract, or popular account, of the contents of these memoirs to be given to the public through the annual report of the Regents to Congress.

II.—*By appropriating a part of the income, annually, to special objects of research, under the direction of suitable persons.*

1. The objects, and the amount appropriated, to be recommended by counsellors of the Institution.

2. Appropriations in different years to different objects, so that, in course of time, each branch of knowledge may receive a share.

3. The results obtained from these appropriations to be published with the memoirs before mentioned, in the volumes of the Smithsonian Contributions to Knowledge.

4. Examples of objects for which appropriations may be made.

(1.) System of extended meteorological observations for solving the problem of American storms.

(2.) Explorations in descriptive natural history, and geological, magnetical, and topographical surveys, to collect materials for the formation of a Physical Atlas of the United States.

(3.) Solution of experimental problems, such as a new determination of the weight of the earth, of the velocity of electricity, and of light; chemical analyses of soils and plants; collection and publication of scientific facts, accumulated in the offices of government.

(4.) Institution of statistical inquiries with reference to physical, moral, and political subjects.

(5.) Historical researches and accurate surveys of places celebrated in American history.

(6.) Ethnological researches, particularly with reference to the different races of men in North America; also, explorations and accurate surveys of the mounds and other remains of the ancient people of our country.

DETAILS OF THE PLAN FOR DIFFUSING KNOWLEDGE.

I.—*By the publication of a series of reports, giving an account of the new discoveries in science, and of the changes made from year to year in all branches of knowledge not strictly professional.*

1. These reports will diffuse a kind of knowledge generally interesting, but which, at present, is inaccessible to the public. Some of

the reports may be published annually, others at longer intervals, as the income of the Institution or the changes in the branches of knowledge may indicate.

2. The reports are to be prepared by collaborators eminent in the different branches of knowledge.

3. Each collaborator to be furnished with the journals and publications, domestic and foreign, necessary to the compilation of his report; to be paid a certain sum for his labors, and to be named on the title-page of the report.

4. The reports to be published in separate parts, so that persons interested in a particular branch can procure the parts relating to it without purchasing the whole.

5. These reports may be presented to Congress for partial distribution, the remaining copies to be given to literary and scientific institutions, and sold to individuals for a moderate price.

The following are some of the subjects which may be embraced in the reports:

I. PHYSICAL CLASS.

1. Physics, including astronomy, natural philosophy, chemistry, and meteorology.
2. Natural history, including botany, zoology, geology, &c.
3. Agriculture.
4. Application of science to arts.

II. MORAL AND POLITICAL CLASS.

5. Ethnology, including particular history, comparative philology, antiquities, &c.
6. Statistics and political economy.
7. Mental and moral philosophy.
8. A survey of the political events of the world, penal reform, &c.

III. LITERATURE AND THE FINE ARTS.

9. Modern literature.
10. The fine arts, and their application to the useful arts.
11. Bibliography.
12. Obituary notices of distinguished individuals.

II.--*By the publication of separate treatises on subjects of general interest.*

1. These treatises may occasionally consist of valuable memoirs translated from foreign languages, or of articles prepared under the direction of the Institution, or procured by offering premiums for the best exposition of a given subject.

2. The treatises should in all cases be submitted to a commission of competent judges previous to their publication.

3. As examples of these treatises, expositions may be obtained of the present state of the several branches of knowledge mentioned in the table of reports.

SECTION II.

Plan of organization, in accordance with the terms of the resolutions of the Board of Regents providing for the two modes of increasing and diffusing knowledge.

1. The act of Congress establishing the Institution contemplated the formation of a library and a museum; and the Board of Regents, including these objects in the plan of organization, resolved to divide the income* into two equal parts.
2. One part to be appropriated to increase and diffuse knowledge by means of publications and researches, agreeably to the scheme before given. The other part to be appropriated to the formation of a library and a collection of objects of nature and of art.
3. These two plans are not incompatible one with another.
4. To carry out the plan before described, a library will be required, consisting, 1st, of a complete collection of the transactions and proceedings of all the learned societies in the world; 2d, of the more important current periodical publications, and other works necessary in preparing the periodical reports.
5. The Institution should make special collections, particularly of objects to illustrate and verify its own publications.
6. Also, a collection of instruments of research in all branches of experimental science.
7. With reference to the collection of books, other than those mentioned above, catalogues of all the different libraries in the United States should be procured, in order that the valuable books first purchased may be such as are not to be found in the United States.
8. Also, catalogues of memoirs, and of books and other materials, should be collected for rendering the Institution a centre of bibliographical knowledge, whence the student may be directed to any work which he may require.
9. It is believed that the collections in natural history will increase by donation as rapidly as the income of the Institution can make provision for their reception, and, therefore, it will seldom be necessary to purchase articles of this kind.
10. Attempts should be made to procure for the gallery of art casts of the most celebrated articles of ancient and modern sculpture.
11. The arts may be encouraged by providing a room, free of expense, for the exhibition of the objects of the Art-Union and other similar societies.

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* The amount of the Smithsonian bequest received into the treasury of
  the United States is _____ $515,169 00
·Interest on the same to July 1, 1846, (devoted to the erection of the
  building)_____ 242,129 00
Annual income from the bequest_____ 30,910 14

12. A small appropriation should annually be made for models of antiquities, such as those of the remains of ancient temples, &c.

13. For the present, or until the building is fully completed, besides the Secretary, no permanent assistant will be required, except one, to act as librarian.

14. The Secretary, by the law of Congress, is alone responsible to the Regents. He shall take charge of the building and property, keep a record of proceedings, discharge the duties of librarian and keeper of the museum, and may, with the consent of the Regents, employ assistants.

15. The Secretary and his assistants, during the session of Congress, will be required to illustrate new discoveries in science, and to exhibit new objects of art; distinguished individuals should also be invited to give lectures on subjects of general interest.

This programme, which was at first adopted provisionally, has become the settled policy of the Institution. The only material change is that expressed by the following resolutions, adopted January 15, 1855, viz:

Resolved, That the 7th resolution, passed by the Board of Regents on the 26th of January, 1847, requiring an equal division of the income between the active operations and the museum and library, when the buildings are completed, be and it is hereby repealed.

Resolved, That hereafter the annual appropriations shall be apportioned specifically among the different objects and operations of the Institution in such manner as may, in the judgment of the Regents, be necessary and proper for each, according to its intrinsic importance, and a compliance in good faith with the law.

REPORT OF THE SECRETARY FOR 1859.

To the Board of Regents:

GENTLEMEN: I have the honor again to present to you the history of the transactions of the Smithsonian Institution for another year, and I am happy to be able, at the beginning of my report, to state that nothing has occurred since your last session to interfere with the successful prosecution of the several objects embraced in the plan of organization.

The funds of the establishment are still in a good condition: the original bequest of Smithson remains in the treasury of the United States; the extra fund which was saved from the annual income, is still invested in State stocks, which have since the last meeting of the Board considerably increased in marketable value, and could now be sold for more than was paid for them. The accumulation of half a year's income in the treasury at the beginning of last year has enabled us to pay in cash for all the materials purchased and labor performed on account of the Institution, and has thus not only been the means of a saving by reduction in the cost of the operations, but also of preventing the embarrassment and anxiety which has sometimes been felt on account of outstanding debts, besides enabling us more readily to adapt the expenditures to the several items of appropriation.

The Institution, during the past year, by its publications, exchanges, researches, &c., has sustained the reputation it had previously acquired, and has continued gradually to extend the sphere of its influence and usefulness. By its persevering efforts to carry out the will of the founder, it has succeeded in rendering familiar to the public mind in the United States the three fundamental distinctions in regard to knowledge, which must have an important bearing on the future advance of science in this country: namely, the *increase* of knowledge, the *diffusion* of knowledge, and the practical *application* of knowledge to useful purposes in the arts.

It is of the highest importance to the well-being of our race that each of these distinctions should be recognized, and that each of the processes to which they relate should receive encouragement and support.

In our country, however, they have not all met with an equal share of attention, and at the beginning of this Institution the confusion of ideas on this subject was so great that in the interpretation of the will, even by some of our prominent and enlightened men, the diffusion of knowledge was identified with its increase; and it was contended that Smithson had used the terms as synonymous, and desired by the one merely to enforce the other. But that this was not the case may be gathered from the meaning attached to these terms by the class of men to which he belonged. "While we may truly exult," says one of his eminent contemporaries,[*] "in the awakening of the national intellect, we must remember that *diffusion* and *advancement* are two very different processes, and that each may exist independent of the other. It is very essential, therefore, when we speak of the *diffusion* or *extension* of science, that we do not confound these stages of development with *discovery* or *advancement*, since the latter may be as different from the former as depth is from shallowness."

That the *diffusion* of knowledge has been an object of solicitude to the enlightened legislatures of almost every State in the Union is evinced by the provision which has been made for libraries, schools, academies, and colleges. The practical application of science to the useful arts has received direct encouragement from the general government by the enactment of patent laws and the establishment of the Patent Office. The fact, however, does not appear to have been so constantly before the public mind that the advance of science or the discovery of new truths, irrespective of their immediate application, is also a matter of great importance, and eminently worthy of patronage and support. The progress of society and the increase of the comfort and happiness of the human family depend as a basis on the degree of our knowledge of the laws by which Divine Wisdom conducts the affairs of the universe. He has created us with rational souls, and endowed us with faculties to comprehend in some measure the modes in which the operations of nature are effected; and just in proportion to the advance we make by patient and persevering study, in the knowledge of those modes or laws, are we enabled to apply the forces of nature to our own use, and to avert the dangers to which we are exposed from our ignorance of their varied influences.

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[*]Mr. Swainson.   Cabinet Cyclopedia, 1834.

Nearly all the great inventions which distinguish the present century are the results, immediately or remotely, of the application of scientific principles to practical purposes, and in most cases these applications have been suggested by the student of nature, whose primary object was the discovery of abstract truth. The statement cannot be too often repeated that each branch of knowledge is connected with every other, and that no light can be gained in regard to one which is not reflected upon all. Thus researches which at first sight appear the farthest removed from useful application, are in time found to have an important bearing on the advance of art, and consequently on the progress of society. To illustrate this position, I shall take the liberty of trespassing on your time with a few instances gleaned from the history of inventions.

Astronomy was not studied by Kepler, Galileo, or Newton for the practical applications which might result from it, but to enlarge the bounds of knowledge, to furnish new objects of thought and contemplation in regard to the universe of which we form a part; yet how remarkable the influence which this science, apparently so far removed from the sphere of our material interests, has exerted on the destinies of the world! Without its guidance what would navigation have remained but a timid exploration of coasts and inlets, leaving the fairest portions of the earth to be the heritage of rude and idolatrous tribes? The steam-engine, in its improved form, is due to the laborious scientific researches of Black, Watt, and Robinson, and the new theory of heat, which is now occupying so much of the attention of the abstract physicist, has lately served to modify our views of this agent, and to develop new and important facts in regard to it which will tend to economize its power, and increase the means of rendering it more effectually the obedient slave of intelligent man.

In the year 1739, the Rev. Dr. Clayton communicated to the Royal Society his discovery of what he called the "spirit of coal," which he confined in a bladder, and showed its burning powers as it issued from a puncture in the membrane. Sixty years after this Mr. Murdock, of Manchester, applied this discovery to the purpose of illumination; and what was at first a mere object of scientific research has now become, from its almost universal employment, a necessity of civilized life.

Early in the present century Davy published an account of a discovery he had made of the effect produced on the nervous system by

the respiration of nitrous oxide, a substance due to chemical research. It was ascertained that the inhalation of the vapor of ether, another chemical product, produced a similar effect, and these facts, many years afterwards, were applied by Jackson and Morton, in our own country, for the purpose of producing insensibility to pain, and thus to relieve an incalculable amount of human misery, and to ameliorate in a measure the original curse to which our race has been subjected.

Dr. Priestley, in the course of a laborious series of investigations relative to the different kinds of air, subjected, on the 1st of August, 1774, to the heat of a burning lens (which is now, through the liberality of one of his grandsons, the property of this institution) a quantity of calcined mercury, and evolved from it a gas since known by the name of oxygen, a discovery which led to a knowledge of the composition of the atmosphere, and finally to the improvement of almost the entire circle of the chemical arts.

About the middle of the last century Franklin devoted his sagacious mind to what was deemed by some of his friends a trifling pursuit— the study of the phenomena produced by the friction of different substances when rubbed together. But from this investigation he deduced his admirable theory of electrical induction, and the fact of the action of points at a distance, on which was founded the protection of buildings from lightning, and which, with the additional discoveries of Volta, Oersted, and others, has given to the world the electrical telegraph.

These are instances of investigations commenced without any idea of immediate practical utility. They exemplify discoveries made by men who studied science for its own sake, and received no other reward than the consciousness of enlarging the bounds of human thought, while it was left to others to gather a rich pecuniary harvest from what they had so effectually sown.

"It is the destiny of the sciences," says Fontanelle, "which must necessarily be in the hands of a few, that the utility of their progress should be invisible to the greater part of mankind, especially if those sciences are associated with unobtrusive pursuits. Let a greater facility in using our navigable waters and opening new lines of communication but once exist, simply because at present we know vastly better how to level the ground and construct locks and flood-gates— what does it amount to? The workmen have had their labors lightened, but they themselves have not the least idea of the skill of the

geometer who directed them; they have been put in motion nearly as the body is by a soul of which it knows nothing; the rest of the world has even less perception of the genius which presided over the enterprise, and enjoys the success it has attained only with a species of ingratitude."

But it is not alone the material advantages which the world enjoys from the study of abstract science on which its claims are founded. Were all further applications of its principles to practical purposes to cease, it would still be entitled to commendation and support on account of its more important effects upon the general mind. It offers unbounded fields of pleasurable, healthful, and ennobling exercise to the restless intellect of man, expanding his powers and enlarging his conceptions of the wisdom, the energy, and the beneficence of the Great Ruler of the universe.

From these considerations, then, and others of a like kind, I am fully justified in the assertion that this Institution has done good service in placing prominently before the country the importance of original research, and that its directors are entitled to commendation for having so uniformly and persistently kept in view the fact that it was not intended for educational or immediately practical purposes, but for the encouragement of the study of theoretical principles and the advancement of abstract knowledge.

Smithson declares his bequest to be for the increase of knowledge and the diffusion of this among men, being well aware that a single new truth added to the general stock must affect man for good in all times and all places. We doubt not that when the importance of the abstract speculations of science is more generally and more justly appreciated, individuals who are favored by Providence with those peculiarities of mind which fit them for the advancement of science will be set apart as the priests or interpreters of nature, and be furnished liberally with the means necessary to benefit their fellow men by the discovery of new principles. The grand philosophical vision of the father of modern science, which has waited so long for its fulfilment, will then be realized, "by the union and co-operation of all in building up and perfecting" that House of Solomon, (as Bacon quaintly termed it,) "the end of which is the knowledge of causes and of the secret motions of things, and the enlarging of the bounds of human empire to the effecting of all things possible."

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Publications.—The publications of the Institution are now divided into three classes: the "Contributions to Knowledge," in quarto form; the "Annual Reports" to Congress, and the "Miscellaneous Collections," in octavo.

The eleventh volume of Smithsonian Contributions is nearly ready for distribution, and will contain a number of original memoirs, which are presented to the world as additions to knowledge of sufficient importance to warrant their publication by the funds of the Institution. The fact, however, should be recollected that the Institution does not merely publish these volumes, but, as a general rule, extends its assistance to the original researches of which the papers published contain the results, sometimes by furnishing the subjects or materials of observation, and sometimes by defraying the whole or a part of the expenses incident to such researches.

The first memoir contained in this volume is on North American Öology, by Dr. Thomas M. Brewer, of Boston, an account of which was given in a previous report. The text of this work was printed in 1857, but the preparation of the plates to accompany it not being completed, it could not be included in any volume previous to the eleventh. Copies, however, of the paper had been presented separately to some of the principal naturalists of this country and Europe, and the work has been received with approbation as an important addition to the branch of natural history on which it treats.

The second paper is on the total eclipse of the sun, September 7, 1858, as observed near Olmos, Peru, by Lieutenant Gilliss, United States navy, illustrated by a plate of the appearance of the sun during the total obscuration. A full account of this paper is given in the last report of the Institution.

The third memoir in the eleventh volume has the following title: "Discussion of the Magnetic and Meteorological Observations made at the Girard College Observatory, Philadelphia, from 1840 to 1845, by A. D. Bache, LL.D." Part 1. Investigation of the eleven-year period in the amplitude of the solar-diurnal variation, and of the disturbances of the magnetic declination.

About twenty years ago the British Association organized a series of cotemporaneous magnetic and meteorological observations at different colonial positions in the British empire, with which most of the civilized governments of the world co-operated. No assistance,

however, was rendered to the enterprise in this country, except in the instance here referred to, in which the observations were conducted by Dr. Bache, at Philadelphia, by means of funds supplied by the members of the American Philosophical Society and the Topographical Bureau of the United States, and with instruments furnished by Girard College. This series of observations commenced in May, 1840, and, with short interruptions, terminated in June, 1845, thus furnishing a record extending over five years, for three or four months of which the observations were made bi-hourly, and for the remainder of the time hourly. A general reduction of these observations was published in 1847, by order of Congress, in three octavo volumes, with an atlas of diagrams. The records, however, contained facts of great interest, which, owing to the laborious duties of Professor Bache, could not then be deduced from them, and he has since renewed the investigation, with the aid of Mr. Schott, and the present paper gives an account of the first results which have thus been obtained.

To present the bearing of the interesting researches exhibited in this paper on the progress of science, it may be proper to state that the magnetic force of the earth is almost constantly disturbed, both in direction and intensity.

1. It is subject to a change which appears to complete its cycle in a large number of years, for the determination of which it is necessary to know the magnetic state of various places on the globe simultaneously at a given epoch, and again after the lapse of several years. 2. It is subjected to a change which is completed in the course of a year; and 3d, another which runs through its course in a single day.

Beside these regular disturbances, there is another series of variations, large in magnitude, denominated magnetic storms, which have been, until lately, considered as fitful, appearing to observe no law, but which were manifest over a considerable part of the earth's surface. These, however apparently irregular as to the individual instances, are in all probability, as has been shown by General Sabine, subject to a law of more frequency of occurrence in certain years.

The object of Professor Bache's paper is to investigate from the data furnished by the Girard observations, the law of recurrence of the latter disturbances. Since this has not as yet been accurately ascertained, and every independent series of observations when properly discussed is of great value in giving more precision to our

knowledge of one of the most remarkable classes of phenomena presented in the whole course of physical science, the results of this discussion cannot but be received with much interest by the scientific world.

As the magnetic needle, for example, may be considered as subjected at the same time to different forces, each tending to produce one of the variations we have mentioned, it becomes a subject of nice inquiry to eliminate the several effects, and to obtain the magnitude and period of each separately. In the case under consideration it was necessary to separate more especially the large apparently fitful variations from the regular daily ones. To effect this, the process proposed by Professor Peirce, of Cambridge, and founded on the doctrine of probabilities, was employed as a criterion in judging as to the magnitude of a disturbance which should be considered as belonging to the class under consideration, and it was finally concluded that all disturbances which exceeded 3′.64 of an arc were abnormal, and accordingly all observations differing by that amount or more from their mean monthly values were marked. Next, a new hourly mean was taken, omitting the values so marked, and each observation again examined in reference to deviations from this new mean, and so on— the last mean thus obtained for each hour during each month gave what was considered the normal daily curve.

From this it appears that the north end of the needle reaches its greatest eastern position between 7 and 8 o'clock in the forenoon, and its greatest western deviation about 1 o'clock in the afternoon.

The author next proceeds to discuss the large disturbances, and from these he deduces the fact that a principal maximum of disturbances occurs in October, a smaller one in April, and the two minima, nearly equal to each other, occur in the months of February and June.

The diurnal variation arising from the large disturbances presents one maximum and one minimum; its most prominent feature is the easterly deflection, which occurs about a quarter after 8 o'clock p. m., at which hour the maximum deflection amounts to 32″ of an arc; the great westerly deflection takes place at a quarter past 6 a. m., and on an average amounts to 14″.

These variations are compared with deductions made from similar observations at Toronto, and are found to be the same in kind, but less in magnitude.

The whole discussion clearly indicates a law of recurrence in the frequency of the large disturbances, although the period over which the observations extend was not sufficient to determine the interval. The observations, however, indicate with great precision the time of the minimum, the rate of diminution as the disturbances diminish in approaching this period, and their increase as they recede from it. The minimum thus found, of frequency of large disturbances, occurred in August, 1843.

The establishment of the elements of a law of periodicity in relation to changes of the magnetic force which, from the time they were first noticed until within a few years past, were regarded as entirely irregular, is in its relation to terrestrial magnetism a fact of importance; but the value of this is highly increased when it is found that these disturbances are connected with changes in matter foreign to our earth. To realize this, we must refer to a series of persevering observations made day by day for thirty years on the spots of the sun, by an astronomer named Schwabe, in an obscure town of Germany. This devotion to an apparently unfertile field of inquiry was finally rewarded by the discovery that the spots on the sun's disc are subject to a regular law of recurrence, and that they pass through the phases of periods of greatest and least frequency in about eleven years; but strange to say, it was afterwards announced by General Sabine that the period of recurrence of large magnetic disturbances coincides both in duration and its epoch of maximum with the period discovered by Schwabe in reference to the solar spots; that is, that at the period of greatest disturbances there occurs the maximum number of spots, and *vice versa*. The investigations of Professor Bache serve to establish this conclusion, and to furnish additional elements for a more accurate comparison. From these results it is clear that the sun exerts an influence on the magnetism of the earth which depends on the existing state of its own luminous atmosphere, affording another example to be added to other illustrations of the same truth, that scientific researches, if skilfully and perseveringly continued, will always lead to valuable results, and often to those which could not have been anticipated by any previous conceptions.

The volumes of records of the Girard observations, which present on casual examination immense series of tabulated figures in which no law or regularity is observable, when scientifically studied and properly interpreted, are thus found to yield truths of the highest

interest. Professor Bache proposes to continue his inquiries and extend his investigation to the influence of the moon and other agents on the magnetism of the earth. He has already finished a second paper on these discussions, and has a third in a state of considerable advancement. These will probably form a part of the twelfth volume of the Contributions.

The eleventh volume also contains a second series of the discussions of the physical observations made by Dr. Kane during his last voyage to the Arctic regions, the first part of which, or that relating to terrestrial magnetism, was published in the tenth volume of Contributions. This second part relates to meteorology, and was prepared for publication in the intervals of his official duties by Chas. A. Schott, esq., assistant in the United States Coast Survey, under the direction of Prof. Bache, and at the expense of the Smithsonian funds. This memoir not only forms an interesting and important addition to meteorology, which will tend to connect the name of our lamented countryman with this branch of science, but also furnishes a model for imitation, of the method in which observations of this character ought to be reduced and discussed in order that they may best subserve the advancement of science.

The following account of some of the points of the memoir and of the facts developed will probably be generally interesting and serve to illustrate its value:

The observations were made at Van Rensselaer harbor, on the western coast of Greenland, and extend over a period of two winters and a considerable part of two summers, during which the vessel was constantly frozen in the ice.

They show in a very striking manner the constant and laborious occupation of the little party in their lone abode, records having been made at every hour of the day and night during the whole period. It would be out of place in this report to give a full account of all the subjects discussed in this memoir, and I shall therefore only glance at a few of the most prominent points, referring to the paper itself for a full exposition of its more valuable contents. It consists of three parts—the first is on temperature, the second on winds, the third on atmospheric pressure. The first part, viz. that on temperature, gives the observations for every hour, from which are deduced the diurnal and annual variations of the thermometer, the influence of the different winds on the temperature, and an

analysis of the recurrence of cold periods during the winter; tables deduced from the daily observations for ascertaining the corrections required to be applied to observations made only once or twice a day in order to obtain the mean temperature of places within the arctic circle; and, finally, observations to determine the diminution of temperature with an increase of elevation.

Beside the corrected records of the motion of the air, the second part of the memoir contains the resultant direction, the average force, the mean velocity, the quantity, the frequency, and the duration of the winds. The third part contains not only the record of the pressure, but also a comparison of the mercurial and aneroid barometers, the diurnal and annual variation, the regular fluctuations of the monthly and annual extremes of pressure.

The expedition was supplied with thirty-six mercurial thermometers, four maximum and minimum thermometers, twenty-four spirit thermometers of different sizes, including two standards and a register thermometer of thirty-six inches in length. A laborious series of the different readings of these instruments, particularly at low temperatures, was made, from which have been deduced corrections to be applied to the records prepared for publication. The differences exhibited by the spirit thermometers at low temperatures was referred to the unequal contraction of colored alcohol not chemically pure. This liquid, when exposed to a great degree of cold, appeared to change its condition, the coloring matter being deposited on the sides of the tube. The lowest temperature observed during the first winter, 1853–'54, was, February 6, —66°.4; and during the second winter, 1854–'55, occurred, January 8, —65 .5.

The highest temperature observed was July 23, 1854, +51°, giving an absolute difference of 117°.4. The diurnal maximum or highest temperature of the day occurred in October and November, about one hour before noon, and in April and May, three hours after noon.

In the months of October, November, and December there are two points of low temperature each day—one at 6 a. m., and the other at about 9 p. m.; during the remaining months of the year there is one minimum during the twenty-four hours, which occurs at 1 a. m.

It was a fortunate circumstance that the observations extended over two winters, and thus gave a more exact mean for that season. The warmest month is July, the coldest March; the temperature of December, however, does not differ much from the latter. The

highest mean monthly temperature seems to occur almost exactly in the middle of July, and the lowest point would probably have been found in February if the series had been extended over several winters. The mean temperature of winter, namely, of December, January, and February, was —28°.59; of spring, —10°.59; summer, +33°.38; autumn, —4°.03. The mean temperature for the whole year was —2°.46. The temperature was always lowest during calms, and rose with the springing up of a wind from any quarter.

There is also a great regularity in the elevation of temperature during the hours of the fall of snow; on an average the sensible heat was increased during this period 7°.7. In seventeen months it snowed during six hundred and eighty hours, and rained during sixty hours.

A series of recurring periods of cold was observed, which Dr. Kane seemed inclined to consider as intimately connected with the phases of the moon, and on this point a series of elaborate investigations was made by Mr. Schott, from which it was found that in a period of six days on an average the cycle was completed, and that the lowest temperatures are reached about the time of full moon. Setting aside some small deviations in the regularity of the curves of temperature, there is not a single exception to the correspondence of the greatest cold near the epoch of full moon, and of least cold near the time of the new moon. It should be observed, however, that since, from the observations made at this Institution, the waves, as it were, of cold air which reduce the temperature of the United States, frequently begin several days earlier at the extreme west, the same coincidence as to identity of occurrence of the maximum cold with any particular phase of the moon cannot be true of all points on the surface of the earth, although the period of recurrence may, as in the case of the tides at different places, be governed by that luminary.

A series of comparative observations at the level of the sea and at the top of the mast of the brig, at eighty feet elevation, was taken during the months of August, September, and October, from which is deduced a diminution of temperature of 1° for two hundred and ten feet of elevation.

The direction of the wind was noted in the original records with reference to the magnetic points of the compass, and the mean results

determined in regard to the true north. It appears from all the observations that the true direction of the wind is from the eastward, varying in the several months northward and southward. There is but one exception; namely, in June; the wind then veers round to the westward of south. The resulting direction for the whole year is almost exactly east; in winter it is E.N.E. and in summer S.E. by S.

The greatest quantity of air which moves over the place during the year comes from a direction north of east.

The predominance of calms is a circumstance quite characteristic of this region. The number of hours of winds recorded was 3,697, and those of still weather 5,063.

The snow or rain wind is between N.N.E. and E.S.E., or from the direction of the Spitzbergen sea, and also from the opposite direction of S.S.W., or that of the upper Baffin's bay. From the northwestern quarter there was hardly any precipitation.

During the whole period there were recorded thirteen gales, with a duration of not less than two hours. They do not appear to be confined to any particular season of the year, and on the average continue about seven hours.

These records are of great interest in enabling us to ascertain whether the great storms which pass over the United States can be traced into the Arctic regions.

For observations on atmospheric pressure the expedition was provided with a mercurial barometer and two aneroids, and from a series of reductions of the observations of these instruments it is concluded that the indications of the aneroid may generally be relied on to within nearly one hundredth of an inch.

Owing to the small amplitude in the oscillations of the barometer, and the magnitude of occasional disturbances, the law of diurnal variation is apparently subject to considerable fluctuations. The principal maximum is reached about one o'clock p. m.; the evening maximum at about ten p. m., in conformity with the general law deduced from observations in the northern hemisphere. The one p. m. minimum seems to occur about three hours earlier than is indicated at more southern stations.

The average maximum height of the barometer is above the mean in the months of January, February, March, April, and May, and descends below the mean in the remaining summer and autumn months. The general law observed in other parts of the world, that the height

of the barometer is less in summer than in winter, is prominently exhibited.

The mean height of the barometer for the whole time was 29.775 inches, which is less than that for places under the tropics; and it should be stated that Van Rensselaer harbor is fourteen degrees farther north than the latitude 64° in which the height of the barometer is a minimum.

The fluctuations in the height of the barometer were greater in winter than in summer. The greatest pressure, 30.97 inches, occurred in the morning of January 22, 1855; the lowest, 28.84 inches, occurred near noon of February 19, 1854. Little change was observed in the barometer during the fall of snow.

The barometer fell during the blowing of all the winds except those from about north by east and southeast.

The observations indicate that the hottest winds are from the northeast, one-half east, and the maximum atmospheric pressure nearly east.

This memoir was referred for critical examination to Professor Peirce, of Cambridge, and Professor Chauvenet, of St. Louis.

The next memoir is by Dr. John Le Conte, of Philadelphia, and is intended to give a catalogue of the Coleoptera or beetles known to inhabit the middle and eastern portions of the great central region of temperate North America. The province here treated of includes Kansas, a portion of Nebraska, and the eastern part of New Mexico. Its eastern limit is well defined, but its other boundaries are indefinite, since it there fades imperceptibly into other provinces of the same great zoological district. The descriptions of new species are principally from specimens furnished through the Smithsonian Institution from collections made by different explorers connected with the surveys of the officers of the United States army.

Before proceeding to describe the special materials used in the preparation of the memoir, the author gives a short sketch of the results already obtained in regard to the geographical distribution of coleopterous insects in this country, illustrated by a map on which the several regions are distinguished by different colors. From this map it appears that the whole area of the United States is divided by nearly meridianal lines into three or perhaps four zoological districts, distinguished each by numerous peculiar genera and species, which, with few exceptions, do not extend into the contiguous districts.

These districts are divided into a number of provinces of unequal size, which are limited by differences in climate, and are therefore sometimes distinctly and sometimes vaguely defined.

The mode of distribution of species in the Atlantic and Pacific districts is entirely different. In the Atlantic districts a large number of species are distributed over a great extent of country; many species are of rare occurrence, and in passing over a distance of several hundred miles but small variation will be found to exist. In the Pacific district a small number of species are confined to a limited region of country. Most species occur in considerable number, and in travelling even one hundred miles it is found that the most abundant species are replaced by others, but of a similar character.

The object of the memoir is to give an account of what is known of this class of insects in Kansas, upper Texas, and Arizona, and to furnish means for facilitating the further exploration of the whole country in regard to the same animals.

This will undoubtedly be considered a valuable addition to a branch of zoology which, however insignificant it may appear to the popular mind, is not only connected with questions of interest in relation to abstract science, but also with the economical resources of the country.

The memoir, beside the colored map to which we have alluded, is illustrated by two engraved plates.

The next paper consists of the result of magnetical and hypsometrical observations in Mexico, to which is appended notes on the volcano of Popocatepetl and its vicinity.

In 1856 Baron Von Müller undertook an exploration of Mexico in reference to its natural history, and proposed to the Smithsonian Institution to make in its behalf a magnetic survey of the same country. This offer having been accepted, an appropriation was made from the Smithsonian fund to pay a portion of the salary of Mr. Sonntag, the assistant of Baron Von Müller; and the magnetic instruments which had been previously lent to Dr. Kane, and used by Mr. Sonntag himself, as one of the assistants of that lamented explorer in his last Arctic exploration, were furnished to the expedition for the contemplated survey. Several records of the unreduced observations made at a number of places in Mexico, were at different times transmitted to the Institution previous to the return of Baron Von Müller to Germany, after which nothing more was obtained; and after considerable delay

we were informed by him that he had been robbed in Mexico, and that the instruments had been captured and destroyed. Not having received a final report from Baron Von Müller, to render the observations which had been obtained from Mexico available to science, they were placed in the hands of Mr. Sonntag on his return to Washington, and have been reduced by him at the expense of the Institution. He has also appended a series of notes relative to the volcano of Popocatepetl and its vicinity, and also a series of barometrical and trigonometrical measurements of heights of various places in the vicinity of the city of Mexico. The observations included those for the declination or variation, the inclination or dip, and, lastly, those for the relative intensity of the magnetic force. A series of observations for each of these elements was made at the following places, namely, Vera Cruz, Potrero, Cocolapam, San Andres, Mirador, city of Mexico, Chalco, and Tlamacas. The average variation of the needle from the whole series of observations was about $8\frac{1}{2}°$ east; at the city of Mexico it was $8° 46'$ east. The average dip for the whole region was about $42\frac{1}{2}°$, and for the city of Mexico $41° 26'$.

The interesting fact is noted in the appendix that the southwestern wall of all the recent Mexican craters observed by the author is higher than the northeastern wall—a phenomenon probably due to the action of the trade-winds constantly impelling the ashes and cinders from the northeast to the southwest. The elevation of eleven different places was determined, including the city of Mexico and the highest peak of Popocatepetl. The former was 7,472.8 feet, and the latter 17,817.6 feet.

We regret very much the loss of the magnetic instruments, not only on account of the use which might be made of them in determining the magnetic elements of different portions of the American continent, but also on account of the interest which attaches to them from having been employed in the observations by Dr. Kane. They have, however, done good service; and although the result of the co-operation of Baron Von Müller has not been as fortunate as we could have wished, still it has added something of considerable value to our knowledge of the terrestrial magnetism of this continent.

Another memoir, which will form a part of the 11th volume of "Contributions," is on the American storm of December 20, 1836, and the European storm of December 25 of the same year, by Professor Elias Loomis, of the University of New York.

About twenty years ago this industrious meteorologist presented to the American Philosophical Society an investigation of the first of the above-mentioned storms, which extended from the Gulf of Mexico to an unknown distance to the north. The area covered by the observations, and to which consequently the investigation was confined, included only the southern part of the storm, and therefore the author regarded the results he had obtained, though of sufficient interest to warrant publication, as not entirely satisfactory. Having since obtained additional information, and adopted with success in the study of similar storms a method of investigation which consists in representing the disturbances of the atmosphere by lines and colors on charts, he concluded to review his former labors, and to publish all his results illustrated by a series of colored maps, which he is now enabled to do through the provision made for this purpose by the Smithsonian Institution.

The author first presents a summary of the observations of the barometer at each of the American stations from which information in regard to the indications of this instrument was derived. The average height of the barometer at each of the stations is given, and the fluctuations from this height during the storm, as well as immediately before and after it. With these data a series of lines is drawn on five charts, exhibiting the progress of the storm for as many successive periods, namely, for December 19, at 8 o'clock p. m.; December 20, at 8 a. m. and 8 p. m.; December 21, at 8 a. m. and 8 p. m. On each chart is drawn a line indicating the places of mean pressure of the atmosphere, or those where the pressure is in a normal condition, also a line indicating two-tenths of an inch of mercury above the mean, &c.

In examining these lines on each map, it is apparent that there exists a large area over which the barometer was below its mean height, which on the evening of the 20th of December extended 980 miles from west to east. On the morning of December 21 it extended 770 miles, and on the evening of the 21st it had become reduced to 600 miles in the same direction. It is evident, also, that toward the north the limit of low barometer extended much beyond the map, and, since the lowest point was found at Quebec, it is inferred that it extended as far north as it did south of this point, and would, therefore, be on the 21st of December at least 3,000 miles in length from north to south. The area, therefore, of least pressure was in the

form of an oval figure, having a length of three times its breadth, and, from the inspection of the several maps, it will appear that it travelled constantly eastward.

A similar table of observations is given in reference to the thermometer as observed at fifty-seven places, and lines corresponding to the mean temperature to ten degrees above and ten degrees below, &c., are drawn on each of the maps. From these it appears that on the evening of the 19th the area of greatest temperature extended from 800 to 1,100 miles in an east and west direction. The centre of this area of high thermometer did not coincide with the centre of low barometer, but was uniformly somewhat to the east of it.

On the same charts the condition of the weather at different intervals as to clearness, cloudiness, rain, &c., is represented by different colors, and from these it will be seen that on the evening of the 19th of December rain or snow was falling over the entire region west of the Mississippi as far as the map extends, and that a cloud covered the whole of the United States except that part bordering on the Atlantic ocean. On the evening of the 20th the rain had reached Washington, and on the morning of the 21st the cloud had covered the whole of the eastern portion of the country, while the sky had cleared off as far as Cincinnati, at the same time that rain was falling in the whole of New England except the State of Maine. On the evening of the 21st the storm was confined to a small portion of the eastern part of the chart, while the sky over nearly the whole United States, with a few exceptions of spots of limited extent, was free from clouds.

The direction of the wind is indicated by arrows, and its intensity shown by their length; and from these it is seen that during the entire period within the area of rain and snow the direction of the wind in the rear of the storm was from the west, northwest, or north, and that in the southwest part of the United States the winds were somewhat more northerly than in the northwest part of the country. In front of the storm the winds generally blew from a southerly point, the average of which was 10° east of south, while in the south and northern parts of the country in front of the storm the wind was easterly. There was thus along a meridian line of at least 1,200 miles in length a violent wind from a point on an average 30° north of west, and on the east side a strong current from a point 10° east of south. These two contrary winds blew with great violence for at

least forty-eight hours, indicating an ascending current of air and a tendency to rotation contrary to the motion of the hands of a watch.

The author next proceeds to offer some suggestions as to the origin of the storm, which in the main agree with the exposition of the phenomena as given in the theory of Espy, namely, the upward motion of the air at the point of lowest barometer, the evolution of latent heat by the condensation of the vapor which it contains, and the transfer eastward of the whole disturbance by the flow of the upper current in that direction. He attributes the principal cause of the cold experienced to the upper current descending to the surface of the earth.

The European storm of the 25th of December of the same year is investigated in the same manner, and the results illustrated by means of eight separate maps, one for each day, commencing with December 21, including the beginning and ending of the disturbance. It was at first supposed that this storm was a continuation of that of the 20th experienced in America; the rate of progress, however, of the latter was such that it could not have reached Europe before the 27th, whereas the storm of the 25th was fully organized on the 23d, and, indeed, its first movements can be distinctly traced in Germany on the 22d. The European storm evidently originated in Europe, and the American storm wasted itself in the Atlantic. It was, however, the possible connexion of these two storms that induced Professor Loomis to collect particularly, during a visit to Europe in 1856–'57, records of meteorological observations for this period. The whole number of stations from which he obtained data was nearly fifty.

It was mentioned in the last report that the Institution had concluded to publish in full detail several series of meteorological observations made for long periods in different parts of the United States.

The 12th volume of Contributions will contain the first of these series, by Professor A. Caswell, of Brown University. The observations commence with December 1, 1831, and end with December 31, 1859,* including a period of a little more than twenty-eight years.

These observations were made three times daily, embracing the thermometer, barometer, the direction and force of the wind, the

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* These tables have been extended to June, 1860.

degree of cloudiness, the amount of precipitation, and, for a portion of the time, the psychrometer.

In addition to these fixed and regular observations, daily and special notice was taken of all phenomena connected with atmospherical changes, with storms, the aurora, &c. The barometrical observations are reduced to the level of the sea and the temperature of 32° Fahrenheit.

The author has given a series of general summaries deduced from the whole period; and when a sufficient number of similar observations for long periods are collected and published, they will be submitted to the process of exhaustive investigation similar to those to which the observations of Dr. Kane have been subjected, in order to determine peculiar points of interest relative to the climate of the United States.

We have found that the printing of papers of this character requires much time and is very expensive, since they are composed almost entirely of rule and figure work. We think, however, the value which will be attached to them will fully warrant the expenditure on account of their publication. They will afford the data for answering many questions which are propounded to the meteorologist, such as the period of recurrence of storms, the connexion of the changes of the weather with the phases of the moon, &c. The means of ascertaining the state of the atmosphere over a considerable portion of the United States on any day for the last quarter of a century will be interesting in many cases, independent of scientific considerations.

The report of the Regents to Congress for 1858, besides an exposition of the condition and operations of the Institution for that year, was, as usual, accompanied by an appendix containing the report of lectures, and other matter which has proved highly acceptable to a large number of intelligent persons in every part of the country. These reports, copies of which are especially solicited by teachers, besides furnishing valuable knowledge not otherwise readily attainable, serve to diffuse information as to the operations of the Institution which tends to increase the number of its friends and co-operators, and to elevate popular conceptions in reference to science, as well as to increase the number of its cultivators.

The number of copies of the report ordered to be printed at the last session was less than that of the preceding year, yet the supply

to the Institution was the same. Indeed it is a gratifying evidence of the public estimation in which the Institution is held, that Congress has been so favorably disposed, even during the depressed condition of the treasury, towards the distribution of this document.

It was mentioned in the last Report that the Institution had made arrangements for the preparation of elementary treatises on the different orders of insects found in North America, with a view to identify the species and facilitate the study of their relation and habits. Considerable progress has been made in this work, and several parts are either in the press or ready to be given to the printer. These treatises will afford the means of instructing the farmer in regard to the character of the insect enemies with which he has to contend, and will enable him by watching their habits, time of appearance, and mode of propagation, to add much valuable information to what is already known relative to the method of guarding against their ravages.

But before anything of this kind can be done systematically, we must be able to recognize the insects, since the same animals are known in different countries by different names. If, therefore, we would avail ourselves of the facts which have already been gathered by patient study in this branch of natural history, we must be assured of the identity of the species; and if we would make the knowledge which already exists in this country generally available, the insects must be described with that scientific precision which will enable them to be immediately recognized with certainty in every part of the world.

These treatises or catalogues will be illustrated by wood-cuts, and published as a part of the Miscellaneous Collections of the Institution.

The last Report contains a series of instructions for collecting and preserving specimens of insects, prepared by the following gentlemen, viz: Dr. John L. Le Conte, of Philadelphia; Dr. B. Clemens, of Easton, Pa.; Dr. P. Uhler, of Baltimore; and Baron R. Osten Sacken, of the Russian Legation. These instructions have not only directed attention to the subject, but furnished the means by which a large number of specimens have been obtained for scientific investigation.

It will be recollected that it is one of the objects of the Institution

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to induce as many individuals as possible, in various parts of the country, to devote their leisure hours to special objects of natural history—to point out to them the pleasures derived from studies of this kind systematically pursued, and the important results which will flow from their labors when combined with those of other persons in the same line, and also to facilitate by catalogues, descriptions, and correspondence, the progress of the student in the elementary part of his studies. In connexion with this object, circulars have been distributed directing special attention to different points, among which we may mention one on American grasshoppers, an insect to the ravages of which a large portion of the United States is frequently subjected, and relative to which every well authenticated fact is of considerable interest. Another circular has been issued in regard to the collection of nests and eggs of birds, to furnish the material for a continuation of Dr. Brewer's work on Oology.

The fact was stated in a previous report that materials had been collected for a new edition of a report on the libraries of this country, originally prepared by Professor C. C. Jewett. This work was entrusted to Mr. William J. Rhees, chief clerk of this Institution, to be done out of the usual hours of his official duties; but the materials which were collected contained so much information relative to educational and other institutions, which was thought too important to be omitted, that the report when completed was found to exceed the limits assigned by the Institution; and rather than abridge it by leaving out a part of the material which had cost so much labor, Mr. Rhees offered to publish it on his own account; and such an arrangement being compatible with the general policy of the Institution, the proposition was agreed to, and the work has accordingly been issued under his own name and responsibility. It forms a volume of 700 octavo pages, and contains a large amount of very interesting and valuable matter which has cost the author a much greater amount of labor than can ever be repaid by even an extensive sale of the work.

In this connexion we may mention that a list of the libraries, societies, and institutions in North America, has also been prepared by Mr. Rhees and printed for the use of the Institution. It forms an octavo pamphlet of 81 pages, and is found of much value in facilitating the distribution of our several classes of publications and in directing circulars, &c.

Researches.—Mr. L. W. Meech has continued his mathematical researches in regard to the light and heat of the sun, and, since the date of the last Report, has succeeded in integrating some of the analytical expressions which had previously appeared likely to prove exceedingly troublesome, and the analysis is now sufficiently advanced for another publication. His next memoir will treat of the relative intensity of the sun's rays after passing through the air to the earth's surface. It will be recollected that his former memoir presented, in tables and curves, the intensity of the sun's rays at the exterior of the atmosphere. The primary formula, to be given in this memoir, has been demonstrated and verified, and the derived formulæ are mostly made out for the range and other phases of the intensity of the light and heat. These depend on what are called elliptical functions, and are much more complicated than those of the former paper. Before curves can be drawn from them, the numerical values for every five degrees of latitude are to be computed and checked, which will require the labor of several months. To defray the expense of this, another small appropriation will be required. The success of the previous labors of Mr. Meech warrants this expenditure, from funds intended for the increase of knowledge, since the results which can now be obtained from his formula will, in all probability, be considered standard elements in the physical theory of heat.

Dr. Wolcott Gibbs has continued his chemical researches, and a paper in relation to them will probably appear in the next volume of Contributions. It will present new processes for the separation of all the platinum metals in a state of absolute purity. These are very simple, and easy of execution, and not only apply to the separation, but to the quantitative analysis of mixtures of the different metals of this group in almost any proportion. The researches also involve the preparation and properties of a new and remarkable series of salts, which, it is thought, will remove the difficulties with which the subject has hitherto been surrounded.

It was stated in the last Report that one of the most important operations in which the Institution had been engaged during the previous year was the construction of a map to present at one view the arable, forest, and sterile portions of the United States. The design at first was merely to exhibit the limits or boundaries of these portions of the country, and this has been faithfully executed by Dr. J. G. Cooper, to whom the work was intrusted, as far as the materials

could be gathered from all the accessible published data, the records of the Land Office, and other sources. The facts presented at once to the eye by this map are in striking accordance, as we have before mentioned, with the deductions from the meteorological materials which have been collected at this Institution, and serve to place in a clear point of view the connexion of climate with the natural productions of different parts of the earth. The plan has, however, since been enlarged, and Dr. Cooper now proposes, with the aid of the Institution, to construct a map which shall give in detail the distribution of the several kinds of trees and shrubs found in different portions of the country; and, in view of this, he has prepared an article, which has been published and widely distributed by the Institution, containing a list of the localities of the most important and useful trees and shrubs, as far as known, and asking additional information. The chief difficult$_J$ in carrying out the plan has been the want of definite knowledge as to the locality of different plants; for example, a plant is mentioned as occurring in Virginia, but this statement is not sufficiently precise, since this State occupies a large surface, on a small portion of which only the plant may be found. Facts are also required as to the abundance of trees in a given locality.

The collection of the material for a map of this kind, in connexion with a work on the forest trees of America, still in progress by Dr. Asa Gray, of Cambridge, is a very important matter both in a political and an economical point of view, and the work might be materially aided, without much expense to the government, by appending a few additional queries to the questions propounded by the marshals who collect the statistics of the census. The outline map, which has already been prepared at the expense of the Institution, has excited much interest, and the proposition to enlarge the plan of the work has been received with commendation.

As an interesting object in regard to physical geography, and intimately connected with meterology and various branches of natural history, a commencement has been made, in connexion with the Coast Survey, in collecting materials for the construction of a hypsometrical map of the United States.

No part of the surface of the earth, of equal dimensions, has been so extensively traversed by lines of explorations for canals, railways, and river improvements, as the United States. The materials, however, which are afforded by these, for constructing a map of the ele-

vations and depressions of the surface of the country are widely scattered, and unless an effort be made to collect them will ultimately be lost. Previous to the connexion of the Institution with this enterprise, circulars were sent by the Coast Survey to engineers and directors of public works, in answer to which replies were received giving the elevation of a large number of points. Since this connexion another circular has been issued, to which a large additional number of answers have been received. The whole number of points heard from is about 9,000. Many of the replies to the circulars have been accompanied by valuable topographical information and maps, some of which, as testified by the contributors, were rescued from the oblivion which has been the fate of the records of many of the earlier surveys. For the exhibition of these points, in connexion with the topography of the country, it is proposed to have them plotted on a map consisting of two sheets, with a projection of $\frac{1}{3000000}$. One sheet is to show the surface east of the Rocky mountains, the lines of water courses, and is to be filled up from the best existing maps; the western sheet is to be copied from the map of the same scale, issued from the office of the Pacific Railroad explorations. An accurate outline map of the United States on this scale will be of great importance as a base-chart on which to delineate the result of various other statistical inquiries which have been instituted by this establishment.

Mr. Lewis H. Morgan, of Rochester, New York, having studied for several years the ethnological peculiarities of the Indians of the North American continent, has discovered among them a system of relationship which he wishes to compare with the systems of consanguinity existing among the natives of other countries, and the Institution, at his request, in order to aid in this research, has distributed circulars to our consuls, missionaries, and ethnologists in various parts of the world. The peculiar system of relationship of the Iroquois, one of the principal families of American Indians first attracted the attention of Mr. Morgan. The fundamental idea of this system, which is carried out with great logical rigor, is, that the bond of consanguinity is never suffered to lose itself in the ever diverging collateral lines—the degrees of relationship are never allowed to pass beyond that of first cousins; after that the collateral lines are merged in the lineal lines, so that the son of a cousin becomes a nephew, and the son of this nephew becomes a grandson. This principle extends upwards as well as downwards, so that the brother of a man's

father becomes his father, and the brother of his grandfather becomes also his grandfather. At first Mr. Morgan supposed this peculiar system to be confined to the Iroquois, but subsequent investigation developed the fact that the same system in its complexity and precision is common to all the Indian tribes of North America. It therefore becomes an object of interest to inquire whether the same system exists among the natives of any other country. It is proper to remark that, at the request of the Institution, General Cass, the Secretary of State, has given to this interesting inquiry the official sanction of his Department, and in a letter appended to our circular, has commended it to all the diplomatic agents of the government abroad.

Laboratory.—During the last year the laboratory has been under the direction of Dr. B. F. Craig, of this city, and, as in former years, a considerable number of specimens of the products of different parts of the country have been examined. The policy adopted from the first in regard to examinations of this kind is to furnish a report free of cost to the parties asking for the information, provided it is of general interest and immediately connected with the advance of science, and can be afforded at little expense to the Institution. If, however, the examination is required principally to promote private interests, a charge is made sufficient to cover the expense of the investigation. By the adoption of this policy, the laboratory is kept in operation by means of a small appropriation for chemicals and apparatus.

It may be proper to mention that during the year Dr. Craig has been engaged in investigations, on his own account, in the laboratory, and that Mr. J. H. Lane has made a series of experiments relative to different points connected with the Atlantic telegraph.

Magnetic Observatory.—The remaining instruments necessary to complete the equipment of the magnetic observatory established at the joint expense of the Institution and the Coast Survey were received and put into operation in the early part of the year; but as it has been found that the changes in the direction and intensity of the elements of terrestrial magnetism at Toronto, Philadelphia, and Washington are almost precisely the same, it has been considered that more important service would be rendered to the inquiries now being made in regard to this branch of physics, if the instruments were

placed in some more distant position, and it has been determined to send them to the Tortugas, a group of islands within the Gulf of Mexico, at which the United States coast survey has also a tidal station.   A single instrument, however, is still to be kept in operation at the observatory to record the changes in declination and to exhibit the perturbations connected with the appearance of the aurora borealis.

Exchanges.—The general system of international exchange, which has been the subject of so much attention heretofore, continues to increase in magnitude and importance.   By reference to the special report on this subject, it will be seen that the operations have been more extensive in 1859 than during any preceding year.   The facilities afforded to the operations of the Institution in the department of exchanges and of natural history by various transportation express companies, as referred to in the preceding reports, have been continued throughout the year.   The steamers of the North German Lloyd have carried a large amount of freight for the Institution, free of charge, between Bremen and New York, as promised by their directors, at the instance of Mr. Schleiden, minister from Bremen, resident in Washington.   The steamer lines to California, consisting of the UNITED STATES MAIL Co., M. O. Roberts, President; the PANAMA RAILROAD Co., David Hoadley, president; and the PACIFIC MAIL Co., W. H. Davidge, president, transported a large amount of material for the Institution up to the expiration of their mail contract in October, when the NORTH ATLANTIC STEAMSHIP Co., I. W. Raymond, president, replaced the United States Mail Co. in the line, which has since continued the same favors.   Mr. Bartlett, of New York, has offered the use of his line of ships to the west coast of South America, and the exchanges with Chili are now carried on chiefly through his vessels.   The steamer Isabel, Messrs. Mordecai & Co., of Charleston, agents, has transported a number of packages for the Institution between Charleston and Key West, free of charge, and Messrs. Russell, Major & Waddel, army contractors of transportation to Utah, have also rendered valuable service in this line.   The Adams Express Co., through Mr. Shoemaker, the superintendent of its southern division, has also exhibited liberality in reducing its charges greatly on heavy freights, and in carrying small parcels free of cost.   It is difficult to estimate the value to the Institution of all these services.   They are interesting as exhibiting a high appreciation of the mission and operations of the Institution, at the same time that they disclose a

spirit of liberality as gratifying as it is beneficial. It is believed that an amount of at least $1,500 has been saved by these free freights during the year, a sum which in effect may be considered as having been added to the income of the Institution, and thereby correspondingly increasing its means of usefulness. Acknowledgments are due to a number of persons in the United States who have assisted without charge in distributing the volumes of Smithsonian Contributions, viz: Hickling, Swan & Brewer, of Boston; D. Appleton & Co., New York; J. B. Lippincott & Co., Philadelphia; Russell & Jones, Charleston; Robert Clarke & Co., Cincinnati; and Bloomfield, Steele & Co., in New Orleans. Messrs. J. W. Raymond and Mr. W. H. Wickham, of New York, and Mr. A. B. Forbes, of San Francisco, agents of the California lines referred to above, have rendered services of a similar character.

It is gratifying to be able to repeat the statement made in previous reports, that all Smithsonian packages are allowed to pass free of duty and without examination at the custom-houses of all the civilized countries with which the Institution is in correspondence.

Library.—During the past year the plan adopted in regard to the increase of the library has been constantly kept in view, namely, to procure as perfect and extensive a series as possible of the transactions and proceedings of all the learned societies which now exist or have existed in different parts of the world. The library, in this respect, is now perhaps the first in the United States, and has increased, since the date of the last Report, not only by the addition of the current publications but by a number of new series and of volumes to complete sets hitherto imperfect.

The catalogue of the serial publications of foreign learned societies, State governments, universities, public libraries, and private parties, contained in the library, mentioned in the last report, has been published. It forms a book of 259 octavo pages and includes all the collections of the kind above mentioned, down to the middle of the year 1859. Copies of this have been sent to all the foreign societies, with a request that deficient series of volumes or parts of volumes be supplied, and that any works of the same kind which are not to be found in the catalogue be furnished from duplicates at the disposal of any of the establishments with which the Institution is in correspondence.

The distribution of this catalogue to the principal institutions of this country and abroad will not only facilitate the completion of the

design of forming as perfect a collection as possible of transactions, but will also render the library more generally useful to the cultivators of science in the United States. But the most important means of facilitating the use of a special library of this class of works will be that of the publication of the classified index of all the physical papers contained in the transactions, now in progress at the expense and under the direction of the Royal Society of London. This index is confined to papers relating to astronomy, mathematics, and general physics, and even with this restriction will include about 250,000 titles.

Professor J. Victor Carus, of Leipsic, informs us that during the last two years he has been collecting materials towards a general catalogue of zoological literature, from 1750 up to the present day, including not only all the separately published works, books, and pamphlets, but also and especially all the papers, notices, and articles contained in periodicals, the number of which is increasing every year. The titles and references will be arranged systematically, not according to the alphabet of authors, but within the classes and groups according to the alphabet of the genera, so that at a glance the whole literature of a particular genus may be found. The author calls for aid in obtaining information as to American zoological papers contained in periodicals difficult of access, and any one who can assist in this desirable object would confer a favor by forwarding to him, through the Institution, information bearing on this subject.

To assist in the same general plan of facilitating the acquisition of a knowledge of what has been done in different branches of science, the Institution has authorized the preparation of a bibliography of American botany, by Mr. Thurber, of New York, under the direction of Dr. Torrey.

Since the date of the last Report, the act amendatory of the law relating to the disposition of books intended for copyright has gone into operation. This act merely requires that one copy of every article intended to be secured to the author by copyright is to be deposited with the clerk of the district court from whom the certificate is obtained, and repeals the requirement that a copy should be presented to the library of Congress and to that of the Smithsonian Institution. All the books which have heretofore been deposited with the district clerks, and those which may be obtained in future, are to be arranged in the Patent Office, where they will form an

extensive collection, the support and preservation of which, however interesting to the bibliographer, are not in accordance with the general objects of the Smithsonian Institution, although as the means of securing evidence of title they are in strict conformity with the design of the Patent Office.

Few, comparatively, of the books which were received by the Institution under the operation of the copyright law were of any scientific value, and by far the greater number, consisting of elementary school books and publications designed especially for children, were entirely foreign to the plan of the library, and yet they have cost the Institution for postage, certificates, entries, care, &c., several thousand dollars. It now becomes a matter of consideration as to their disposition, and petitions have been received from the Washington Library and that of the Young Men's Christian Association, of this city, that they may be placed in their charge. Without expressing any opinion as to the propriety of complying with these requests, I beg to submit these propositions to the Board.

The operation of the former law of copyright illustrates the necessity of caution, on the part of the Institution, in receiving miscellaneous donations into a special collection without careful discrimination, particularly if the gift is coupled with the condition that the articles are to be perpetually preserved. Without care in this respect a large amount of trash must inevitably accumulate, which will interfere with the extension of the collection in the desired direction.

Among the special donations to the library are a series of expensive illustrated works from the Duke of Northumberland, privately printed by him as materials for the history of the county which bears his name. They include a survey of the Roman wall which was built across the north of England, a description of coins of the Roman families, some of which were found in that locality, and an account of some ancient castles which possess historical interest.

Another portion of the great work of Lepsius on Egypt has been presented by the King of Prussia, and valuable donations in the way of completing our transactions have been received from different societies.

The purchases for the library have been principally in the way of completing such transactions and of supplying such additional series as we have failed to obtain through our exchanges.

The library now contains a very large collection of the catalogues and reports of different public institutions in this country, which have been classified and arranged in a separate apartment so as to be readily accessible for statistical inquiries; and we trust that this collection will continually be enlarged by additions of the current reports, particularly those of all institutions which receive the Smithsonian publications.

Meteorology.—The arrangement between the Patent Office and this Institution in relation the collection of meteorological statistics still continues. The amount appropriated, however, by the former has been less than in previous years. The observers have very much increased in number, and are now divided into three classes—the first making records with a full set of instruments, the second with a thermometer and rain gage, and the third without instruments; all, however, reporting the state of the sky, the direction of the wind, beginning and ending of storms, and casual phenomena. All the observations which have been taken since 1854 have been reduced, and are now in the hands of the Commissioner of Patents to be presented to Congress as an Appendix to his Agricultural Report. It is presumed they will be ordered to be printed since they form an interesting part of the agricultural statistics called for by the Department of the Interior.

The several systems of observations made in different parts of the American continent have been continued and extended during the past year. Those under the direction of the Surgeon General of the United States have been made to include the new military posts of the army, and a series of investigations of much interest has been prosecuted in California by Lieut. R. S. Williamson, of the topographical corps, relative to the diurnal changes, the diminution of temperature depending upon elevation, and the extent of simultaneous barometrical fluctuations.

Observations have also been made by the following surveying and exploring parties, sent out under different departments of the government, viz:

Under the INTERIOR DEPARTMENT:

The Wagon Road Expedition from South Pass to California, under command of F. W. Lander.

The survey of the boundary between Texas and New Mexico, John H. Clark, commissioner.

Under the STATE DEPARTMENT:

The northwestern boundary survey, Archibald Campbell, commissioner.

Under the WAR DEPARTMENT:

Explorations in Utah, by Captain J. H. Simpson, U. S. A.

Explorations of the head waters of the Missouri and Yellowstone, by Captain W. F. Raynolds, U. S. A.

The Wagon Road Expedition from Walla-Walla to Fort Benton, Lieutenant John Mullan, U. S. A.

Exploration of San Juan and Upper Colorado, by Capt. J. N. Macomb, U. S. Top. Engs.

Observations for determining the rise and fall of the lake surface, by Capt. A. W. Whipple, U. S. Top. Engs.

Survey of the northern and northwestern lakes, by Capt. G. G. Meade, U. S. Top. Engs.

STATE OF TEXAS. Geological survey of the State, B. F. Shumard, State geologist.

In addition to the regular observations, the following were received by the Institution during the year 1859:

From Dr. S. P. Hildreth, Marietta, Ohio, observations for forty-two years, from 1818 to 1859, inclusive, on the barometer, thermometer, and amount of rain, with remarks on the weather.

From Professor A. Caswell, Brown University, Providence, Rhode Island, observations for twenty-eight years, from 1832 to 1859 inclusive, on barometer, thermometer, winds, rain, and remarks on the weather.

From Dr. Nathan D. Smith, near Washington, Arkansas, observations for twenty years, from 1840 to 1859, inclusive, on thermometer, rain, and remarks on the weather.

From Hiram A. Cutting, Lunenburg, Vermont, abstracts of thermometer observations, from 1848 to 1858, inclusive.

From Henry Connelly, Rigolet, Esquimaux bay, Labrador, observations on barometer and thermometer, from November, 1857, to May, 1859, inclusive.

From Abram Van Doren, Falmouth, Virginia, psychrometrical observations at Mount Langton, Bermuda, from November, 1847, to May, 1848, and from November, 1848, to March, 1850, inclusive.

From Dr. John F. Posey, Savannah, Georgia, hourly barometrical and thermometrical observations during half the day for about ten days in each month, from May, 1858, to September, 1859, inclusive.

From Queen's College, Kingston, Canada West, summary of observations for 1858.

The observations from Captain George G. Meade, noted on the opposite page, consist of fifty-eight sheets, made with full sets of instruments, on the northern and northwestern lakes, from June, 1858, to November, 1859, inclusive. The continuation of these will be of much value in regard to the climate of this region. Those from Captain A. W. Whipple were made at Oswego harbor, Detroit river, St. Clair flats, and Lake George, of St. Mary's river, for the purpose of determining the rise and fall of the lake surface, from the year 1854 to 1859, inclusive.

Between four and five thousand newspaper notices of the weather of 1859 have been cut out and preserved. Besides the papers received by the Institution, a large number of exchange papers are obtained from the office of the *Evening Star*. These two sources together furnish the means, either by original or copied articles, of obtaining popular notices of the principal changes of weather and meteorological phenomena in nearly all parts of the country. The method of preserving the scraps is to paste them on sheets arranged, not by the date of the paper, but by that of the occurrence noted. By this arrangement all notices of any storm, hot or cold terms, aurora, earthquake, or any other phenomenon, are brought together, no matter how widely apart may be the place of publication. It would be an acceptable donation if all readers, when they meet with any meteorological scrap worthy of preservation, would send the paper containing it to the Institution, or the article cut out, with the name, date, and place of publication of the paper plainly marked on the slip. Without these marks it would be sometimes impossible to identify either time or locality. The preferable mode is to send the entire paper.

A general distribution of blanks is made twice a year. In this distribution twelve blanks are sent to each observer, one for each month, to be retained for his own use, and one to be returned to the Institution.

As the registers come from all parts of the country, and arrive at different times, a number are received every day, sometimes as many as thirty or forty. A book is kept containing the names of all the observers, with their place of observation; and when the registers are received, an entry of each one is made in this book, so that, by inspection, it can be seen at any time for what months registers have

been received from each observer. A memorandum is also made of deficiencies, omissions, instruments used, discontinuance, change of address, or such other items as may be necessary for reference or to show the character of the observations. The date of the reception is marked in red ink on each register, and they are placed on shelves, those for each month being kept together. With the registers, letters are often received asking for information on meteorological and other subjects. These embrace a very wide range of inquiry, and, with the other letters received, make large demands on the time of the secretary. When a number of registers have accumulated, they are sent to Professor Coffin, at Easton, Pennsylvania, who, with his corps of assistants, makes the reductions and prepares them for publication.

Early in March, when a sufficient time is supposed to have elapsed to allow all, or nearly all, the registers for the preceding year to be received by due course of mail, an examination of the record is made, and a notice is sent to each observer, whose register for any month may not have reached us, informing him of the deficiency, and requesting that, if possible, it may be supplied.

It sometimes happens that through loss in the mail or other causes the blanks fail to reach some of the observers, and their stock, therefore, becomes exhausted before the time for the semi-annual distribution. In such cases, as soon as the Institution is informed of the fact, a new supply is forwarded.

Three forms of blanks are now distributed, corresponding to the three classes of observers: First, a large blank, marked No. 1, which contains columns for the records of the barometer, its observed height, attached thermometer, and height reduced to the freezing point; of the thermometer in the open air; the psychrometer, or wet and dry bulb thermometer; the depth of each rain and snow, with the time of beginning and ending; the direction and force of winds; the kind, amount, and motion of clouds; and also for the force of vapor and relative humidity of the atmosphere as deduced from the observations with the wet and dry bulb thermometers.

Second, a blank half the size of the former, marked No. 2, and containing all the columns which are on the first blank, except those for the barometer and psychrometer, and the deductions from them.

Third, a blank the same size as the second, marked No. 3, and

containing columns for such observations only as may be made without any instruments, viz: the amount, kind, and motion of clouds; the time of beginning and ending of rain and snow, the direction and force of wind by estimation, and general remarks on the weather. On the reverse of all the blanks is a place for remarks on casual phenomena, as tornadoes, auroras, meteors, &c.

The importance of such meteorological records as may be kept without instruments seems to be much underrated, and many persons have declined to make or continue observations unless supplied with full sets of instruments, assigning as a reason that such records can be of no value. But a little reflection will show that these observations may furnish interesting and important information. If kept daily and in all parts of the country and sent to the Institution, they would, without the aid of any other record, enable an investigator to determine the direction and rate of motion of every storm, hurricane, tornado, and thunder shower in the United States, and also the time and place of their origin and termination when these occurred within the limits of the observers.

They would enable him to mark out with accurate lines the districts most subject to these atmospheric disturbances, and those free from them or only partially visited, and also the portions of the year at which they are most or least frequent. They would enable him to ascertain the number, extent, and principal phenomena of all the auroras, earthquakes, and meteors; the occurrence of the first, last, and severe frosts, the comparative duration of clear and cloudy weather, and the prevailing winds, both surface and upper current. If to ascertain these points in the meteorology of every part of North America is important, then the keeping such records as may be made without instruments ought not to be omitted. While there are persons in every neighborhood in this country who could faithfully and accurately fill these blanks, the number who can obtain instruments and properly observe them must be comparatively limited.

Frequent applications are made to the Institution offering to record meteorological observations for a reasonable pecuniary compensation. But the policy originally adopted, on account of the want of means, of declining to pay observers, has been uniformly adhered to. To depart from this policy, while it might here and there secure an additional observer, would involve difficulties of discrimination altogether disproportionate to the advantage gained. While, however, no

pecuniary compensation is made to observers, the facilities exten ed
to them, besides the reports and other documents occasionally dis-
tributed, though not expected to be a remuneration for the time and
labor devoted to the subject, are still of some value.   An inducement
also exists in the pleasure to be derived from the study of the changes
of the phenomena of nature, besides the consciousness of co-operating
with many others in a system intended to advance an important branch
of science.

Although the information derived from the system of observations,
under the immediate direction of the Institution, is exceedingly
valuable in many points, it is to be regretted that changes in the
observers are so frequent.   The value in determining the great ques-
tion·as to the periodicity of the weather and its average conditions
as to heat, moisture, cloudiness, occurrence of frosts, &c., other things
being the same, depend upon the length of time the series of observa-
tions have been continued, and results which may be relied upon
as the element of insurance against failure of crops and disasters by
storms are valuable strictly in proportion to the number of years
embraced in the observations.

The reason why meteorology is not further advanced is not on
account of the want of observations, but of extended series at a
number of properly chosen places.   From observations of this kind
it is found that much of the apparent irregularity and caprice of the
weather is due to our limited vision, and that by extending the records
and properly studying them, many phenomena which are apparently
fitful and exempt from all law will be reduced to order and periodicity.

According to Mr. Glaisher's investigations of the records made at
the Royal Observatory for a long series of years, there is a rotation
in the character of the weather at London about every fifteen years,
the seasons growing warmer and warmer until they reach a maximum
of temperature, and then gradually colder until they come to the
lowest point, when they begin a new cycle of temperature.   A series
of meteorological tables may appear to the casual observer a mere
mass of dull, uninteresting figures, yet a little study will enable us,
says a popular writer of the day, to read in them the past history of
"rich harvests, prosperous commerce, good health, plenty, and con-
tentment; or, perhaps, the gloomier side of the picture, scanty crops
and high prices, stagnant trade and social irritation, prevalent diseases
and busy death;" and, it might be added, a still greater interest is

inspired by the prospect they afford, that in the course of time they will not only reflect the past but foreshow the future, and enable us by their timely monitions to avail ourselves of the benignity, or guard against the ravage of the coming day.

During the year 1859 a remarkable exhibition of the Aurora Borealis occurred, and special efforts have been made to collect all the reliable observations which could be obtained in regard to this phenomenon. The materials thus accumulated have been arranged and will be published in order to render them generally accessible. The occurrence of the Aurora, as is well known from the observations of Arago and others, is attended with a remarkable disturbance of the magnetic needle; and since the latter, as has been shown in the account of the paper of Dr. Bache, is connected with the spots on the sun, it would follow that the Aurora, although apparently of electrical origin, is connected with influences entirely exterior to our planet, and hence precise information as to its appearance over so wide an area as that in which it was exhibited about the beginning of September last, must be specially acceptable to the student of terrestrial physics.

Two remarkable meteors have appeared during the last year, one of which exploded near the boundary between New York and Massachusetts, and the other apparently descended to the earth either in Delaware bay or in the ocean in its vicinity. All the facts collected in regard to these mysterious visitants of our atmosphere have been referred to scientific gentlemen for critical examination, the results of which will be published either by the Institution or in the American Journal of Science.

The reduction of the current observations is continued by Professor Coffin, who is also engaged, with the assistance of the Institution, in the investigation of the winds of the southern hemisphere and in the extension of his previous researches in regard to those in the northern hemisphere.

The office duties of distributing the blanks; arranging the meteorological material and returns received from observers; of superintending the observations made at the Institution, and in assisting in the Smithsonian publications relative to meteorology, are assigned to Mr. William Q. Force, of Washington, and to his industry, ingenuity, and efficiency the system owes many improvements.

4

Museum.—During the year 1859 the labelling and repairing the specimens received from the Patent Office, and setting up and classifying new specimens collected by the Institution, or deposited by different government explorations, have been uninterruptedly continued under the immediate direction of Professor Baird.

The extensive series of corals collected during the United' States exploring expedition have been arranged and labelled by Professor Dana, of New Haven, by whom they were originally described, and now constitute an interesting and attractive part of the general museum. We have also employed a distinguished conchologist, Mr. P. P. Carpenter, of Warrington, England, to classify and label the extensive collection of shells, and have been favored in this work with the co-operation of the principal gentlemen most distinguished in this country for their original investigations in this branch of natural history. Mr. Isaac Lea, of Philadelphia, assisted by Dr. Foreman, has named the Unionidæ; Mr. Binney, of New Jersey, the Helicidæ; Mr. Stimpson, the shells of the eastern coast of the United States; and Dr. Gould has identified the new species of the exploring expedition, and rendered aid in the classification of the collection generally. Assistance has also been rendered by Mr. J. G. Anthony, Mr. James Lewis, Dr. Newcomb, and Mr. Lapham.

A taxidermist has been constantly engaged in going over the collection. He has mounted several hundred new specimens of birds, and set up a considerable number of large quadrupeds.

According to the statement of Professor Baird, the number of entries in the record books, of additions to the museum, in the line of zoology, during the year 1859, amounts to 11,691, and the whole number of records in all the books is 37,197; but, as explained in previous reports, this number is far from exhibiting the aggregate of specimens catalogued; each entry frequently includes all the specimens of any one species received at one time from one locality, and, in some cases, embraces several hundred individual objects. It will not be too high if we estimate five specimens on the average to each entry, and we shall then have 185,985 as the aggregate of these objects now in the museum. In addition to the foregoing, there is a large collection of specimens in ethnology, botany, mineralogy, and geology.

The object of the Institution in obtaining so large a number of duplicates is, that they may be distributed for the advancement of

knowledge to persons who may be engaged in original investigations in natural history, to museums for the completion of their lists, and also to colleges for the purposes of education.

As it is not a part of the policy of the Institution to form a general collection like that of the British museum, which can only be the work of the general government, but to assist in advancing and diffusing a knowledge of the natural history of North America, we have been anxious to distribute the duplicates in such a manner as to render them subservient to the objects in view, and it is hoped that we shall begin the work of distribution within the present year. As usual, a number of young gentlemen have availed themselves of the facilities offered by the library and collections of the Institution to prosecute their researches in different branches of natural history; and in some cases, in which such confidence seemed fully justified, series of specimens have been intrusted for examination to individuals at a distance. Although the primary object of the Institution is not educational, yet the museum is arranged with especial reference to the study of the elements of different branches of science; and the distribution of the extra specimens will furnish the means of diffusing a knowledge of natural history more generally throughout the country.

Explorations.—A number of expeditions sent out by the general government have been furnished with instructions, and in some cases with apparatus for the collection of objects of natural history, and several explorations have been undertaken by individuals under the immediate auspices of the Institution. I need only mention in this place that conducted by Mr. Robert Kennicott in the regions of the Hudson's Bay territory and in Russian America. This young gentleman is assisted by the Smithsonian Institution, the University of Michigan, the Audubon Club and Academy of Sciences of Chicago, and a number of liberal-minded persons interested in natural history. His labors have been greatly facilitated by the cordial co-operation of Sir George Simpson, governor, and the other officers of the Hudson's Bay Company, to whom we are also indebted for valuable donations of specimens and records of meteorological observations.

For a detailed account of the explorations and the museum, I refer to the report of Professor Baird hereto appended.

Lectures.—The following lectures were delivered during the winter of 1859–'60:

Six lectures by Professor SAMUEL W. JOHNSON, of Yale College, on Agricultural Chemistry:

1. The Plant, its Structure and Composition.

2. The Atmosphere and Water in their relation to Vegetable Growth.

3. The Soil, as related to Agricultural Productions.

4. The Improvement of the Soil by Tillage, Drainage, Amendments, and Fertilizers.

5. The Conversion of Vegetable into Animal Produce.

6. Systems of Farm Practice Viewed in the Light of Agricultural Science; Rotation of Crops; Exhaustion and Maintenance of Agricultural Resources.

Three lectures by PHILIP P. CARPENTER, esq., of England:

1. Shells of the Gulf of California.

2. The Cuttle Fish Tribe, their Forms and Habits in the Ancient and Existing Seas; including the Paper and Pearly Nautilus, &c.

3. Crawling Shells.

One lecture by Professor HENRY COPPÉE, of the University of Pennsylvania, on Coincidences in the Conquests of Mexico.

Four lectures by Professor BENJAMIN PEIRCE, of Harvard College, Cambridge, Massachusetts:

Two on The Diversities in Mathematical Powers of Different Races and Nationalities;

Two on Comets.

Three lectures by Dr. BENJAMIN A. GOULD, of Cambridge, Massachusetts, on Chance, Probability, Accident.

Three lectures by Professor A. T. BLEDSOE, of the University of Virginia, on The Social Destiny of Man.

Three lectures by the Rt. Rev. M. J. SPAULDING, Bishop of Louisville, Kentucky, on The Elements and History of Modern Civilization.

One lecture by WILLIAM GILPIN, esq., of Missouri, on The Characteristics and Physical Geography of the Western Portion of North America.

Five lectures by T. STERRY HUNT, F. R. S., of the Geological Survey of Canada:

1. On Chemical and Physical Geology; Introduction of Geological Agencies.

2. Chemistry of the Earth's Crust.
3. Life in its Geological Relations.
4. Geology of the Metals, Mineral Springs, Metamorphism.
5. Igneous Rocks, Volcanoes, Mountain Chains.

The interest is still kept up in the lectures, although they occasionally called forth criticisms on account of the character of particular courses, which, while they are received with much interest by one class of hearers, are not in accordance with the taste of those whose pursuits or reading lie in an opposite direction.

While a large number of persons regularly attend the lectures for the sake of the advantage to be derived from them, others, and particularly young persons, attend as a mere pastime, or assemble in the lecture room as a convenient place of resort, and by their whispering annoy those who sit near them. Frequent complaints have been made on this account, and it has been suggested that it might be well to try, as an experiment, a plan similar to that adopted at the Lowell Institute, in which, as in the case of the Smithsonian Institution, free lectures are given to a promiscuous audience. To secure proper order, and to prevent the interruption of the speaker by persons arriving after the lecturer has commenced, a series of numbered tickets may be distributed at the commencement of the season, and the names of the persons who receive them entered in a book, opposite the number of the ticket; and in addition to this, the doors may be closed a few minutes after the lecture commences.

Respectfully submitted.

JOSEPH HENRY,
Secretary.

JANUARY, 1860.

APPENDIX TO THE REPORT OF THE SECRETARY.

SMITHSONIAN INSTITUTION,
Washington, December 31, 1859.

SIR: I have the honor herewith to present a report, for 1859, of the operations you have intrusted to my charge, namely, those which relate to the printing, the exchanges, and to the collections of natural history.

Very respectfully, your obedient servant,

SPENCER F. BAIRD,
Assistant Secretary Smithsonian Institution.

Prof. JOSEPH HENRY, LL.D.
Secretary Smithsonian Institution.

PUBLICATIONS.

The publications of the Institution during the past year have been as follows:

Annual Report of the Board of Regents of the Smithsonian Institution, showing the operations, expenditures, and condition of the Institution for the year 1858. One volume, 8vo., pp. 448.

An account of the Total Eclipse of the Sun on September 7, 1858, as observed near Olmos, Peru. By Lieutenant J. M. Gilliss, United States navy. 4to. pp. 24, and one plate.

Catalogue of Publications of Societies and of other Periodical Works in the Library of the Smithsonian Institution. July 1, 1858. Part 1. Foreign works. 8vo. pp. 260.

Appendix to the Catalogue of Described Diptera of North America. By Baron R. Osten Sacken. 8vo. pp. 4.

Directions for Collecting, Preserving, and Transporting Specimens of Natural History, prepared for the use of the Smithsonian Institution. Third edition. 8vo. pp. 40.

Discussion of the Magnetic and Meteorological Observations made at the Girard College Observatory, Philadelphia, in 1840, 1841,1842, 1843, 1844, and 1845. Part 1. Investigations of the Eleven-Year Period in the Amplitude of the Solar-diurnal Variation, and of the Disturbances of the Magnetic Declination. By A. D. Bache, LL.D. Pages 22. Quarto.

Meteorological Observations in the Arctic Seas. By Elisha Kent Kane, M. D., United States navy. Made during the second Grinnell expedition in search of Sir John Franklin, in 1853, 1854, and 1855, at Van Rensselaer harbor, and other points on the west coast of Greenland. Reduced and discussed by Charles A. Schott, assistant United States Coast Survey. Pages 120. Quarto.

The Coleoptera of Kansas and Eastern New Mexico.   By John L. Leconte, M. D.   Pages 66, and three plates.   Quarto.

Observations on Terrestrial Magnetism in Mexico, conducted under the direction of Baron Von Müller, with notes and illustrations of the volcano Popocatapetl and its vicinity.   By August Sonntag.   Pages 92, and one plate.   Quarto.

Observations on Certain Storms in Europe and America, in December, 1836.   By Elias Loomis, LL.D., professor of natural philosophy in the University of the city of New York.   Pages 28, and thirteen plates.   Quarto.

In addition to the preceding publications, the following are in an advanced stage of printing, and will be ready for distribution in a short time:

Catalogue of the Described Lepidoptera of North America.   By Rev. Dr. John G. Morris.   Octavo.

Check Lists of the Shells—land, fresh water, and marine—of North America.   Octavo.

Meteorological Observations made at Providence, Rhode Island. By Prof. A. Caswell.   Quarto.

Meteorological Observations made near Washington, Arkansas.   By Dr. Nathan D. Smith.   Quarto.

New Edition of the List of Foreign Institutions in Correspondence with the Smithsonian Institution.   Octavo.

Circular in reference to the collection of facts relating to the grasshopper tribes of North America.   Octavo.

Circular in reference to the collecting of nests and eggs of North American birds.   Octavo.

Circular in reference to the collecting of North American shells. Octavo.

EXCHANGES.

As in previous years, a continuous increase and expansion of operations has taken place in this department.   The annexed tables will give a better account of their magnitude than any general remarks on the subject.   It will be sufficient to say that the number of institutions and individuals in the United States availing themselves of the Smithsonian facilities now embraces nearly all those publishing works of a scientific and literary character.

The expenses of the system of exchanges conducted by the Institution have, of course, increased with the expansion of operations, and but for the free facilities so generously accorded by various parties would have arisen to such an amount as to render it necessary to call on each of the institutions benefited for a share of the cost. This has not yet been done, except in the case of the United States Coast Survey, which has greatly exceeded all the rest in the bulk of its transmissions; but it may soon be required from other parties.

The thanks of the Smithsonian Institution for free freights of packages containing exchanges and specimens of natural history are especially due to the following companies:

The North German Lloyd steamers, running between Bremen and New York, of which Messrs. Gelpcke, Keutgen, and Reichelt, 84 Broadway, are agents. This line has carried 250 cubic feet for the Institution at a single trip, without charge.

The California steamer line, consisting of the Pacific Mail Steamship Company, between San Francisco and Panama, (as well as between San Francisco and the ports of Oregon and Washington Territory,) of which Mr. W. A. Davidge is president; the Panama Railroad Company, David Hoadley, president; from Aspinwall to New York the line at first consisted of the United States Mail Steamship Company, M. O. Roberts, president, up to the 5th of October last, when it was replaced in the route by the North Atlantic Steamship Company, I. W. Raymond, president. The agents of this line, Mr. W. H. Wickham, at New York, and Mr. A. B. Forbes, assisted by Mr. Samuel Hubbard, at San Francisco, have also been very zealous in their attentions to the interests of the Institution.

Messrs. Adams & Co., through the superintendent of the southern division, Mr. S. M. Shoemaker, of Baltimore, and the Washington agent, Mr. A. J. Falls, have very materially aided the Institution by a reduction of freights on heavy goods, and their remission entirely on small packages. This has also been done by Messrs. Wells, Fargo & Co. in California.

The line of sailing vessels between New York and the west coast of South America, belonging to Mr. Bartlett, 110 Wall street, has continued to carry our Chilean exchanges free of charge.

To Mr. Edward Cunard, of New York, the Institution is indebted for the offer to carry a specified amount of freight from New York to Liverpool by his steamers, free of charge—a privilege which has already, in part, been made use of.

The Isabel steamer, running between Charleston, Key West, and Havana, has also continued to carry packages free of charge, through the liberality of the agents, Messrs. Mordecai & Co., of Charleston.

Messrs. Russell & Jones, contractors of army transportation to Utah, and through Kansas and Nebraska, have also kindly extended the facilities of their line to the Institution.

Considering the large amount of freight which some of the above-mentioned lines transport annually for the Smithsonian Institution, it will readily be understood how much they have assisted in carrying out its objects, as, with a fixed income, the expenses of the exchanges and natural history operations conducted by the Institution would be entirely beyond its present means.

A.

Receipt of books, &c., by exchange, in 1859.

Volumes—Octavo	659
Quarto	303
Folio	60
	1,022
Parts of volumes and pamphlets:	
Octavo	1,696

Quarto	756	
Folio	88	
		2,540
Maps and charts		40
Total		3,602

Being an increase over the year 1858 of 1,062 volumes and parts of volumes. The number of separate donations amounted to 1,252. It will be remembered that this does not include the volumes purchased for the current researches and operations of the Institution, or for the completion of such series of transactions and periodicals as could not otherwise be obtained.

B.

Showing the statistics of foreign exchanges of the Smithsonian Institution in 1859.

Agent and country.	Nos. of addresses.	Nos. of packages.	Nos. of boxes.	Bulk of boxes in cubic feet.	Weight of boxes in pounds.
1. DR. FELIX FLÜGEL, *Leipsic*—					
Sweden	12	43			
Norway	5	22			
Denmark	12	36			
Russia	38	112			
Holland	33	90			
Germany	31	87			
Switzerland	294	878			
Belgium	12	43			
Total	437	1,311	32	440	13,327
2. H. BOSSANGE, *Paris*—					
France	105	333			
Italy	55	174			
Portugal	3	8			
Spain	6	19			
Total	169	534	15	228	5,753
3. HENRY STEVENS, *London*—					
Great Britain and Ireland	151	670			
Total	151	670	25	346	9,200
4. REST OF THE WORLD	68	220	10	40	1,200
Grand total	825	2,735	82	1,054	29,480

The great increase in the number and bulk of packages of books sent abroad during the period reported on is owing to several causes. Among these may be mentioned the fact that the table embraces the statistics of three different transmissions, rendered necessary by the accumulation of materials in the Smithsonian building.   A large proportion of the works sent consisted of such publications of the United States government as the Pacific Railroad Report, the Mexican Boundary Report, the Report on Commercial Relations, the Coast Survey Report, &c.

Of the Pacific Railroad Report alone, over one hundred sets were sent, in behalf of the War Department, to the principal public libraries and societies in Europe, and more than three hundred copies of the Coast Survey Report to similar institutions.   The Patent Office exchanges with parties in Europe also occupied a very large bulk.

C.

Packages received by the Smithsonian Institution from parties in America for foreign distribution in 1859.

	Number of packages.
Albany, N. Y.—	
Professor J. Hall	98
Boston—	
American Academy of Arts and Sciences	77
Society of Natural History	40
Cambridge, Mass.—	
American Association for the Advancement of Science	64
Professor Gray	15
Charleston, S. C.—	
Elliott Society of Natural History	27
Columbia, Mo.—	
Professor Swallow	17
Columbus, Ohio—	
State Agricultural Society	100
Des Moines, Iowa—	
State of Iowa	193
Georgetown, D. C.—	
College	6
New Haven, Conn.—	
American Journal of Science	12
American Oriental Society	19
Professor J. D. Dana	20
Yale College	2
Little Rock, Ark.—	
State of Arkansas	800
New York—	
New York Lyceum of Natural History	74
Philadelphia—	
Academy of Natural Sciences	325
American Philosophical Society	132
Isaac Lea	134
Dr. Joseph Leidy	30
W. Sharswood	30
Providence, R. I.—	
State of Rhode Island	6

C—Continued.

	Number of packages.
St. Louis, Mo.—	
Academy of Sciences	148
Dr. B. F. Shumard	15
Dr. George Engelmann	90
Dr. Wizlizenus	3
Washington, D. C.—	
United States Patent Office	1,344
United States Coast Survey	598
Secretary of War	245
Lieutenant G. K. Warren	40
Miscellaneous	633
Total	5,337

D.

Addressed packages received by the Smithsonian Institution from Europe for distribution in America in 1859.

	Number of packages.
Albany, N. Y.—	
Albany Institute	2
Dudley Observatory	44
New York State Library	18
New York State Agricultural Society	8
New York State Medical Society	2
Professor James Hall	7
Amherst, Mass.—	
Amherst College	15
Ann Arbor, Mich.—	
Observatory	9
Annapolis, Md.—	
United States Naval School	2
Atlanta, Ga.—	
Medical College	1
University of Georgia	1
Baltimore, Md.—	
Maryland Historical Society	7
Bloomington, Ind.—	
Indiana University	1
Boston, Mass.—	
American Academy of Arts and Sciences	64
American Statistical Association	10
Boston Society of Natural History	43
Bowditch Library	4
Historical Society	2
Prison Dicipline Society	2
Public Library	21
State Library of Massachusetts	4
Brunswick, Me.—	
Bowdoin College	9

D—Continued.

	Number of packages.
Burlington, Vt.—	
University of Vermont	7
Cambridge, Mass.—	
American Association for the Advancement of Science	15
Cambridge Astronomical Journal	19
Cambridge Observatory	42
Library of Harvard College	22
Professor L. Agassiz	32
Professor Asa Gray	19
Professor B. Peirce	8
Charlottesville, Va.—	
University of Virginia	9
Charleston, S. C.—	
Eliott Society of Natural History	10
Public Library	2
Society Library	2
Cincinnati, Ohio—	
Mercantile Library	7
Observatory	7
Public Library	1
Columbia, Mo.—	
Professor G. C. Swallow	9
Columbia, S. C.—	
South Carolina College	6
Columbus, Ohio—	
Ohio State Board of Agriculture	48
Chapel Hill, N. C.—	
University of North Carolina	1
Chicago, Ill.—	
Mechanics' Institute	7
Detroit, Mich.—	
Michigan State Agricultural Society	26
Easton, Pa.—	
Lafayette College	1
Frankfort, Ky.—	
Geological Survey of Kentucky	8
Gambier, Ohio—	
Kenyon College	9
Georgetown, D. C.—	
Georgetown College	23
Hanover, N. H.—	
Dartmouth College	10
Harrisburg, Pa.—	
State Library	1
Hartford, Ct.—	
Young Men's Institute	6
Jacksonville, Ill.—	
Illinois College	1
Madison, Wis.—	
Wisconsin State Agricultural Society	28
Historical Society of Wisconsin	8
Milledgeville, Ga.—	
Oglethorpe University	5
Montreal, Canada—	
Natural History Society	1
Natchez, Miss.—	
Public Library	1

D—Continued.

	Number of packages.
New Brunswick, N. J.—	
Professor George H. Cook	8
New Harmony, Ind.—	
Dr. D. D. Owen	5
New Haven, Ct.—	
American Journal of Science	10
American Oriental Society	8
Professor J. D. Dana	41
Professor Silliman	25
Yale College	17
Newark, Del.—	
Delaware College	1
New Orleans, La.—	
New Orleans Academy of Natural Science	31
University of Louisiana	2
Newport, R. I.—	
Redwood Library	1
New York—	
American Ethnological Society	7
American Geographical and Statistical Society	37
American Institute	8
Astor Library	3
Mercantile Library Association	7
New York Lyceum of Natural History	29
Oregon city, O.—	
University of Oregon	1
Philadalphia, Pa.—	
Academy of Natural Sciences	105
American Philosophical Society	67
Central High School of Philadelphia	6
Franklin Institute	17
Historical Society of Pennsylvania	1
Pennsylvania Institute for the Blind	-
Philadelphia College of Pharmacy	-
Philadelphia Library Company	11
University of Pennsylvania	1
Wagner Free Institute	10
Isaac Lea	16
Dr. John Leconte	5
Dr. Joseph Leidy	19
William Sharwood	13
Princeton, N. J.—	
College of New Jersey	2
Providence, R. I —	
Brown University	8
Richmond, Va.--	
State Library	1
Rochester, N. Y.—	
University	1
San Francisco, Cal.—	
California Academy of Natural Sciences	23
Geological Survey of California	1
Savannah, Ga.—	
Georgia Historical Society	1
St. Louis, Mo.—	
St. Louis Academy of Science	70
University	11
Dr. B. F. Shumard	5
Dr. George Engelmann	7

D—Continued.

	Number of packages.
Santiago, Chile—	
University of Chile	6
Observatory	1
Springfield, Ill.—	
Illinois State Agricultural Society	1
Tallahassee, Fla.—	
Public Library	1
Toronto, Canada—	
Canadian Institute	4
Tuscaloosa, Ala.—	
University of Alabama	1
Washington, D. C.—	
President of the United States	1
United States Patent Office	72
Ordnance Bureau	3
Light-house Board	11
United States Coast Survey	3
Professor Bache	44
National Observatory	27
Lieutenant Maury	54
Secretary of War	3
Commissioner of Indian Affairs	1
Surgeon General	3
Congress Library	15
Lieutenant J. M. Gilliss	31
National Institute	3
Waterville, Me.—	
Waterville College	1
Worcester, Mass.—	
American Antiquarian Society	6
Individuals not mentioned above	246
Miscellaneous societies, &c., not mentioned	76
Total	1,933

MUSEUM.

ADDITIONS TO THE MUSEUM.

Extensive as were the additions to the museum in 1858, (exclusive of those transferred from the Patent Office,) those of 1859 have exceeded them in magnitude and extent. The number of separate donations amounted to 301, (127 in 1858,) embracing nearly 500 different packages. A detailed list of these donations will be found at the end of this report, to which I would refer, and I will proceed here to mention more in detail such of the collections received as from their importance appear to require it.

As heretofore, most of the large collections were received from officers in charge of parties under different departments of the government. Although the number of these parties has not been as great as in some other years, the results attained by them are in no way inferior in importance.

EXPLORATIONS UNDER THE WAR DEPARTMENT.

1. *Construction of wagon road from Walla-Walla, Oregon, to Fort Benton, under Lieutenant John Mullan, U. S. A.*—This expedition, with Dr. Mullan as medical officer, went to California in the spring, by steamer, and thence to Walla-Walla, the starting point. Supplies were sent up the Missouri by the steamers of the American Fur Company of St. Louis, one of which made the first trip through to Fort Benton. The supplies were accompanied by Mr. John Pearsall, an assistant of Lieutenant Mullan, who made large collections of nests, eggs, birds, insects, and fossils. Collections were also made by Lieutenant Mullan, between Walla-Walla and his winter camp, near Fort Owen, not far from the Bitter Root valley, where he was established at the last advices.

2. *Exploration of the Upper Missouri and Yellowstone. under Captain J. W. Raynolds, U. S. A.*—This expedition, accompanied by Dr. Hayden as geologist and naturalist, went up the Missouri in the steamboat of the Fur Company, in June last, and disembarked at Fort Pierre. From this starting point they proceeded across to the Yellowstone, at Fort Sarpy, and thence, after various lateral explorations, to their winter quarters on Deer creek, at the crossing of the Platte, about 100 miles west of Fort Laramie, and in the immediate vicinity of the Upper Platte Indian agency, in charge of Major Twiss. During the winter, excursions have been made in different directions for the purpose of geological explorations, and large collections secured in all departments of natural history. The collections received in Washington from Captain Raynolds consist mainly of the zoological series gathered up to the arrival of his party at Fort Sarpy, and embrace specimens of birds, eggs, insects, &c.

3. *Exploration of the San Juan river and Upper Colorado, under Captain J. N. Macomb, U. S. A.*—This party, accompanied by Dr. Newberry as geologist and naturalist, did not get into the field until a late period in the summer, and returned before winter to the east. The principal results of the expedition, independently of interesting geographical discoveries, are to be found in the department of geology, in which Dr. Newberry was enabled to extend his observations made while on the Colorado expedition under Lieut. Ives. Among other fossils, Dr. Newberry obtained remains of a new genus and species of extinct lizard, larger than any formerly found in North America.

4. *Explorations with the army in Utah, under Captain Simpson, U. S. A.*—This party, under Captain Simpson, accompanied by Mr. Charles S. McCarthy as collector, left Fort Leavenworth for Camp Floyd, in May, 1858, reaching its destination in September. From this time until March, 1859, the party was occupied in making roads between Camp Floyd and Fort Bridger, and in marking reservations. From May to August, 1859, Captain Simpson explored two wagon road routes between Camp Floyd and Genoa, in Carson valley, which shorten very much the distance from Salt Lake City to California.

As much of this region had never been before traversed by scientific men, the results were very interesting, consisting of several new and peculiar forms of fishes, as also of reptiles, insects, &c. Many rare birds, with their eggs, were also procured.

The party left Camp Floyd for Leavenworth in August, by way of the Timpanogos, the Uintah mountains and the valley of the Green river.

In addition to large zoological collections made by Mr. McCarthy, Mr. Engelmann, the geologist of the expedition, made some important geological discoveries, and brought in many fossils and plants.

UNDER THE STATE DEPARTMENT.

5. *Survey of the northwest boundary: Archibald Campbell, esq., commissioner.*—This expedition, with Dr. Kennerly as the surgeon and naturalist, and Mr. George Gibbs as geologist, has already been referred to in previous reports. These gentlemen have continued to make large collections during the year, those received filling twelve boxes. The specimens gathered about the Chiloweyuck depot proved particularly interesting, from including a new salmon, *Anodonta*, and other species of animals. The collections also embrace skins and skulls of the *Aploceras montanus*, or Rocky mountain goat, the only ones known in American collections; also of *Lagomys princeps*, or little chief hare, and skins of *Lagopus leucurus*, the white-tailed ptarmigan, and of the black-throated diver, *Colymbus arcticus*, in adult summer dress.

UNDER THE NAVY DEPARTMENT.

6. *Exploration of the Parana and its tributaries, under Captain T. J. Page, U. S. N.*—This survey, in continuation of that prosecuted

by Captain Page several years ago, is accompanied by Mr. Christopher Wood as taxidermist, and has already sent home quite a large collection of birds, embracing several rare and perhaps new species.

UNDER THE INTERIOR DEPARTMENT.

7. *Wagon road from El Paso to Fort Yuma, under Colonel Leech.*— This expedition returned to Washington in 1858, but the collections did not arrive until early in 1859. These were made by Dr. S. Hayes, and consisted chiefly of a large and valuable herbarium, embracing several new species of plants.

8. *Wagon road from the South Pass to California, under Colonel F. W. Lander.*—This party passed so rapidly over the country as to be unable to do much in the collecting of specimens of natural history. Mr. James A. Snyder, who had charge of this department, among other duties, succeeded in obtaining specimens, both in skin and in alcohol, of the very rare *Lagomys princeps*, or "coney" of the Mormons. These were captured in the Wahsatch mountains, to which, according to Colonel Lander, they appear to be chiefly confined.

IN CONNEXION WITH THE SMITHSONIAN INSTITUTION.

9. *Exploration of the vicinity of Fort Tejon and of Cape St. Lucas, by Mr. John Xantus.*—Among the very important researches in the natural history of America, the explorations of Mr. John Xantus deserve particular mention. In previous reports, the collections made by Mr. Xantus at Fort Tejon have been referred to. During a residence there of about sixteen months, from the summer of 1857 to the autumn of 1858, although constantly occupied with official duties, he has exhausted the natural history of the vicinity of the fort in the most thorough manner. All departments are fully represented in his collections, which filled thirty-five boxes ; the birds alone embracing nearly 2,000 specimens and 144 species.

Professor Bache, the Superintendent of the United States Coast Survey, having determined to establish a tidal station at Cape St. Lucas, Lower California, Mr. Xantus was placed in charge, and reached the Cape in April last. He has since that time made, in the intervals of his official duties, and forwarded to Washington, collections which vie in thoroughness with those of Fort Tejon, and exceed them in number of species, embracing as they do the marine as well as the fresh water and land forms. Of 42 species of birds first received from him, 8 are new; of crustaceans, there are over 100 species, many of them new; while in all other departments the collections have been proportionately great.

The results obtained at Cape St. Lucas by Mr. Xantus add another to the many benefits to natural history as well as to physical science rendered incidentally by the operations of the United States Coast Survey, as shown previously in the important collections of Lieutenant Trowbridge, Mr. Würdemann, Mr. Cassidy, Mr. Wayne and others.

10. *Explorations of the Hudson's Bay Territory, by Mr. Robert Ken-nicott.*—Mr. Kennicott, under the patronage of the Smithsonian Insti-tution, and by the assistance of the University of Michigan at Ann Arbor, the Audubon Club of Chicago, the Chicago Academy of Sci-ences, and a number of gentlemen interested in the natural history of the Arctic regions, has been during the past year engaged in an ex-ploration of the north, which promises results of no ordinary import-ance.   His labors have been greatly facilitated by the cordial co-operation of Sir George Simpson, governor of the territory, and the officers of the service, especially of Mr. Barnston, of Michipicoten, and Mr. B. R. Ross, of Fort Simpson.   Mr. Kennicott left in May for Lake Superior, *via* Toronto and Collingwood.   From Fort William, on Lake Superior, he was conveyed to Norway House in the Company's boats, and thence towards Fort Simpson, on the Mackenzie.   At the latest advices, of July 29, he had reached Methy or La Loche Portage, and, in company with Mr. Ross, it was his expectation to proceed in a few days to Fort Simpson, there to winter.   He intends in the spring to go to Great Slave or Bear Lake to collect eggs, and hopes to re-main long enough in the north to spend another spring and summer on the Youkon of Russian America, and another on the shores of the Arctic ocean, north of Great Bear Lake.

A small portion only of the collections made by Mr. Kennicott have yet reached Washington; those gathered between Norway House and Portage La Loche having arrived at Pembina too late to come down this year.   Among the specimens received, however, is a fine skin of the rare *Larus Sabini*, or the fork-tailed gull, shot on Lake Winnipeg.

Mr. Kennicott was accompanied to Lake Winnipeg by Mr. Charles A. Hubbard, of Milwaukie, who returned home in the fall, by way of Fort Garry and Pembina, from whom the Institution received a val-uable collection of eggs, and through him, from Mr. Donald Gunn, a number of birds and of specimens in alcohol.

11. *Explorations on the Saskatchewan, by Captain Blakiston, R. A.—* The Institution has also received some interesting collections made on the Saskatchewan, by Captain Thomas Blakiston, R. A., through the Royal Artillery Institution at Woolwich, London.   These consist of eggs and skins of birds, several of the former of which are rare and new to our museum.

Dr. Rae, the celebrated Arctic traveller, presented to the Institution specimens of *Spermophilus parryi* from Repulse bay, latitude 56° 30′ north, thus supplying an important addition to the collection of North American mammals.

12. *Various other points on the west coast.*—Other collections of in-terest consist of nests, eggs, and skins of birds, and of other animals from Fort Umpqua, sent by Dr. Vollum; of skeletons of the sea otter, by Mr. R. W. Dunbar; of specimens in alcohol, by Mr. Alexander Tay-lor; and of eggs, by Dr. C. A. Canfield.

13. *Explorations of Fort Crook, by John Feilner, esq.*—Mr. Feilner during the past year has made large collections of birds and mam-mals at Fort Crook, rivalling those of Mr. Xantus at Fort Tejon.

Among them are good specimens of such rare birds as *Picus william-soni, albolarvatus, thyroideus*, &c.

14. *Exploration in the Rocky mountain region.*—In addition to the government collections of Captain Simpson and Captain Macomb, Dr. Anderson, U. S. A., has continued to send a number of rare birds from Cantonment Burgwin. Dr. Irwin, U. S. A., at Fort Buchanan, has also made large collections for the Institution, none of which, however, have yet been received except one box of birds. Dr. George Suckley, U. S. A., while accompanying a detachment of troops from Leaven-worth to Camp Floyd, made some interesting collections, including the eggs of the Rocky mountain plover. Several rare animals have also been received from Colonel Vaughan, Indian agent for the Blackfeet, near Fort Benton.

Dr. Brewer, U. S. A., presented to the collection, through Captain J. H. Simpson, some rare reptiles and shells from southern Utah.

15. *Explorations in and near Florida.*—Dr. H. Bryant and Mr. William Cooper spent the winter in the Bahamas chiefly in and about Nassau, and made rich collections of animals, series of which were presented by them to the Institution.

Dr. J. G. Cooper visited Key West in March, and thence proceeded to Cape Florida, where he spent two months, and another month on Indian River and the St. John's. In addition to extensive zoological collections, Dr. Cooper paid particular attention to the trees of Florida,, and succeeded in obtaining several West Indian and tropical species not previously known to occur in the United States.

A large number of marine animals in alcohol, and of birds and their eggs, were collected at the Tortugas for the Institution by Captain, Woodbury, U. S. A., and by Dr. Whitehurst.

Dr. Bean, of Micanopy, Florida, has also furnished a collection of the eggs, reptiles, and fishes of the State. Among the first are the only eggs of the glossy ibis, *Ibis ordii*, yet detected within our limits.

In connexion with Florida explorations, with which he was so closely associated, it is my painful duty to mention the death, during the past year, of Mr. Gustavus Würdemann, a tidal observer of the United States Coast Survey, and for many years an active correspondent of the Smithsonian Institution. Occupied with the important scientific duties incident to his place in the Coast Survey, Mr. Würdemann yet found time for attention to natural history; and at his different stations, of St. Joseph's islands, Texas; Calcasieu, Louisiana; Fort Morgan, Alabama; and Key West, Tortugas, Indian Key, Key Biscayne, Char-lotte Harbor, &c., Florida, made collections which have proved of the greatest service in supplying information concerning the zoology of the Gulf region. Independently of the many new and little known crustacea, shells, and other marine animals collected by Mr. Würde-mann, he succeeded in adding several species of birds to the fauna of the United States; among others, *Larus cucullatus, Certhiola fla-veola, Quiscalus baritus, Corvus Americanus var. floridanus, Ardea Würdemanni*, &c.

16. *Miscellaneous localities.*—A collection of eggs and birds of Il-linois was received from Mr. Tolman; of insects and reptiles of

Kansas, from Mr. H. Brandt; and of eggs, reptiles, and shells, from St. Charles's College, Louisiana. Mr. Theodore Gill, during a vist to Newfoundland, obtained a collection of fishes and invertebrates for the Institution. Mr. Willis, of Halifax, has also furnished valuable collections of eggs and shells. For a statement of many other important additions to the Smithsonian Museum from private sources, I beg leave to refer to the list hereunto appended.

17. *Other parts of the world.*—A series of skins of the tropic-birds, and of eggs of birds breeding on the Bermudas, has proved of much interest for comparison with North American. These were presented by Chief Justice Darrell; a collection of fishes and reptiles from Nicaragua, presented by Dr. H. C. Caldwell, United States navy, embraced several species new to the Smithsonian collection. Mr. McLeannan, of the Panama Railroad Co., has presented to the Institution many species of birds taken on the Isthmus not before in the museum. Captain Dow, of the same company, has also supplied a pair of living curassows.

18. *Miscellaneous collections.*—One of the most important additions to the museum during the year has been made by Mr. William Stimpson, embracing, among other specimens, the whole of his collection of shells of the Atlantic coast, including the types of his "shells of New England," and the "marine invertebrates of Grand Manan." A large portion of the collection consists of specimens in alcohol, of many species, the animals of which are almost entirely unknown elsewhere.

With this collection and that of the exploring expedition, and other parties from the west coast, the Smithsonian Institution has within its walls the best single collection extant of the marine shells of North America.

Dr. Jan has presented, on the part of the Museo Civico, Milan, of which he is director, a large number of species of snakes, for the most part types of his great work on serpents.

From the zoological museum of the University of Copenhagen, Dr. J. J. Steenstrup, director, the Institution has received types of various species of radiates, crustacea, &c., described by C. F. Lütken, Dr. H. Kroyer and others.

WORK DONE IN CONNEXION WITH THE COLLECTIONS.

Much progress has been made during the year in putting the specimens already exhibited in the museum in order, in adding additional ones, and in properly labelling and arranging the whole. So much, however, is required to be done, that it is not to be wondered at if the amount actually accomplished is not at first fully realized by the visitor. The taxidermist has completed the change of stands of all the mammals and of the North American birds on the south side of the hall, and is now engaged on the exotic birds. He has also mounted several hundred birds, chiefly from fresh specimens, for the purpose of exhibiting deficient species of more special interest.

A considerable number of large quadrupeds have also been

mounted and put in place—such as two grizzly bears, a cinnamon bear, an antelope, a pair of mule deer, several wolves, &c.

The labor of cataloguing, entering, and arranging the collections has also been diligently continued during the year, as shown by the accompanying table. Much assistance in this has been derived from the voluntary services of Mr. Robert Kennicott, Mr. E. Coues, Mr. Prentice, Mr. A. J. Falls, and others.

Table exhibiting the entries in the record books of the Smithsonian Museum in 1859, in continuation of previous years.

	1851.	1852.	1853.	1854.	1855.	1856.	1857.	1858.	1859.
Mammals	....	114	198	351	1,200	2,046	3,200	3,226	3,750
Birds........................	....	....	....	4,353	4,425	5,855	8,756	11,390	15,913
Reptiles	....	....	....	....	....	106	239	4,370	4,616
Fishes	....	....	....	....	....	155	613	1,136	1,740
Skeletons and skulls	911	1,074	1,190	1,275	2,050	3,060	3,340	3,413	3,650
Eggs of birds........	....	....	....	....	....	....	....	1,032	2,525
Crustacea	....	....	....	....	....	....	....	939	939
Mollusks	....	....	....	....	....	....	....	....	2,000
Radiates	....	....	....	....	....	....	....	....	1,100
Fossils	....	....	....	....	....	....	....	....	171
Minerals	....	....	....	....	....	....	....	....	793
Total.................	911	1,188	1,388	4,979	7,675	11,222	16,158	25,506	37,197

The actual number of entries during the year amounts to 11,691, being the difference between the aggregates of 1858 and 1859, and 2,343 more than the entries of 1858. As explained in previous reports, these numbers are far from exhibiting the aggregate of specimens catalogued, except in the case of mammals, birds, and osteological preparations.

Each entry generally includes all specimens of the same species received at one time from a given locality, and may embrace hundreds of individual objects. Thus in the oological catalogue, one number covers 242 eggs of the white ibis, *Ibis alba*, received from Dr. Bean. In a similar manner one entry of shells may embrace hundreds or even thousands of specimens. It will not be too high an estimate, probably far below it, to assume 5 specimens as the average to each number, making 185,985 as the aggregate of objects entered on the record books.

At the present time there remain to be catalogued as many, probably, as 10,000 jars of fishes and other alcoholic specimens, independently of the entire series of the crustacea, radiates, &c., collected by the North Pacific and Behring Straits Expedition; the fishes of the Wilkes Exploring Expedition, in the hands of Professor Agassiz, embracing over a thousand species; the skins of the birds of the same expedition; and the shells, radiates, &c.; many thousand series of fossils, vegetable and animal, both vertebrate and invertebrate; all the insects,

worms, microscopical preparations, &c.; a large portion of the eth-
nological collections, the entire herbarium, &c., amounting, in all
probability, to 100,000 numbers.

The series of skins and eggs of North American birds has been
entirely rearranged during the year to accommodate the large increase
both of the species and the specimens. This has only been done ap-
proximately in regard to the mammals, as the additions have been so
great, since their first arrangement in 1857, as to require a study of
the entire class in order properly to determine their names.

The corals have all been arranged and fully labelled by Professor
J. D. Dana during the year, and now constitute a highly interesting
and important feature of the public collection.

During the year the services of Mr. P. P. Carpenter, the well
known conchologist, having been secured, he is now at work upon the
arrangement and labelling the shells, the whole labor of which will
probably be completed in 1860. The co-operation of all the Ameri-
can conchologists, known as original investigators, has also been ob-
tained, and the objects of their especial attention submitted to them
for determination; the *Unionidæ* to Mr. Lea, assisted by Dr. E.
Foreman; the *Helicidæ* to Mr. Binney; the east-coast shells to Mr.
Stimpson; the west-coast to Mr. Carpenter himself; while Dr. Gould
has identified the new species of the exploring expedition, and ren-
dered aid in the criticism of the collection generally. Much assist-
ance has also been rendered by Mr. J. G. Anthony, Mr. James Lewis,
Dr. Newcomb, Mr. Lapham, and other gentlemen, who have made
conchology a speciality.

In the proposed method of arrangement of the shells, the types of
descriptions, and a good representative of each species in different
ages and varieties, will be cemented to square plates of glass; the se-
ries, illustrating geographical distribution, being kept in trays.

A specimen of each species of North American shell will be ex-
hibited on the glass plates in table cases, lined on the bottom with
black paper; but of exotic shells there will only be table surface
enough for a type of each genus. The other portions of the series
will be kept in drawers below the tables. It is also proposed, for the
more ready appreciation of points connected with geographical dis-
tribution, to keep in separate series the shells of northern China,
Japan, and the North Pacific; the boreal shells of the west coast as
far south as San Diego; the marine shells from San Diego to Panama;
the marine shells of the west coast of South America; the marine shells of
the Atlantic coast to Fernandina, Florida; the marine shells of Florida,
the West Indies, and the Gulf of Mexico; and the different American
land and fresh water species. The shells of the rest of the world, with
a few exceptions, will probably be arranged in one systematic series.

As stated in the last report, a large portion of the collections in
charge of the Smithsonian Institution are in the hands of various
eminent naturalists for determination. Professor Agassiz and Mr. J.
C. Brevoort have different portions of the series of fishes; Mr. Bar-
nard, certain Echini; Professor Agassiz, the turtles; Mr. Cassin the
South American birds; Mr. Lyman, the *Ophiuridæ;* Mr. Putnam, the

Etheostomoids; Drs. Torrey and Gray, the exploring expedition plants, &c. None of the collections, stated last year to be out of the building, have yet been returned.

Much progress has been made upon the descriptive works on North American *Diptera*, by Dr. Loew and Baron Osten Sacken; on *Neuroptera*, by Dr. Hagen; on *Hymenoptera*, by Mr. De Saussure; on *Coleoptera*, by Dr. Leconte; on *Hemiptera*, by Mr. Uhler; and on *Lepidoptera*, by Dr. Morris, based, to a greater or less extent, on specimens supplied by the Smithsonian Institution. It is hoped that nearly all these works will be submitted to the Institution during 1860; and their publication cannot fail to give an impetus to the study of entomology, so important in its application to the interests of agriculture. In this connexion it may be proper to state that a circular has been drawn up by Mr. Uhler, and published by the Institution, embodying numerous queries, having for their object the eliciting such information respecting the habits and history of the grasshopper tribes of North America as may serve as a basis of operations in restraining their ravages.

A circular intended to secure contributions of shells from different localities in North America, and a new edition of the circular in reference to North American eggs, have also been prepared for distribution.

PRESENT CONDITION OF THE COLLECTIONS.

In the last report I presented a list of all the great collections constituting the bulk of the museum of the Smithsonian Institution, and the additions since then are enumerated in the following pages.*

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* For convenience of reference, it may be well to continue the enumeration here from page 55 of the report for 1858 :

50. Collections made in Kansas, Nebraska, and Utah, by Captain J. H. Simpson, U. S. A.
51. Collections made on the South Pass wagon-road route, under F. W. Lander, esq.
52. Collections made on the El Paso and Fort Yuma wagon-road route, by J. B. Leech, esq.
53. Collections made on the wagon-road route from Walla-Walla to Fort Benton, by Lieutenant John Mullan, U. S. A.
54. Collections made during an exploration of the Upper Missouri and Yellowstone, by Captain J. W. Raynolds, U. S. A.
55. Collections made during the exploration of the San Juan and Upper Colorado, by Captain J. N. Macomb, U. S. A.
56. Collection made in an exploration of Cape St. Lucas, Lower California, by John Xantus, esq.
57. Collections made at Fort Crook, by John Feilner, esq.
58. Collections made in the Arctic regions, by Robert Kennicott, esq.
59. Collections made in Illinois, by J. W. Tolman, esq.
60. Collections made in Central Florida, by Dr. J. B. Bean.
61. Collections made in Kansas, Nebraska, and Utah, by Dr. Suckley, U. S. A.
62. Collections made in the Bahamas, by Mr. William Cooper.
63. Collections made in the Bahamas, by Dr. H. Bryant.
64. Collections made in South Florida, by Dr. J. G. Cooper.
65. Collections made in the Tortugas, by Captain H. G. Wright, U. S. A.
66. Collections made in the Tortugas, by Captain D. P. Woodbury, U. S. A.
67. Collections made in the Tortugas, by Dr. Whitehurst.
68. Collections made in Louisiana, by the professors and students of St. Charles College
69. Collections made on the Isthmus of Panama, by James McLeannan, esq.
70. Collections made in the West Indies and in Newfoundland, by Theodore Gill.
71. Collections from the Saskatchawan, by Captain John Blakiston, R. A.
72. Collections made by Commodore Perry on the Japan expedition.

The more prominent results of additions to the museum during the current year are to be seen in the approach to completion of the series of North American mammals and birds by the reception of such rare species as *Lagomys princeps*, *Aploceros montanus*, &c. Specimens of the musk ox, barren ground bear, and reindeer, with other species, collected by Mr. B. R. Ross for the Institution, were detained at Pembina with Mr. Kennicott's collections, having reached that point too late in the season to come through to St. Paul.

Among the species of birds added in 1859 may be mentioned *Phaeton flavirostris*, *Puffinus obscurus*, *Xema sabini*, *Rosthramus sociabilis*, *Vireo altiloquus*, *Sula personata*, *Tetrao richardsonii*, *Lagopus leucurus*, and adult *Colymbus arcticus* and *Podiceps arcticus*, in summer dress, with the numerous novelties from Cape St. Lucas sent by Mr. Xantus.

The series of South American birds has been largely increased in number of species. Many very rare North American eggs have been added to the collection. The other specimens received, with the exception of Mr. Stimpson's Atlantic coast shells, have served to fill up slight gaps in species or localities, rather than to bring any to a condition of completeness.

LIST OF DONATIONS TO THE MUSEUM DURING 1859.

Abbott, William.—Skins of birds from Florida.

Adams, Alvin.—Bark of giant tree of California (*Sequoia gigantea.*)

Adams, Alvin, William R. Patterson, Dr. Nichols, Dr. Emmerson, Dr. Emmerton, Dr. Thorndyke, Dr. Shrady, Dr. Robinson, and Dr. Hells.—Stalactite from the "Gothic Chapel" of the Mammoth Cave.

Akhurst, J.—Skins of birds from Central America, eggs of European and American birds, bats in alcohol from the West Indies.

Amherst College.—Eggs of Massachusetts birds.

Anderson, Dr. W. W., United States army.—Skins of birds and mammals from Rocky mountains.

Andrews, Professor E. B.—Fishes and reptiles in alcohol, Ohio.

Arnold, Benjamin.—Indian relics from Rhode Island.

Ashmead, Samuel.—Set of marine algae of United States coast.

Ashton, T. B.—Box of birds' eggs from New York.

Audubon Naturalists Club of Chicago.—Mounted specimen of *Grus americana*, or whooping crane.

Ayres, Dr.—Birds' eggs of California.

Baird, S. F.—Skin of *Arctomys monax*, New York.

Baird, William M.—Eggs of birds of New Jersey.

Baker, E. B.—Alcoholic collections, skins of birds, and egg of *Pandion* ——? Charlotte Harbor, Florida.

Barnston, George.—Skins of fishes, birds' eggs, &c., specimens in alcohol, from Lake Superior.

Beadle, Delos W.—Fifteen bottles alcoholic collections, Canada.

Bean, Jr. J. B.—Eggs of birds; reptiles, fishes, &c., from Florida.

Bedford, Alexander.—Fossils, Michigan.

Bellman, C.—Reptiles, fishes, &c., from Mississippi.

Benners, Henry.—Specimens in alcohol from Tortugas.

Berthoud. Dr. E. L.—Fossils from Kansas and Isthmus of Panama.

Blake, W. P.—Minerals, and eggs of birds from Georgia.

Blanc, Rev. A.—Eggs of birds of Louisiana and other zoological collections.

Bloomfield, Captain R. N.—Box of "coca" leaves.

Bossard, P.—Box of shells from Ohio.

Boston Society of Natural History.—Series of plants collected in California, by E. Samuels.

Brackett, George E.—Skin of star-nosed mole.

Brandt, H.—Box of insects, reptiles, and mammals, Kansas.

Brewer, Dr. T. M.—Eggs of fifty-seven species of European birds.

Brewster, Miss L.—Nests, eggs of birds from Massachusetts.

Brown, Dr. George C.—Skins of *Pityophis melanoleucus*, New Jersey; eggs of birds.

Brugger, Samuel.—Nests and eggs of birds, and mammals, Centre county, Pennsylvania.

Bryant, Dr. H.—Fishes, skins of birds, and eggs from the Bahamas.

Caldwell, Dr. H. C., United States navy.—Birds and alcoholic collections from Nicaragua.

Campbell, A.—Nine boxes natural history collections from northwest boundary survey, near Puget's Sound, collected by Dr. Kennerly, and one collected by George Gibbs.

Canfield, Dr. C. A.—Collection of eggs of Californian birds.

Carleton, Major J. H., United States army.—Shells from the Colorado desert.

Carter, B. F.—Shells from Texas.

Catley, H.—Lepidopterous insects from Oregon.

Churchill, R. C.—Eggs and birds from Maryland.

Clark, J. H.—Coal from Rabbit-ear Creek, Texas.

Clark, William.—Centipede; Van Buren, Arkansas.

Clark, W. P.—Box of eggs and can of fishes from Ohio.

Cope, E. D.—Type specimens of *Desmognathus ochrophea*, Pennsylvania.

Copenhagen Zoological Museum.—Type specimens of marine invertebrates of North Seas and West Indies.

Cooper, W.—Fifty-nine species of shells of Bahamas, &c., collection of Bahama fishes; shells and echinoderms of the Atlantic coast of the United States.

Cooper, Dr. J. G.—Specimens of woods, reptiles in alcohol, and eggs of Pipiry flycatcher from Florida.

Corey, O. W.—Box of fossil crinoids from Indiana.

Craig, Dr., United States army.—Skin of black wolf of Washington Territory.

Curley, Professor.—*Gordius* or hair worm in alcohol.

Danker, Henry A.—Nests and skins of birds from New York.

Davis, William A.—Fossils; Illinois.

Darrell, Chief Justice.—Skins and eggs of Bermuda birds; reptiles in alcohol.

Dawson, Professor J. W.—Fossil *Pupa vetusta* from Nova Scotia.

Day, Mrs. Thomas W.—Collection of eggs from California.

Diggs, Captain.—Skin of otter, Washington Territory.

Dorsey, William.—Minerals from Chile.

Dow, Captain J. M.—Alcoholic specimens, Nicaragua; and silver ore from San Salvador; two living Curassows, Central America.

Drexler, C.—Mounted birds and fishes.

Dunbar, R. W.—Skeleton of sea otter; specimens of woods and Indian curiosities, coast of Oregon.

Dunham, Ed.—Mounted specimens of curlew and grayback snipe.

Duval, Alfred.—Seeds, silk cotton, minerals, &c.; Payta, Peru.

Eastman, S. C.—Eggs of birds from Labrador.

Edwards, W. A.—Eggs of frigate pelican, echini and crustacea, Central America.

Edwards, Amory.—Brazilian "coal."

Falls, A. J.—Fresh birds, Washington.

Fisher, Dr. G. J.—Eggs of hawks from New York.

Flint, C. L.—Nest of golden crowned thrush.

Force, Colonel P.—Hornets' nest; Washington.

Gantt, Dr. W. H.—Nests and eggs of birds from Texas.

Gardiner, Capt. J. W., United States army.—Skin and skeleton of Fisher (*Mustela pennantii;*) Indian nets, Fort Crook, California.

Gary, Jacob S.—Fossil wood, Ohio.

Gerhardt, A.—Skins and eggs of birds from Georgia, with alcoholic specimens.

Gibbs, George.—Seeds of Coniferae; Puget's Sound.

Gibbes, Professor L. R.—Box of fresh shrips.

Gill, Theo.—Birds, reptiles, fishes, &c., of Trinidad and Newfoundland.

Gilliss, Lieutenant J. M., United States navy.—Eye of *Loligo*, from a grave at Areca, Peru; *Etheostoma* from Washington.

Gilpin, Dr.—Mammals and reptiles in alcohol from Nova Scotia.

Glover, T.—Living sandhill crane (*Grus canadensis*) with zoological collections in alcohol, Florida.

Godfrey, Mrs. A. D.—Skin of snow goose from Puget's Sound.

Gordon, Dr. H. N.—Skin of young cinnamon bear (*Ursus Americanus*,) Illinois.

Goss, B. F.—Eggs of birds and skins of mammals from Kansas.

Grayson, A. J.—Two boxes of skins and eggs of birds, and bottle of reptiles from California.

Gunn, Donald.—Skins of birds and mammals from North Red River.

Hall, Dr. J.—Living *Anolis carolinensis*, Alabama.

Hampton, W. C.—Seeds of Australian plants.

Harvey, Prof. W. H.—Collection of Australian marine algae.

Hayden, Dr. F. V.—(See Captain Raynolds.)

Hayes, Dr. S.—Mezquite gum and alcoholic collections; dried plants Western Texas, New Mexico, and California; skins of birds; *Scalops latimanus* in alcohol, &c., Fort Belknap, Texas.

Hayes, Dr. S.—(See Colonel Leech.)

Hoy, Dr. P. R.—Skins of rare birds of Wisconsin.

Haymond, Dr. R.—Nests and eggs of birds of Indiana.

Hepburn, J.—Skins and eggs of California birds.

Hine, D.—Cannel coal, Ohio.

Hinman, W. M.—Package of plants from Laramie Peak.

Holden, W.—Thirty-six species of Univalve shells from Ohio.

Hoopes, B. A.—Reptiles and mammals in alcohol, Lake Superior.

Hopkins, Prof. Wm.—Birds' eggs, New York.

Hopkins, Arch.—Birds' eggs, Massachusetts.

Hubbard, Chas. A.—Skins and eggs of birds from the Red River of the North.

Hunter, Dr. C. L.—*Arvicola pinetorum* from North Carolina.

Interior Department.—(See Colonel Leech.)

Irwin, Dr., U. S. A.—Box of birds from New Mexico.

Jan, Dr. C.—Type series of exotic serpents from the Museo Civico, Milan.

Jardine, Sir W.—Skin of *Plectrophanes lapponica* from Arctic America.

Jenkins, J. H.—Shells of Washington Territory.

Johns, Dr., U. S. A.—Fragments of Mastodon remains from Scott's Bluffs.

Kellogg, F.—Tertiary shells from Texas.

Kennedy, J. M.—Four specimens of birds from New Mexico.

Kennerly, Dr.—Seeds of *Thuya gigantea,* Washington Territory. See also A. Campbell.

Kennicott, R.—Zoological collections made between Lake Superior and Lake Winnipeg.

King, Dr. W. S., U. S. A.—Skin of Spermophile and Mexican weasel from Texas.

Kohler, W.—Lead ores from Union mines, Austinville, Virginia.

Krider, John.—Eggs of fish hawk.

Kuhn, Dr. L. de B.—Skull of bears, shells, insects, &c., Washington Territory.

Latchford, Thomas.—Fossil bones from Laurel Factory.

Lawrence, Geo. N.—Skins of birds from the Atlantic coast.

Leech, Colonel.—Collections of plants made by Dr. Hayes on the El Paso wagon road expedition in Texas, New Mexico, and California.

Libhart, J. J.—Box of birds' eggs, Pennsylvania.

Loomis, Miss M.—Fossil seeds from Burlington, Vermont.

Luther, Dr. S. M.—Shells and birds' eggs of Ohio.

McCarthy, Charles S.—See Captain J. H. Simpson.

McCown, Captain J. P., U. S. A.—Box of Kansas birds.

McCurdy, Dr. Samuel.—Skins of harlequin duck, Washington Territory.

McCurdy, S. B.—Curiously marked wood.

McIlvaine, J. H.—Eggs of birds.

McKee, Dr. J. Cooper, U. S. A.—Skins of mammals and birds, Fort Defiance.

McLain, William M.—Birds and eggs from Maryland; fish hook used by the natives of the Sandwich Islands.

McLeannan, James.—Sixty species birds from Isthmus of Panama.

McWilliams, Dr.—Skins of blue grosbeak and yellow-billed cuckoo.

Mallet, Prof. J. W.—Fossils from Alabama.

Martin, T. S.—Skins of birds of California.

Merrill, Lieut., U. S. A.—Box fossils from Green Bay, Wisconsin.

Moore, Miss J.—Living hawk moth from near Washington.

Moore, Dr. George F.—Birds' nests and eggs from North Carolina.

Morris, Rev. Dr. John G.—Box European eggs.

Mullan, Lt. John, U. S. A.—Three boxes zoological and geological collections made by John Pearsall on the Upper Missouri.

Nichols, Dr. C. H.—Armadillo from Paraguay.

Nunn, R. J.—Can of crustacea from Georgia.

D' Oca, R. Montes.—Mammals; reptiles of Mexico.

Odell, B. F.—Living rattlesnake from Iowa.

Packard, A. S.—Reptiles and mammals in alcohol; eggs of birds from Maine.

Page, Captain, T. J., U. S. N.—Sixty species of birds from Paraguay.

Paine, Charles S.—Box of birds' eggs from Vermont.

Paine, H. M.—Eggs of birds from New York.

Poey, Prof. F.—*Lucifuga subterranea*, a new blind fish of Cuba.

Pearsall, John.—See Lieutenant Mullan.

Peña, Don F. de.—Box of "coca" leaves.

Peters, R.—Skin of Cotton rat, *(Sigmodon hispidum,)* Georgia.

Peters, T. M.—Ferns, *(Trichomanes,)* from Alabama.

Piers, Dr., Royal navy.—Skin of *Thalassidroma furcata* from Barclay sound, northwest coast.

Plummer, Captain J. B., U. S. A.—Skins of birds, reptiles and fossils from Texas.

Porter, Prof. T. C.—Bottle of fishes, *(Rhinichthys,)* Pennsylvania.

Postell, J. P.—Specimens of *Unio spinosus* from Georgia.

Prentiss, D. W.—Skin of *Sorex* from near Washington.

Putnam, F. W.—Nest and eggs of birds, Massachusetts.

Rae, Dr.—Skins of *Spermophilus parryi*, Repulse bay, latitude 56° 30′ N.

Raymond, C.—Birds and other animals, Peru.

Raynolds, Captain W. F., U. S. A.—Zoological collections made in the Upper Missouri region by Dr. F. V. Hayden.

Reid, Peter.—Eggs of Hawk.

Richard, J. H.—Mounted European birds.

Roanoke Library Association.—Fossil bone from the green sand of North Carolina.

Robinson, E. S.—Nests and eggs from Mississippi.

St. Louis Academy of Science.—Serpent in alcohol, *(Celuta vermis,)* supposed to have been found in a block of stone.

Samuels, E.—Mounted microscopical preparations.

Sclater, P. L.—Mexican and Central American birds.

Simpson, Captain J. H., U. S. A.—Zoological collections made in Utah by Charles S. McCarthy.

Simpson, Dr., U. S. A.—Mounted snow owl, Rhode Island.

Smith, A. C., M. D.—Can of fishes from Ohio.

Smith, J. W.—Boring and other shells from Puget's Sound.

Sowthwick, Dr.—Skins of *Sciurus abertii*, Fort Union, New Mexico.

Spence, Mrs. David.—Collection of eggs from California.

Steele, Judge.—File fish in alcohol from Florida.
Stimpson, William.—Type series of marine invertebrates of Atlantic coast.
Stockton, Natural History Society.—Nests and eggs of California birds.
Suckley, Dr. George.—Harpoon used by Indians in catching whales on northwest coast, with other Indian curiosities. Zoological specimens from Kansas.
Swift, Dr., U. S. A.—Skin and skull of bighorn and horns of elk from Rocky mountains.
Taylor, A. S.—Box of crustaceans and shells, with other zoological collections from California; skull of grizzly bear.
Tolman, J. W.—Two boxes birds' eggs from Illinois.
Trask, Dr.—Crustaceans of California.
Trembley, Dr. J. B.—Shells and eggs from Ohio.
Tuley, Colonel James.—Fresh skin of elk.
Tully, Bernard.—Stalactites from Weir's Cave.
Vagnier, Thomas.—Nests and eggs of birds from Indiana.
Van Bokkelen, Lieutenant J. J.—Skin of puffin, Washington Territory.
Vaughan, Colonel A. J.—Foetal beaver and fossil coral; skulls of mammals and skins of birds from Upper Missouri.
Vickary, N.—Nests and skins of birds from Massachusetts.
Vialleton, Rev. F. and Rev. A. Blanc.—Collections of shells, eggs, insects, and reptiles from Louisiana.
Vollum, Dr. E.—Box of zoological specimens in alcohol, skins of animals, and birds' eggs from the coast of Oregon.
Vortisch, Rev. L.—Aboriginal stone hammer of green stone, Germany.
Walsh, Benjamin D.—Skin of bat and nest of humming bird from Illinois.
Walton, Hon. E. P., M. C. — Spawn of conch from Buzzard bay, Massachusetts.
Warren, Lieutenant J. R., U. S. A. — Box of Indian curiosities, (deposited.)
Waugh, A. Townsend.—Box insects, Maryland.
Weeks, W.—Bottle of fishes from Connecticut river.
Welch, George.—Eggs of birds of Massachusetts.
White, Lieutenant J. W., and officers of the "Jeff. Davis."—Zoological collections, chiefly invertebrates, from Puget's Sound.
Whitehurst, Dr. D. W.—Eggs of birds and specimens in alcohol from Tortugas.
Williams, E. C.—Large plank cut from redwood tree, (*Sequoia sempervirens,*) Cape Mendocino, California.
Willis, J. R.—Nests and eggs of birds from Nova Scotia.
Wilson, Dr. L. N.—Skins of carrion crow, (*Cathartes atratus*;) white ibis, &c., and dried grasses from Georgia.
Wolcott, F. H.—Box of "vegetable eggs."
Wood, Dr. W.—Birds' eggs from Connecticut.
Woodbury, Captain D. P., U. S. A.—Fishes and marine invertebrates in alcohol; skins and eggs of birds, and dried corals from Tortugas.

Wright, Charles.—Minerals and fossil radiates from Cuba.

Würdemann, G.—Twelve skins of birds, Florida.

Xantus, John.—Twenty boxes of natural history collections from Fort Tejon, California.

Unknown.—Box of coal; Indian remains from Scioto river; larvae of *Telephorus?* from the surface of snow in Oneida county, New York; package auriferous earth from Pike's Peák.

LIST OF METEOROLOGICAL STATIONS AND OBSERVERS

FOR THE YEAR 1859.

BRITISH AMERICA.

Name of observer.	Station.	N. latitude.	W. longitude.	Height.	Instruments.°
		° ′	° ′	*Feet.*	
Baker, J. C.	Stanbridge, Canada East, (P. O. Saxe's Mills, Vt.)	45 08	73 00		T.
Connelly, Henry	Rigolet, Labrador				B. T.
Craigie, Dr. W	Hamilton, Canada West	43 15	79 57		B. T.
Delany, Edward M. J.	Colonial Building, St. John's, Newfoundland.	47 35	52 41	170	B.T.R.
Gunn, Donald	Red River Settlement, Hudson's Bay Territory.	50 06	97 00	853	T. R.
Hall, Dr Archibald	Montreal, Canada East	45 30	73 36	57	A.
Hartt, Chas. Fred'k	Acadia College, Wolfville, Nova Scotia.	45 06	64 25	95	A.
Hensley, Rev. J. M	King's College, Windsor, Nova Scotia.	44 59	64 07	200	A.
Magnetic Observatory.	Toronto, Canada West	43 39	79 21	108	A.
Mackenzie, J	Moose Factory, Hudson's Bay Territory.	51 15	80 45		B. T.
Ross, Bernard R.	Fort Simpson, Hudson's Bay Territory.	61 51	121 25		T.
Royal Engineers	Halifax, Nova Scotia	44 39	63 37	8	A.
Smallwood, Dr. Charles.	St. Martin, Isle Jesus, Can. E.	45 32	73 36	118	A.

ALABAMA.

Name of observer.	Station.	County.	N. latitude.	W. longitude	Height.	Instruments.
			° ′	° ′	*Feet.*	
Alison, H. L., M. D.	Carlowville	Dallas	32 10	87 15	400	T. R.
Cobbs, Rev. R. A.	Union Town	Perry	32 30	87 31		N.
Foster, William L.	Montgomery	Montgomery	32 22	86 31		N.
Hurt, Ashley D.	Moulton	Lawrence	34 32	87 25	643	A.
Jennings, Dr. S. K. {	Selma	Dallas	32 25	86 51	200	T.P.R.
	Orville }		32 24	87 06		T.P.R.
Nicholson, Rev. J. J.	Mobile	Mobile	30 30	88 30	188	B. T.
Shepherd, Rev. J. Avery	Montgomery	Montgomery				T. R.
Smith, Rev. Stephen U.	Livingston	Sumter	32 30	88 16	180	T. P.
Troy, Matthew, M. D.	Cahaba	Dallas	32 19	87 16		T.
Tutwiler, Henry.	Havana	Greene	32 50	87 46	500	T. R.
Waller, Robert B.	Greensboro'	Greene	32 40	87 34	350	A.

* A signifies Barometer, Thermometer, Psychrometer, and Rain Gage.
  B " Barometer.
  T " Thermometer.
  R " Rain Gage.
  N " No instrument.

ARKANSAS.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
			o ′	o ′	Feet.	
Barlow, Dennis........	Arkadelphia .	Clark.	34 08	93 00	....	N.
Blackwell, W. H.......	Perryville. ..	Perry.	35 05	93 16	....	N.
Coulter, B. F..........	Brownsville..	Prairie	34 50	92 00	....	N.
Davies, James T.......	Gainesville ..	Greene......	....	90 00	500	N.
Featherston, Geo. W....	Waldron	Scott........	34 53	94 00	....	N.
Finley, P. F..........	Spring Hill ..	Hempstead ..	33 30	93 40	....	T.
Flippin, W. B....	Yellville	Marion......	36 30	93 00	1,000	N.
Graham, Paul.........	Bentonville ..	Benton......	36 23	94 10	1,790	N.
Martin, G. Alex., M. D..	Jacksonport..	Jackson......	35 56	91 16	....	T.
Reynolds, J...........	Spring Hill ..	Hempstead ..	33 30	93 40	....	T.
Smith, Dr. N. D.	Washington..	Hempstead ..	33 44	93 41	....	T. R.
Weast, J. W.........	Yellville	Marion......	36 30	93 00	1,000	N.
Younger, Armistead....	Buck Horn....	Independence.	34 30	92 00	650	N.

CALIFORNIA.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
°Ayres, W. O., M. D....	San Francisco.	San Francisco.	37 48	122 27	130	A.
°Belcher, W. C.........	Marysville...	Yuba.	39 29	121 30	80	T.
Boucher, Wesley K......	Mokelumne Hill.	Calaveras....	38 18	120 28	1,502	N.
Canfield, Colbert A.,M.D.	Monterey....	Monterey....	36 36	121 54	40	T.
Frombes, Prof. Oliver S..	Santa Clara..	Santa Clara..	37 18	122 00	100	A.
Gordon, Robert.........	Auburn.	Placer........	38 54	121 12	1,176	T.
Gould, Lewis A.........	Santa Clara..	Santa Clara..	37 20	122 00	98	N.
°Logan, Thos. M., M. D.	Sacramento ..	Sacramento ..	38 35	121 28	41	A.
Randall, Robert B.......	Crescent City.	Del Norte. ..	41 45	124 11	12	N.
Slaven, James....	Honcut	Yuba	39 25	121 30	....	T. R.

DISTRICT OF COLUMBIA.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
Smithsonian Institution.	Washington .	Washington .	38 53	77 01	60	A.

CONNECTICUT.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
Harrison, Benj. F......	Wallingford..	New Haven..	41 27	72 50	133	A.
Hunt, Rev. Daniel......	Pomfret.....	Windham ...	41 52	72 23	587	A.
Johnston, Prof. John....	Middletown..	Middlesex. ..	41 32	72 39	175	A.
Rankin, James.........	Saybrook	Middlesex ...	41 18	72 20	10	T. R.
Yeomans, William H....	Columbia....	Tolland.	41 40	72 42	....	T.

DACOTAH.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
Norvell, Freeman.......	Greenwood ..	....	42 52	98 24	1,900	T. R.

* The names of these observers were accidentally omitted in the list for 1858.

DELAWARE.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
			° '	° '		
Mauld, Dr. D. W........	Georgetown...	Sussex	....	....	....	R.

FLORIDA.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
Abert, Thayer	Warrington...	Escambia ...	30 21	87 16	9	A.
Bailey, James B.........	Gainesville ..	Alachua......	29 35	82 26	184	T. R.
Baldwin, A. S., M. D....	Jacksonville .	Duval........	30 15	82 00	14	A.
Bean, Dr. James B	Micanopy.....	Alachua......	29 35	82 31	78	A.
Dennis, Wm. C..........	Key West	Monroe	24 33	81 28	16	B. T.R.
Gibbon, Lardner........	Tallahassee ..	Leon	30 29	84 07	....	T.
Ives, Edward R.........	Lake City ...	Columbia....	30 12	82 37	174	T. R.
Mauran, P. B , M. D....	St. Augustine	St. John's ...	29 48	81 35	8	B. T.R.
Steele, Judge Aug.......	Atsena Otie ..	Levy	29 08	83 04	17	B. T.R.
Whitner, Benj. F	Tallahassee ..	Leon	30 24	84 17	70	T.

GEORGIA.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
Anderson, Jas., M. D ...	Thomaston ..	Upson	32 56	84 30	750	A.
Arnold, Mrs. J. T.......	Zebulon.	Pike	33 07	84 26	....	T.
Camp, Benj. F..........	Covington ...	Newton	33 40	84 00	763	N.
Doughty, Dr. Wm. H...	Augusta......	Richmond ...	33 27	81 33	152	A.
Easter, Prof. John D....	Athens.......	Clarke	33 58	83 35	850	A.
Gibson, R. T	Savannah ...	Chatham	32 02	81 01	18	T. R.
Grant, W. T., M D.....	Thomson	Columbia....	33 26	82 28	....	B. T R.
Pendleton, E. M., M. D.	Sparta	Hancock	33 17	83 09	550	T. R.
Posey, John F., M. D...	Savannah ...	Chatham	32 05	81 07	42	A.
Stanford, Col. John R...	Clarksville...	Habersham ...	34 40	....	....	N.
Van Buren, Jarvis......	Clarksville ..	Habersham ..	34 35	83 31	1,632	T. P.
Westmoreland,J.G., M.D.	Atlanta	Fulton	33 45	84 31	1,050	B. T.R.

ILLINOIS.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
Aldrich, Verry.........	Tiskilwa.....	Bureau......	41 15	89 66	550	N.
Allison, Jesse..........	Bloomington.	McLean ,....	40 30	89 00	....	N.
Babcock, Andrew J.....	Aurora	Kane	41 41	88 17	650	T. R.
Babcock, E............	Riley	McHenry . ..	42 11	88 20	760	T. R.
Bacon, E. E	Willow Creek.	Lee........	41 45	88 56	1,040	T.
Ballou, N. E., M. D.....	Sandwich . ..	De Kalb	41 31	88 30	575	T. R.
Bassett, George R	Woodstock ..	McHenry . ..	42 18	88 30	....	T. R,
Bowman, Dr. E. H.....	Edgington...	Rock Island.	41 25	90 46	686	T. R,
Brendel, Fred'k, M. D..	Peoria	Peoria	40 43	89 30	460	T.P.R.
Brookes, Samuel	Chicago	Cook	42 00	87 30	....	T.
Cantril, Joshua E	Waynesville .	De Witt	40 16	89 07	....	T. R.
Capen, E..............	Batavia	Kane	41 52	88 20	636	B T.
Cobleigh, N. E.........	Lebanon	St. Clair.....	38 37	89 56	....	B. T.R.

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ILLINOIS—Continued.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
			° ′	° ′	*Feet.*	
Bell, Mrs. E. M. A---- } Wallace, Sam'l Jacob- }	Carthage ----	Hancock ----	--------	--------	--------	Γ. R.
Collier, Prof. Geo. H.---	Wheaton. ---	Du Page-----	41 49	88 06	682	A.
Eldredge, Rev.William V.	Newton -----	Jasper ------	--------	--------	--------	T. R.
Ellsworth, Lewis-------	Naperville---	Du Page-----	41 46	88 11	--------	N.
Ellsworth, Milton S-----	Naperville ---	Du Page ----	41 46	88 11	--------	T.
Grant, John-----------	Manchester --	Scott -------	39 33	90 34	683	A.
Haeuser, Emil---------	Galena - -----	Jo. Daviess --	--------	--------	--------	N.
Harris, J. O., M. D-----	Ottawa------	La Salle-----	41 20	88 47	500	T. R.
James, John, M. D ----	Upper Alton-	Madison-----	39 00	89 36	--------	A.
Little, J. Thomas-------	Dixon-------	Lee ---- ----	41 45	89 31	--------	N.
Mead, S. B., M. D------	Augusta-----	Hancock ,---	40 12	91 45	○203	T.P.R.
Mead, Dr. Thompson---	Batavia -----	Kane -------	41 52	88 20	636	A.
Newcomb, John B------	Elgin -------	Kane -------	42 00	88 15	777	A.
Riblet, J. H -----------	Pekin-------	Tazewell ----	40 36	89 45	--------	T. R.
Rogers, O. P. & J. S.-----	Marengo ----	McHenry - --	42 14	88 38	842	B. T. R.
Smith, Chas. E --------	Evanston - --	Cook -------	42 10	87 30	†18	T.
Smith, Geo. O., M. D----	Ottawa------	La Salle-----	40 57	87 55	551	T. R.
Swain, John, M. D------	West Urbana.	Champaign --	40 09	88 17	727	A.
Thomas, Mrs. Wm. S ---	Carbon Cliff--	Rock Island--	41 32	90 29	688	N.
Titze, Henry A --------	West Salem--	Edwards ----	38 30	88 00	--------	Γ. R.
Tolman, James W-------	Winnebago Depot-----	Winnebago --	42 17	89 11	800	T. R.

INDIANA.

Anderson, H. H. -------	Rockville----	Parke-------	36 00	87 00	--------	N.
Austin, W. W. --------	Richmond ---	Wayne------	39 47	84 47	800	T. R.
Bartlett, Isaac ---------	Logansport --	Cass --------	40 45	86 13	600	
Bullock, J. T----------	Shelbyville --	Shelby------	39 00	87 00	--------	.
Chappellsmith, John----	NewHarmony	Posey-------	38 08	87 50	320	A.
Haines, John----------	Richmond---	Wayne------	39 52	84 59	--------	B. T.
Larabee, Wm. H-------	Green Castle-	Putnam-----	39 30	86 47	--------	N.
Martin, Dr. Alex. ------	New Albany -	Floyd ------	38 17	85 45	--------	T.
Menifield, Geo. C.------	Mishawaka --	St. Joseph ---	41 37	86 02	685	T. R.
Moore, Joseph---------	Richmond---	Wayne------	39 47	84 47	800	A.
Smith, Hamilton, jr----	Cannelton ---	Perry -------	37 57	86 42	450	A.
Sutton, George, M. D --	Aurora------	Dearborn----	--------	--------	--------	B T.R.
Turner, A. P------ ----	Patoka------	Gibson------	--------	--------	--------	N.
Vagnier, Prof. Thos. ----	Notre Dame -	St. Joseph ---	41 40	87 10	--------	B.T. P.
Webb, Miss G. --------	South Bend--	St. Joseph ---	41 45	86 20	--------	N.

* Above low water mark at Quincy.
† Above Lake Michigan.

IOWA.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
			° ′	° ′	*Feet.*	
Beal, Dexter ----------	Grove Hill---	Bremer -----	42 45	87 15	---------	T.
Beeman, Carlisle D. ----	Rossville ----	Allamakee---	43 10	91 20	1,400	T. R.
Corse, John M. --------	Burlington --	Des Moines --	40 53	91 10	°486	T. R.
Doyle, L. H. ----------	Waterloo ----	Black Hawk .	42 30	92 31	---------	N.
Finley, H. S. ----------	Davenport ---	Scott --------	41 30	90 38	555	N.
Fory, John C----------	Bellevue ----	Jackson -----	42 15	92 25	---------	T. R.
Goss, William K. ------	Border Plains	Webster -----	42 05	94 00	---------	B.T.R.
Horr, Asa, M. D. ------	Dubuque ----	Dubuque ----	42 30	90 52	°666	A.
Hudson, A. T., M. D----	Lyons--------	Clinton -----	41 50	90 10	401	R.
McBeth, Miss Sue ------	Fairfield ----	Jefferson ----	41 01	91 57	940	N.
McConnel, Townsend ---	Pleasant Plain	Jefferson ----	41 07	94 54	950	T. R.
McCready, Daniel------	Fort Madison	Lee---------	40 37	91 28	---------	T. R.
McKenzie, John M. ----	Fayette -----	Fayette -----	42 51	91 51	1,000	T.
Odell, Rev. Benj. F. ----	Plum Spring-	Delaware----	42 40	91 21	---------	T.
Parvin, Theodore S. ----	Muscatine ---	Muscatine ---	41 25	92 02	586	A.
Shaffer, J. M., M. D. ----	Fairfield ----	Jefferson ----	41 01	91 57	940	A.
Sheldon, Daniel -------	Forestville---	Delaware----	42 40	91 50	---------	T.

KANSAS.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
Berthoud, E. L---- ----	Leavenworth	Leavenworth	39 19	94 50	809	R.
Brown, G. W------ ----	Lawrence----	Douglas-----	38 58	95 12	800	N T.P.
Clarkson, Rev. David---	Fort Riley ---	Riley -------	39 00	96 30	1,300	N.
Collier, D. C------ -----	Denver City -	Arapahos----	--------	--------	--------	T.
Drummond, Rev. J. H..	Celestville. ..	Lykins------	38 40	95 16	---------	N.
Ellis, Dr. Wm. T------	Lecompton --	Douglas-----	39 03	95 10	760	T.
Fish, Lucian ----------	Burlingame--	Shawnee ----	--------	--------	--------	T. R.
Goodnow, Isaac T------	Manhattan --	Riley -------	39 13	96 45	1,000	T. R.
Goss, B. F------ ------	Neosho Falls-	Woodson ----	38 03	95 31	---------	T. R.
McCarty, H. D---- ----	Leavenworth	Leavenworth.	39 20	94 33	1,342	A.
Millar, John H--------	Wyandot----	Wyandot.----	39 08	94 31	707	T. R.
Preston, Rev. N. O-----	Manhattan ..	Riley -------	39 13	96 45	---------	N.
Wattles, John O----- } Wattles, Celestia----- }	Moneka -----	Linn -------	38 30	98 00	---------	T. R.

KENTUCKY.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
Beatty, O.--------------	Danville ----	Boyle-------	37 40	84 30	950	B.T.R.
Lunemann, John H.--- } Miles, Thos H., S. J.-- }	Bardstown---	Nelson------	37 52	85 18	---------	A.
Mattison, Andrew------	Paducah ----	Mt. Cracken .	37 00	87 21	---------	N.
Ray, L. G., M. D.-------	Paris -------	Bourbon ----	38 16	84 07	800	B.T.R.
Savage, Rev. G.S., M. D.	Millersburg--	Bourbon -----	38 40	84 27	804	B.T.R.
Walker, Mrs. Mary A. } Barbage, Joshua C.---- }	Hardinsburg -	Breckenridge	37 40	86 15	500	N.
Williams, Rev. S. R.-- }	Louisville ---	Jefferson ----	38 03	85 30	452	A.
	Lexington---	Fayette ----	38 06	84 18	---------	A.
Young, Mrs. Lawrence--	Louisville.---	Jefferson ----	38 07	85 24	570	A.

* Above low water in the Mississippi.

LOUISIANA.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
			° ′	° ′	*Feet.*	
Jackson, A. W., M. D.	Falls River	Point Coupee	30 20	91 31	....	N.
Kilpatrick, A. R., M. D.	Trinity	Concordia	31 30	91 46	108	T. R.
Swasey, Col. C. B.	Independence	Livingston	30 30	90 33	50	N.
Taylor, Lewes B.	New Orleans	Orleans	29 58	90 07	....	A.

MAINE.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
Adams, John W.	Portland	Cumberland	43 39	70 00	180	N.
Bickford, Calvin	Warren	Lincoln	44 00	70 29	....	N.
Brackett, G. Emerson	Belfast	Waldo	44 23	69 08	....	T. R.
Brown, E. E., and others.	Hartland	Somerset	45 00	69 30	200	N.
Buller, E. A.	Freedom	Waldo	44 30	70 30	....	N.
Dana, W. D.	North Perry	Washington	45 00	67 06	100	B T.R.
Gardiner, R H	Gardiner	Kennebec	44 40	69 46	90	A.
Guptill, G. W	Cornishville	York	43 40	70 44	800	T. R.
Johnson, Warren	Topsham	Sagadahoc	44 00	70 00	100	T.
Lord, W. G	Limington	York	43 40	70 45	500	N.
Moore, Asa P	Lisbon	Androscoggin	44 00	70 04	130	T. R.
Nichols, Charles L. {	Newcastle	Lincoln	44 07	69 35	88	T.
	Bangor	Penobscot	44 48	68 47	....	T.
Parker, J. D.	Steuben	Washington	44 44	67 50	50	A.
Van Blarcom, James	Vassalboro'	Kennebec	44 28	69 47	....	T.
Verrill, G. W., jr.	Norway	Oxford	44 10	70 35	....	N.
West, Silas	Cornish	York	43 40	70 44	784	T. R.
Wilbur, Benj. F.	Dexter	Penobscot	44 55	69 32	700	R.
Willis, Henry	Portland	Cumberland	43 39	70 15	87	A.
Wyman, A. H.	N'th Belgrade	Kennebec	44 30	70 00	....	N.

MARYLAND.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
Baer, Miss H. M.	Sykesville	Carroll.	39 23	76 57	700	T. R.
Bell, Jacob E.	Leitersburg	Washington	39 35	77 30	....	T. R.
Goodman, Wm. R.	Annapolis	Anne Arundel	38 59	76 29	20	A.
Hanshew, Henry E.	Frederick	Frederick	39 24	77 26	....	A.
Lowndes, Benj. O	Bladensburg	Prince George.	38 57	76 58	70	T. R.
Mayer, Prof. Alfred M	Baltimore	Baltimore	39 18	76 37	....	A.
McWilliams, Dr. Alex'r.	Leonardtown	St. Mary's	38 17	76 43	....	B. T.
Stephenson, Rev. Jas.	St. Inigoes	St. Mary's	38 10	76 41	45	A.
Sutton, Rev. A	Chestertown	Kent	39 12	75 59	....	A.

MASSACHUSETTS.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
Adams, Edward L.	Boston	Suffolk	42 20	71 03	....	B. T.
Alcott, William P. } Berger, M. L. } Morley, John H. }	Williamstown	Berkshire	42 43	73 13	725	B.T.R.

MASSACHUSETTS—Continued.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
			° ′	° ′	*Feet.*	
Bacon, William........	Richmond ...	Berkshire....	42 23	73 20	1,190	T. R.
Brewer, Francis A......	Springfield...	Hampden ...	42 06	72 36	....	N.
Collier, Alfred.........	South Groton.	Middlesex ...	42 30	....	300	T.
Davis, Rev. Emerson ...	Westfield....	Hampden ...	42 06	72 48	180	A.
Fallon, John	Lawrence....	Essex	42 42	71 11	133	A.
Felt, Charles W...... ⎫ Hanna, George B .. ⎬ Parsons, Benj'n W.. ⎭	Bridgewater .	Plymouth ...	42 00	71 00	150	A.
HarvardColl.Observatory	Cambridge...	Middlesex ...	42 23	71 07	80	A.
Mack, A. W...........	Danvers......	Essex	42 35	71 03	....	B. T.
Metcalf, Jno. G., M. D..	Mendon.	Worcester ...	42 06	71 33	....	T. R.
Mitchell, Hon. Wm	Nantucket...	Nantucket...	41 17	70 06	30	A.
Rodman, Samuel.......	New Bedford.	Bristol	41 39	70 56	90	A.
Prentiss, Dr. Henry C...	Worcester ...	Worcester ...	42 16	71 48	537	A.
Snell, Prof. E. S.	Amherst.....	Hampshire ..	42 22	72 34	267	A.
Tirrell, N. Q , M. D....	Weymouth...	Norfolk......	42 10	71 00	150	B.T.R.
Whitcomb, L. F........	Florida	Berkshire....	42 40	73 10	2,000	N.

MICHIGAN.

Abbe, Cleveland	Lansing	Ingham	42 44	80 00	850	N.
Allen, James, jr.......	Port Huron...	St. Clair.....	42 58	82 24	606	T.
Blaker, Dr. G. H., jr.. ..	Marquette. ..	Marquette ...	46 32	87 41	630	A.
Bowlsby, Geo. W	Monroe	Monroe	41 56	83 30	584	B.T.R.
Campbell, Wm. M., M.D.	Battle Creek .	Calhoun......	42 20	85 10	825	B.T.R.
Coffin, Matthew........	Otsego	Allegan	42 28	85 42	662	N.
Crosby, J. B...........	New Buffalo..	Berrien	41 45	86 46	661	B.T.R.
Holmes, J. C	Lansing	Ingham	42 44	84 15	750	B.T.R.
Pitcher, Dr. Zena.... ⎫ Horton, L. S ...... ⎭	Detroit.......	Wayne.	42 24	82 58	597	A.
Streng, L. H	Grand Rapids.	Kent	43 00	86 00	680	T. R.
Webb, Miss G........	Ypsilanti	Washtenaw ..	41 45	86 20	600	N.
Whelpley, Miss H. I....	Monroe.......	Monroe	41 56	83 23	590	T. R.
Woodard, C. S.........	Ypsilanti	Washtenaw. .	42 15	83 47	751	A.

MINNESOTA.

Clark, Thomas.........	Beaver Bay..	Lake.........	47 12	91 19	657	A.
Garrison, O. E.........	Princeton . ..	Benton......	45 50	93 45	....	T. R.
Hibbord, A. A.	Burlington ..	Lake.........	47 01	92 30	645	T. R.
Riggs, Rev. S. R.	Pajutazee....	Brown	45 00	94 00	....	T. R.
Smith, A. C.	Forest City...	Muker	45 45	96 00	....	T. R.
Thickstun, T. F........	Chatfield	Fillmore	....	....	°325	T. R.
Wieland, Henry	Beaver Bay ..	Lake.........	47 11	91 25	850	T. P.

* Above La Crosse.

MISSISSIPPI.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
			° ′	° ′	*Feet.*	
Cribbs, J. R.	Westville....	Simpson.....	32 00	90 00	....	T.
Johnston, Wm. M., M. D.	Hernando ...	De Soto	34 45	90 15	*70	A.
Lull, James S............	Columbus ...	Lowndes	33 30	88 29	227	A.
McCary, Robert.........	Natchez.....	Adams	31 34	91 25	264	B. T. R.
Moore, Prof. Albert.....	Grenada.....	Yalobusha...	33 45	90 00	....	N.
Robinson, Rev. E. S. - {	Paulding .. Prairie Line }	Jasper	32 20	89 20	....	A.

MISSOURI.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
Bailey, S. S..............	Dundee	Franklin	38 30	91 10	536	T.
Bowles, S. B., M. D.....	Greenfield ...	Dade.........	37 22	93 41	1,800	N.
Byrns, Robert, M. D. ...	Kirksville ...	Adair	....	....	....	N.
Campbell, John.........	Carrollton ...	Carroll	39 30	93 31	....	N.
Christian, John.........	Harrisonville.	Cass	....	....	....	N.
Conkling, Thos. J.	Trenton......	Grundy	40 13	97 00	....	N.
Dalton, O. D...........	Greenville ...	Wayne	....	....	....	N.
Dodson, B. D.	Toronto	Camden	37 54	92 30	....	N.
Englemann, George	St. Louis	St. Louis	38 37	90 16	481	A.
Fendler, Augustus.......	St. Louis	St. Louis	38 37	90 16	470	T.
Finley, R. W.	Richmond ...	Ray	39 16	94 30	....	N.
Force, Nathan P........	Farmington..	St. Francis...	37 48	91 25	....	N.
Hamaker, Marion F.....	Warrenton...	Warren	38 45	91 11	....	T.
Hanan, B. P............	Luray.......	Clark	40 28	91 57	....	R.
Heaston, D. J...........	Bethany.....	Harrison	40 15	94 00	....	N.
Horner, W. H.	Hornersville .	Dunklin.....	36 03	90 00	....	N.
Huffaker, S. J..........	Carrollton ...	Carroll	39 30	93 31	....	N.
Kiger, W. B.	Emerson	Marion	40 00	92 00	....	N.
Lingow, B. G.	Waynesville .	Pulaski	37 45	92 13	....	N.
Lumpkin, Wm. M.	Tuscumbia...	Miller.......	38 30	91 59	600	N.
Mallinckroot, Conrad...	Augustus	St. Charles...	38 30	90 46	780	T.
Maxey, W. F.	Paris........	Monroe	39 30	92 00	700	T.
Myers, J. H.	Kirksville ...	Adair	40 38	92 50	1,000	N.
Sutherland, Norris	Boonville....	Cooper	38 55	92 30	....	N.
Tidswell, Mary Alice ...	Warrenton...	Warren	38 37	91 16	825	T.
Vankirk, W. J.	Bolivar	Polk.........	37 29	92 45	... .	N.
Vogel, Chas	Rhineland ...	Montgomery .	38 42	91 41	300	T. R.
Weatherford, John M. ..	Lancaster....	Schuyler.....	40 30	92 45	....	N.
Weber, Philip	Hermann	Gasconade ...	38 40	91 27	598	N.
Wells, Wm.............	Stockton	Cedar	39 36	93 48	800	T. R.
Wilson, Joseph A........	Lexington ...	Lafayette....	39 15	93 45	....	N.
Wyrick, M. L...........	Cassville	Barry	36 41	93 57	3,000	T. R.

* Above low water mark at Memphis.

NEBRASKA.

Name of observer.	Station.	County.	N. latitude.	N. longitude.	Height.	Instruments.
			o ′	o ′	*Feet.*	
Bowen, Anna M. J._____	Elkhorn City_	Douglas_____	41 22	96 12	1,000	T.
Byers, William N _____	Omaha _ ____	Douglas_____	41 15	96 10	1,300	T. R.
Evans, John_____	Fontanelle __	Dodge _____	40 31	96 45	_____	T.
Hamilton, Rev. Wm____	Bellevue ____	Sarpy . _____	41 08	95 50	_____	T. R.
Mason, Edgar E._____	Nebraska City	Otoe _____	40 40	95 44	1,050	T. R.
Miller, Ch. H_____	S. Pass Wagon	Road Exped'n	42 28	108 40	○	B. T.
Rain, John G_____	Omaha _ ____	Douglas_____	41 20	95 57	1,400	T. R.
Smith, Charles B_____	Brownville __	Nemaha_____	40 30	96 00	_____	T.
Twiss, Major Thos. S____	Deer Creek __	P. O. Fort La-ramie.	42 50	105 50	5,000	T. R.
White, Bela_____	Kenosha ____	Cass _____	40 51	95 54	1,050	N.

NEW HAMPSHIRE.

Name of observer.	Station.	County.	N. latitude.	N. longitude.	Height.	Instruments.
Bell, Samuel N_____	Manchester __	Hillsborough_	42 59	71 28	300	B.T.R.
Brown, Branch ____ _ __	Stratford ____	Coos _ _____	44 08	71 34	1,000	T. R.
Chase, Arthur _____	Claremont ___	Sullivan_____	43 22	72 21	535	B.T.R.
Hall, Joseph H_____	Top of Mount Washingt'n	Coos _____	44 15	71 16	6,285	A.
Hoyt, Peter L., M. D.____	Wentworth __	Grafton _____	43 49	72 00	_____	T. R.
Odell, Fletcher _____	Shelburne ___	Coos _____	44 23	71 06	700	B. T.
Smith, Rufus_____	N. Littleton _	Grafton _____	44 20	72 13	_____	N.
Wiggin, Andrew _____	Stratham ____	Rockingham _	43 00	73 35	100	N.

NEW JERSEY.

Name of observer.	Station.	County.	N. latitude.	N. longitude.	Height.	Instruments.
Allen, Edwin_____	New Brunsw'k	Middlesex ___	40 39	75 31	90	N.
Parry, William ____ ___	Cinnaminson.	Burlington __	40 00	75 01	83	N.
Thornton, Miss E. E____	Moorestown _	Burlington___	39 58	74 13	104	B. T. P.
Watson, George_____	Woodstown __	Salem_____	39 39	75 20	30	T. R.
Whitehead, W. A_____	Newark _____	Essex _____	40 45	74 10	35	B.T.R.
Willis, O. R_____	Freehold ____	Monmouth __	40 15	74 21	_____	T.

NEW YORK.

Name of observer.	Station.	County.	N. latitude.	N. longitude.	Height.	Instruments.
Arden, Thomas B_____	Garrison's ___	Putnam. ____	41 23	74 02	180	T. R.
Aubier, John_____	Fordham ____	Westchester__	40 54	73 57	147	A.
Bowman, John_____	Baldwinsville	Onondaga____	43 04	76 41	_____	T.
Brown, Rev. John J____	Dansville ____	Livingston___	42 38	77 44	672	A.
Dayton, E. A_____	Madrid _ ____	St. Lawrence_	44 43	75 33	280	B.T. P.
Denning, William H___	Fishkill Land-ing.	Dutchess_____	41 33	74 18	42	B.T. R.
Dewey, Prof. Chester, Ira C. Clark_____ T. T. Filmer _____	Rochester____	Monroe_____	43 08	77 51	516	B.T. R.

NEW YORK—Continued.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
			° ′	° ′	*Feet.*	
Elleston, Job	Geneva	Ontario				T.
Frost, Col. E. C	Havana	Schuyler	42 30	76 31	1,041	T.
Grush, James W ⎱ Alexander, James M ⎰	Spencertown	Columbia	42 18	73 32	700	A.
Guest, W. E	Ogdensburg	St. Lawrence	44 43	75 37	232	R.
Holmes, E. S	Wilson	Niagara	43 20	78 56	250	T.
House, John C	Waterford	Saratoga	42 47	73 39	70	A.
Howell, R	Nichols	Tioga	42 00	76 32		T.
Ingersoll, J. D.	Ilion	Herkimer	43 00	79 51		N.
Ives, William	Buffalo	Erie	42 50	78 56	600	A.
Julien, Alexis A ⎱ Schauber, H. A ⎰	Schenectady	Schenectady	42 49	73 55	300	T.
Kelsey, Kathalo	Great Valley	Cattaraugus	42 12	78 45		N.
Landon, Anna S	Eden	Erie	42 30	79 11	700	T.
Mackie, Matthew	Clyde	Wayne	43 10	77 10	400	B. T.
Malcom, Wm. S	Oswego	Oswego	43 28	76 30	250	B. T.R.
Matthews, M. M., M. D	Rochester	Monroe	43 08	77 51	525	A.
Morris, Professor O. W	New York	New York	40 43	74 05	25	A.
Paine, H. M., M. D	Clinton	Oneida	43 03	75 18	500	T. P.R.
Potter, C. D., M. D	Adams Centre	Jefferson	43 48	75 52	632	R.
Riker, Walter H	Saratoga	Saratoga	43 06	74 00	306	A.
Sartwell, Dr. H. P	Penn Yan	Yates	42 62	77 11	740	A.
Sias, Professor Solomon	Fort Edward	Washington	43 13	73 42		A.
Spooner, Dr. Stillman	Wampsville	Madison	43 04	75 50	500	T. R.
Sylvester, Dr. E. Ware	Lyons	Wayne				B. T.
Titus, Henry Wm	Bellport	Suffolk	40 44	72 54	15	A.
Tompkins, William	Germantown	Columbia			175	N.
Van Kleek, Rev. R. D	Flatbush	Kings	40 37	74 01	54	B. T.R.
Wadsworth, A. S	East Henrietta	Monroe	43 06	77 51	600	B. T.P.
White, Aaron	Cazenovia	Madison	42 55	75 46	1,260	A.
Yale, Walter D	Houseville	Lewis	43 40	75 32		T. R.
Young, Jude M	West Day	Saratoga	43 20	75 16	1,200	T.
Zaepffel, Joseph	West Morisania.	Westchester	40 53	74 01	190	T.
Zimmerman, Godfrey	Pine Hill	Erie	42 45	79 06	680	N.

NORTH CAROLINA.

Carter, Thomas ⎱ Hamilton, W. H ⎰	Raleigh	Wake	35 40	78 52	317	N.
Kerr, Professor W. C	Davidson College.	Mecklenburg.	35 30	80 54	850	B. T.R.
McDowell, Rev. A	Murfreesboro'	Hertford	36 30	77 01		A.
Moore, Geo. F., M. D	Green Plains	Northampton	36 32	77 45		T. R.
Phillips, Prof. James, D.D.	Chapel Hill	Orange	35 54	79 17		B. T.R.
Westbrook, Samuel W	Greensboro'	Guilford	37 00	80 01	840	N.

OHIO.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
			° ′	° ′	*Feet.*	
Abell, B. F..............	Welshfield ...	Geauga	41 23	81 12	1,205	T. R.
Ammen, J	Ripley	Brown	38 37	83 31	....	B.T. R.
Anthony, Newton......	Mount Union.	Stark	40 54	81 31	....	T.
Atkins, Rev. L S......	Madison	Lake........	41 49	80 10	....	T.
Benner, J. F............	New Lisbon..	Columbiana..	40 45	80 45	961	B.T. R.
Bennett, Sarah E.......	Toledo	Lucas	41 49	83 34	....	T.
Bowen, Wm. F.........	Sharonville ..	Hamilton . ..	39 19	84 30	800	N.
Boyd, S. T.............	Iberia........	Morrow	40 46	82 52	1,160	T. R.
Chase, T...............	Marion	Marion	40 30	83 21	....	N.
Clark, Wm. P..........	Medina......	Medina	41 07	81 47	1,255	A.
Colbrunn, Edward	Cleveland ...	Cuyahoga ...	41 30	81 40	665	T.
Cotton, D. B., M. D....	Portsmouth..	Scioto.......	38 45	82 50	529	B.T. R.
Crane, Geo. W..........	Bethel	Clermont....	39 00	84 00	555	T. R.
Davidson, H. M.........	Freedom	Portage	41 13	81 08	1,100	B.T. R.
Davis, W. E............	Lancaster....	Fairfield	39 41	82 37	1,020	B.T. R.
Dille, Israel	Newark	Licking	40 07	82 21	825	T.
Gamble, J. W.........	Russell's Sta'n	Highland....	39 13	83 36	1,000	N.
Hammitt, John W	College Hill .	Hamilton....	39 19	84 25	800	N.
Hampton, W. C........	Mt. Victory..	Hardin......	40 35	83 36	1,500	N.
Harper, Geo. W.........	Cincinnati ...	Hamilton....	39 06	84 27	500	A.
Haywood, Prof John...	Westerville ..	Franklin	40 04	83 00	....	A.
Hill, F. G..............	Dallasburg ..	Warren	39 30	84 31	800	N.
Hillier, Spencer L......	Breckville ...	Cuyahoga ...	41 15	81 30	800	A.
Huntington, Geo. C....	Kelley's Isl'nd	Erie	41 36	82 42	587	B.T. R.
Hyde, Gustavus A......	Cleveland ...	Cuyahoga ...	41 30	81 40	643	B.T. R.
Ingram, John, M. D....	Savannah....	Ashland.....	41 12	82 31	1,098	A.
Johnson, Enoch D......	....	Monroe	39 30	81 00	540	T. R.
King, Mrs. Ardelia C...	Madison.....	Lake.......	41 52	81 00	620	T. R.
Luther, S. M	Hiram	Portage	41 20	81 08	1,290	T. R.
Mathews, J. McD.. D. D.	Hillsborough	Highland....	39 13	83 30	1,134	A.
McClung, Charles L.....	Troy........	Miami	40 03	84 06	ᴼ404	B.T. R.
McMillan, S. B.........	East Fairfield	Columbiana..	40 47	80 44	1,152	A.
Peck, W. R., M.D......	Bowl'g Green	Wood	41 15	83 40	700	B.T. R.
Peters, Adam..........	Zanesville ...	Muskingum..	....	....	....	T.
Phillips, R. C. and J. H.	Cincinnati ...	Hamilton....	39 06	̔4 27	540	B.T. R.
Raffensperger, E. B.....	Toledo	Lucas	40 30	83 20	....	N.
Rhoades, Dr. John	Hocking Port.	Athens......	39 00	81 30	....	N.
Shaw, Joseph..........	Bellefontaine.	Logan	40 21	83 20	1,040	T. R.
Shields, Rev. Robert, } Smith, John C...... {	Bellecentre ..	Logan	40 30	83 45	1,170	B.T. R.
Treat, Samuel W........	Windham ...	Portage	41 10	81 05	939	T.
Tweedy, David H.......	Mt. Pleasant.	Jefferson	40 20	83 34	....	N.
Ward, Rev. L. F........	Avon	Lorain	41 27	82 04	800	A.
Warder, A. A...........	Cincinnati ...	Hamilton... ..	39 08	84 35	800	T. R.
Williams, Prof. M. G...	Urbana......	Champaign ..	40 06	83 43	1,015	B.T. R.
Wilson, Prof. J. H......	College Hill..	Hamilton....	39 19	84 26	800	B.T. R.
Young, Prof. Chas. A., } Childs, E. W...... {	Hudson	Summit	41 15	81 24	1,137	B T. R.

OREGON.

Stebbins, George H.....	Portland	Multnomah ..	45 24	121 30	170	T. R.

* Above Ohio river at Cincinnati.

PENNSYLVANIA.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
			° ′	° ′	*Feet.*	
Alsop, Samuel.........	Westchester...	Chester	39 57	75 34	550 .	A.
Baird, John H.........	Tarentum. ...	Alleghany....	40 38	79 46	950	T.
Barrett, James.........	Linden......	Lycoming....	41 14	77 11	514	A.
Boyers, W. R.........	Altoona	Blair........	40 30	78 31	1,168	B.T.R.
Brewster, Wm., M. D...	Huntingdon ..	Huntingdon .	40 35	78 03	734	B.T.R.
Brugger, Samuel.......	Fleming.....	Centre........	40 55	77 53	780 .	T. R.
Burrell, J. Ilgen.......	Bellefonte ...	Centre........	40 50	77 49	....	T.
Coffin, Selden J...... } Houghton, George S }	Easton	Northampton.	40 43	75 16	320	A.
Cook, Thos. E. & Sons ..	Bendersville..	Adams	....	....	....	N.
Darlington, Fenelon....	Parkersville..	Chester	39 54	75 37	218	T. R.
Eggert, John..........	Berwick......	Columbia....	41 05	76 15	583	A.
Friel, P..............	Shamokin ...	Northumber'd	40 15	76 30	700	T. R.
Hance, Ebenezer.......	Morrisville...	Bucks.	40 12	74 48	30	B.T.R.
Harvey, J. C..........	Nazareth.. ..	Northampton.	40 43	78 41	530	B.T.R.
Heckerman, Henry.....	Bedford	Bedford	40 01	78 30	....	T. R.
Heisely, Dr. John......	Harrisburg...	Dauphin.....	40 16	76 15	....	B.T.R.
Heyser, William, jr.	Chambersburg	Franklin.....	39 58	77 45	618	A.
Hickok, W. O.........	Harrisburg...	Dauphin	40 20	76 50	320	A.
Hoffer, Dr. Jacob R.....	Mount Joy...	Lancaster....	40 08	76 30	....	A.
Jacobs, Rev. M.........	Gettysburg...	Adams......	39 49	77 15	624	B.T.R.
James, Prof. Charles S...	Lewisburg ...	Union	40 58	76 58	....	A.
Kirkpatrick, Prof. J. A...	Philadelphia .	Philadelphia .	39 57	75 11	50	A.
Kohler, Edward........	Whitehall st'n	Lehigh......	40 40	75 26	250	T.
Meehan, Thomas.......	Germantown.	Philadelphia..	....	....	....	N.
Moore, Mahlon.........	Morrisville...	Bucks	....	....	....	N.
Mowry, George........	Somerset	Somerset	40 00	79 03	2,195	A.
Ralston, Rev. J. Grier....	Norristown...	Montgomery .	40 08	75 19	153	A.
Scott, Samuel..........	Worthington.	Armstrong...	41 50	79 31	1,050	T. R.
Smelley, John H.......	Lima	Delaware	39 55	75 25	226	T.P.R.
Smith, Wm., D. D......	Canonsburg. .	Washington ..	40 17	80 10	936	A.
Speer, Alex. M., M. D...	Pittsburg	Alleghany....	40 32	80 02	850	B.T.R.
Stewart, Thos. H.......	Murrysville ..	Westmorela'd	40 28	79 35	960	A.
Swift, Dr. Paul........	W. Haverford	Delaware	40 00	75 21	400	T. R.
Tracy, James E........	E. Smithfield.	Bradford......	41 58	76 37	1,000	N.
Travelli, John I........	Sewickleyville	Alleghany....	40 38	80 14	....	B.T.R.
Wilson, Prof. W. C.....	Carlisle	Cumberland .	40 12	77 11	500	A.

RHODE ISLAND.

Caswell, Prof. A........	Providence...	Providence ..	41 49	71 25	120	A.

SOUTH CAROLINA.

Name of observer.	Station.	County.	N. latitude.	W. longitude	Height.	Instruments.
			° ′	° ′	*Feet.*	
Barton, E. H., M. D....	Columbia....	Richland.....	33 57	81 07	233	A.
Cornish, Rev. John H...	Aiken........	Barnwell.....	33 32	81 34	565	T. R.
Glennie, Rev. Alex'r....	Georgetown...	All Saints....	33 29	79 17	20	A.
Johnson, Joseph, M. D } Dawson, J. L., M. D }	Charleston...	Charleston. ..	32 46	80 00	20	B.T.R.
Ravenel, Thomas P.....	Black Oak.. ..	Charleston....	33 00	80 00	50	A.
Superintendent Arsenal Academy.	Columbia....	Richland.....	34 00	81 07	295	A.

TENNESSEE.

Barney, Chas. R.........	University Pl.	Franklin.....	35 12	86 00	2,000	T. R.
Blake, J. R.............	La Grange...	Fayette				B.T.R.
Dodge, J. W. & Son....	Pomona......	Cumberland .	36 00	85 00	2,200	T.
Houghton. S. W........	Winchester ..	Franklin.....	35 10	86 11		T. R.
Stewart, Prof. Wm. M..	Clarkesville. .	Montgomery .	36 28	87 13	481	A.
Tuck, W. J., M. D.... } Mitchell, R. W., M. D }	Memphis.. ...	Shelby..	35 08	90 00	262	A.

TEXAS.

Allis, Melvin H........	Gonzales	Gonzales	29 35	97 30		N.
Colman, William.	Texana......	Jackson	29 00	96 30	60	N.
Crockett, Jno. M	Dallas	Dallas	32 45	96 46		T.
Cunningham, J. D......	Bastrop	Bastrop......				N.
De Graffenried, W. G., M. D.	Columbus ...	Colorado	29 43	96 36	198	N.
DeJernett, R., M. D	Greenville...	Hunt	33 10	97 23		N.
D'Spain, Dr. B. L .... } Ewing, J. M......... }	Tarrant	Hopkins.....	33 30	96 41		N.
Epperson, W. T........	Jefferson	Cass...	32 30	94 30	65	N.
Freese, G	Boston	Bowie.......	33 25	94 40	600	N.
Friedrich, Otto	New Braun-fels.	Comal	29 41	98 15		B. T. R.
Gaffney, James O	San Patricio .	San Patricio .	27 45	98 31		R.
Gantt, Dr. Wm. H......	Union	Washington .	30 11	96 31	540	T. R.
Gardiner, Jas..........	Port La Vaca.	Calhoun.....	28 38	96 37	25	N.
Gibbs, T..............	Huntsville...	Walker......				R.
Glasco, J. M	Gilmer......	Upshur......	32 46	94 49	1,017	T.
Kapp, Ernst...........	Sisterdale....	Blanco	29 54	98 35	1,000	A.
Kellog, F	Wheelock ...	Robertson ...	30 50	96 30	450	R.
Moke, Dr. James E.....	Woodboro' ..	Grayson.....	33 47	96 36		N.
Palm, Swante..........	Austin	Travis	30 15	97 47		A.
Rayal, Jas. T	Kaufman	Kaufman	32 30	96 00		N.
Rucker, B. H	Washington .	Washington .	30 26	96 15		B. T. R.
Schuman, Bruno.......	Roundtop ...	Fayette	30 06	96 37		T. R.
Sias, Prof. Solomon	Bonham	Fannin......	33 40	96 13	435	A.

TEXAS—Continued.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instrument.
			° ′	° ′	*Feet.*	
Turner, T. A	Springfield	Limestone	31 30	96 15	4,500	N.
Van Nostrand, J	Austin	Travis	30 20	97 46	650	T. P. R.
Wade, F. S	Cross Roads	Williamson	30 29	97 26	672	T. R.
West, Dr. N. P	Burkeville	Newton	31 00	93 31		T.
Yellowby, Prof. C. W	Webberville	Travis	30 10	97 31		B. T. R.
Yoakum, F. L	Larissa	Cherokee	31 45	95 20		T. P.R.

UTAH.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instrument.
Phelps, W. W	Great Salt Lake City.	Salt Lake	40 45	111 26	4,250	A.

VERMONT.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instrument.
Buckland, David	Brandon	Rutland	43 45	73 00		T. R.
Cutting, Hiram A	Lunenburg	Essex	44 28	71 41	1,124	A.
Fairbanks, Franklin	St. Johnsbury	Caledonia	44 25	72 00	540	B.T.R.
Jackman, Prof. A	Norwich	Winds or	43 42	72 21		B.
Paddock, James A	Craftsbury	Orleans	44 40	72 29	1,100	T. R.
Parker, Joseph	West Rupert	Bennington	43 15	73 11	750	T.
Petty, McK.	Burlington	Chittenden	44 27	73 10	367	A.

VIRGINIA.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instrument.
Abell, J. Ralls	Charlottesv'le	Albemarle	38 00	78 31	521	T. R.
Astrop, Col. R. F	Crichton's Store.	Brunswick	36 40	77 46	500	T. R.
Poyers, Wm. R	Buffalo	Putnam	38 30	81 57	480	B.T. R.
Dickinson, George C	Cobham Depot	Albemarle	38 05	78 21	450	T. R.
Ellis, D. H	Wardensville	Hardy	39 30	78 03	1,720	B.T. R.
Fraser, James	New England	Wood	39 20	81 00		N.
Hotchkiss, Jed	Stribling Springs.	Augusta	38 20	79 05	1,600	T.
Jones, Silas B	Fork Union	Fluvanna	37 40	78 21		N.
Kendall, James E	Kanawha C. H	Kanawha	38 20	81 30	720	T. R.
Lockwood, George P	Wheeling	Ohio	41 09	80 46		T. R.
Mackie, Rev. C. B	Anna	Fairfax	38 56	77 04	180	T. R.
Meriwether, Chas. I	Richmond	Henrico				T. R.
Marvin, John W	Winchester	Frederick	39 15	78 10		T. R.
Pickett, John	The Plains	Fauquier	38 50	77 51		T.
Purdie, John R., M. D	Smithfield	Isle of Wight.	37 02	76 37	100	T. R.
Reynolds, W. C	Kanawha Salines.	Kanawha	38 30	81 30		T.P. R.
Robey, Chas. H	Fredericksb'g	Spottsylvania	38 30	77 30	600	N.
Ruffin, Julian C	Garysville	Prince George	37 21	77 34	100	B.T. R.

VIRGINIA—Continued.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
			° ′	° ′	Feet.	
Sanders, B. D.	Wellsburg	Brook				T. R.
Slaven, James	Meadow Dale.	Highland	38 23	79 36	1,800	T. R.
Spence, Edward E.	Montross	Westmoreland	38 07	76 46	200	T. P.R.
Stalnaker, J. W., M. D.	Lewisburg	Greenbrier	37 49	80 28	2,000	T. R.
Upshaw, George W.	Lloyd	Essex				T. R.
Van Doren, Abram	Falmouth	Stafford	38 15	77 34	350	T. R.
Webster, Prof. N. B.	Portsmouth	Norfolk	36 50	76 19	12	B.T. R.

WISCONSIN.

Name of observer.	Station.	County.	N. latitude.	W. longitude.	Height.	Instruments.
Armstrong, S.	Pardeeville	Columbia	43 44	89 16		T.
Atwood, Isaac	Lake Mills	Jefferson	43 00	89 00		N.
Bean, Professor S. A.	Waukeska	Waukesha	42 50	88 11	833	B.T.P.
Clarke, Prof. Ambrose W.	Delafield	Waukesha	43 20	88 31	900	B. T.
Curtis, W. W.	Rocky Run	Columbia	43 26	89 20		T. R.
Doyle, L. H.	Otsego	Columbia	43 30			N.
Ellis, Edwin, M. D.	Whittlesey.	La Pointe	46 33	91 00	658	T. R.
Gordon, W. A., M. D.	Wausau	Marathon	45 00	89 30		T. R.
Gridley, Rev. John	Kenosha	Kenosha	42 35	87 50	600	B.T. R.
Haeuser, Emil.	Black River Falls.	Jackson	44 17	90 30		T.
Johnson, A. K.	Platteville	Grant	45 00	90 00		T. R.
Kellogg, George J.	Janesville	Rock	42 43	89 90	780	T.
Lapham, Increase A.	Milwaukie	Milwaukie	43 03	87 54	593	B.T. P.
Larkin, Prof. E. P.	Milwaukie	Milwaukie	43 02	87 55	684	B.T.
Lüps, Jacob	Manitowoc	Manitowoc	44 07	87 45	* 80	B. T.
Mann, Wm	Superior	Douglass	46 46	92 03	680	T. R.
Mason, Prof. R. Z	Appleton	Outagamie	44 10	88 35	800	A.
Matthews, D.	Burlington	Racine	42 39		700	N.
Nourse, J. Harvey	Bayfield	La Pointe	47 00	91 00		T. R.
Pashley, J. S., M. D.	Mosinee	Marathon	44 44	89 35		T.
Pickard, J. L., M. D.	Platteville	Grant	42 45	90 00		T. R.
Pomeroy, F. C.	Milwaukie	Milwaukie	43 03	87 57	658	T.
Porter, Prof. Wm	Beloit	Rock	42 30	89 04	750	B.T.R.
Sterling, Prof. J. W.	Madison	Dane	43 05	89 25	1,068	A.
Underwood, Col. D.	Green Bay	Brown	44 30	88 00	584	T. R.
Winkler, C., M. D.	Milwaukie	Milwaukie	43 03	87 57	600	B.T.R.

* Above Lake Michigan.

MEXICO.

Name of observer.	Station.	Latitude.	Longitude.	Height.	Instruments.
		° ′	° ′	*Feet.*	
Berendt, German, M. D.	Vera Cruz, Vera Cruz.......	19 12	96 09	26	A.
Hieto, J. A°..........	Cordova, Vera Cruz........	18 54	--------	2,820	B. T. R.
Laszlo, Charles...... {	Minititlan, Tehuantepec	17 59	94 07	60	A.
	Chinamaca, Tehuantepec....	18 02	94 15	100	A.
Sartorius, Charles°....	Mirador, Vera Cruz..........	19 15	96 25	3,600	A.

CENTRAL AMERICA.

Casudas, Antonio	Guatemala, Guatemala.......	14 37	90 30	....	A.

WEST INDIES.

Hayne, J. B..........	Turk's Island	....	....	....	B. T.

BERMUDA.

Royal Engineers, (in the Royal Gazette.)	Centre Signal Station, St. George's.	....	....	....	A.

SOUTH AMERICA.

Dorsey, Edward B.....	Chanarcillo, Chile...........	27 28S	70 28	3,860	B. T.
Hering, C. T..........	Plantation Catharina Sophia, Colony of Surinam, Dutch Guiana.	5 48	56 47	....	A.

ASIA.

Barclay, R. G., M. D..	Jerusalem, Palestine	31 47	35 13E	2,610	A.

* Sent observations also for 1858.

METEOROLOGICAL RECORDS RECEIVED FROM PERSONS NOT REGULAR OBSERVERS.

From ANDREW SCOTT.—Copy of journal kept at the public library, Nassau, N. P., Bahamas, lat. 25° 05', long. 77° 21', by A. M. SMITH, Librarian, January, 1858, to August, 1859, observations of barometer, thermometer, and wind.

From Dr. EDWARD P. VOLLUM, U. S. A.—Twenty-four slips ozone paper, kept at Fort Umpqua, Oregon, from June 12 to August 5, 1859.

From Captain A. W. WHIPPLE, U. S. A.—Observations of barometer, thermometer, psychrometer, rain, clouds, and winds, from May 16 to September 30, 1859, made at Lake George, Chippewa county, Michigan. The observations for May were taken by J. H. FOSTER; for July, August, and September by EDWARD PERRAULT.

From A. MATTISON, Paducah, Ky.—Observations made at Bogota, South America, on barometer, thermometer, and rain, by EZEQUIEL URICOECHEA, for June, 1857, and March, April, and May, 1859. Printed slips.

From Lieutenant JOHN MULLAN, U. S. A., in charge of the military wagon road expedition from Fort Walla-Walla to Fort Benton. Observations made on barometer and thermometer at Cantonment Jordan, in the valley of the St. Francis Borgia river, for December, 1859, by W. W. JOHNSON.

From B. F. SHUMARD, Austin, Texas.—Observations on barometer, thermometer, winds, and clouds, for the year 1859, made on the geological and agricultural survey of Texas, under his direction, by GEORGE G. SHUMARD, M. D.

From JOHN H. CLARK, commissioner, United States and Texas boundary survey.—Barometric record from January 15 to September 21, 1859; the record not continuous, but made from time to time at the various astronomical stations. Received through the Interior Department.

From S. T. ANGIER, of Columbia, Texas.—Desultory notes of weather made in Sussex county, Virginia, during the year 1820.

From Captain JOHN POPE, U. S. A.—Observations with full set of instruments on expedition in Texas and New Mexico, made by JAMES M. READE, from March, 1855, to December, 1857.

Stations from which Telegraphic Reports of the weather were received at the Smithsonian Institution in the year 1859.

New York, N. Y.	Wytheville, Va.	Charleston, S. C.
Philadelphia, Pa.	Grafton, Va.	Augusta, Ga.
Pittsburg, Pa.	Wheeling, Va.	Savannah, Ga.
Baltimore, Md.	Parkersburg, Va.	Macon, Ga.
Frederick, Md.	Marietta, Ohio.	Columbus, Ga.
Hagerstown, Md.	Chillicothe, Ohio.	Atlanta, Ga.
Cumberland, Md.	Cincinnati, Ohio.	Prairie Bluff, Ala.
Richmond, Va.	Bristol, Tenn.	Montgomery, Ala.
Petersburg, Va.	Knoxville, Tenn.	Lower Peach Tree, Ala.
Norfolk, Va.	Chatanooga, Tenn.	Mobile, Ala.
Staunton, Va.	Raleigh, N. C.	Gainesville, Miss.
Charlottesville, Va.	Wilmington, N. C.	Jackson, Miss.
Lynchburgh, Va.	Columbia, S. C.	New Orleans, La.

DEATHS OF OBSERVERS.

SAMUEL BROWN, Bedford, Pa., died in end of 1858 or beginning of 1859. He commenced observing before 1854. The last register received from him was for October, 1858.

JOHN LEFFERTS, observer at Farmer, Seneca county, N. Y., died on the 14th of January, 1859.

WILLIAM CRANCH BOND, an eminent American astronomer and director of the Cambridge observatory, died on the 29th of January, 1859, in the 69th year of his age.

Dr. J. W. TUCK, secretary of the Board of Health, Memphis, Tenn., died about the first of June, 1859. Observer since 1857.

Dr. E. H. BARTON, formerly of New Orleans, died at Charleston, S. C., in September, 1859.

Dr. JOHN JAMES, Upper Alton, Illinois, observer since 1854, died October 11, 1859.

Dr. JOHN F. POSEY, Savannah, Ga., died in January, 1860. Observer since previous to 1854.

REPORT OF THE EXECUTIVE COMMITTEE.

The Executive Committee respectfully submit to the Board of Regents the following report of the receipts and expenditures of the Smithsonian Institution during the year 1859, with estimates for the year 1860:

Receipts.

The whole amount of Smithson's bequest deposited in the treasury of the United States is $515,169, from which an annual income, at six per cent., is derived, of		$30,910 14
Extra fund of unexpended income, invested as follows:		
In $75,000 Indiana 5 per cent. bonds, yielding	$3,750 00	
In $53,500 Virginia 6 per cent. bonds, yielding	3,210 00	
In $7,000 Tennessee 6 per cent. bonds, yielding	420 00	
In $500 Georgia 6 per cent. bonds, yielding	30 00	
In $100 Washington 6 per cent. bonds, yielding	6 00	
		7,416 00
		38,326 14
Balance in the hands of the Treasurer January 1, 1859		16,141 36
		54,467 50

Expenditures.

For building, furniture, and fixtures	$1,720 57	
For items common to the different objects of the Institution	11,519 04	
For publications, researches, and lectures	11,072 32	
For library, museum, and gallery of art	10,521 46	
		34,833 39
Balance in the hands of the Treasurer January 1, 1860, including $5,000 of the *extra fund* not yet invested		19,634 11

7

Statement in detail of the expenditures in 1859.

BUILDING, FURNITURE, AND FIXTURES.

Repairs and incidentals	$1,054 28	
Furniture and fixtures in common	274 28	
Furniture and fixtures for museum	135 21	
Magnetic observatory	256 80	
		$1,720 57

GENERAL EXPENSES.

Meetings of the Board	$14 50	
Lighting and heating	902 68	
Postage	558 65	
Transportation and exchange	1,458 34	
Stationery	289 37	
General printing	569 60	
Apparatus	766 13	
Laboratory	68 36	
Incidentals general	570 06	
Salaries—Secretary	3,500 00	
Chief clerk	1,400 00	
Book-keeper, janitor, &c	1,064 00	
Extra clerk hire	357 35	
		11,519 04

PUBLICATIONS, RESEARCHES, AND LECTURES.

Smithsonian Contributions	3,964 15	
Reports on progress of knowledge	1,831 19	
Other publications	686 37	
Meteorology	3,247 36	
Investigations, computations, and researches	464 05	
Lectures	879 20	
		11,072 32

LIBRARY, MUSEUM, AND GALLERY OF ART.

Library—Cost of books	2,530 83	
Pay of assistants	1,347 00	
Transportation	217 74	
Incidentals	25 00	
Museum—Salary of Assistant Secretary	2,000 00	
Explorations	315 65	
Collections	16 87	
Incidentals, jars, alcohol, &c	1,115 65	
Assistants and labor	2,378 38	
Transportation	544 34	
Gallery of Art	30 00	
		10,521 46
Total expenditure		34,833 39

The estimated income for the year 1859, inclusive of the balance in the hands of the Treasurer, was $54,495 56, and the actual income $54,467 50; showing a difference of $28 06 less than the estimate. This difference arises from the fact that in the previous reports the whole amount of $3,210 interest on Virginia stock was assumed to have been placed to the credit of the Treasurer, although Riggs & Co. retained $16 06 for commission, &c., at the time of the purchase.

In addition to the above, the interest for two years on the Washington corporation stock, amounting to $12, was considered as in the hands of the treasurer, although it had not actually been drawn.

The estimated expenditure was $38,000. The actual expenditure $34,833 39; showing a difference of $3,166 61 less than the estimate, due principally to a less expenditure on the building, furniture, and the publications.

The amount of income above that of the expenditure was $3,492 75, which, added to the actual balance ($16,141 36) in the hands of the Treasurer at the beginning of the year 1859, makes $19,634 11. It is necessary to mention that $5,000 of this belongs to the extra fund, which has not yet been invested.

The annual appropriation from Congress for keeping the museum of the exploring expedition has been expended, under the direction of the Secretary of the Interior, in assisting to pay the extra expenses of assistants and the cost of preserving and arranging the specimens.

An appropriation has also been continued during the past year by the Patent Office for the collection of meteorological statistics for the Agricultural Report.

It is believed the expenditures under these heads have been economically and judiciously made, and that the services rendered to government have been strictly and faithfully performed.

The specimens intrusted to the care of the Institution are now undergoing a thorough examination, and, being scientifically arranged, are in a better condition to meet the wants of the naturalist, and to interest the public, than ever before.

The committee respectfully submit the following estimate of the expenditures for the year 1860:

Estimate of appropriations for the year 1860.

BUILDING, FURNITURE, AND FIXTURES.

Repairs and incidentals	$1,500 00	
Furniture and fixtures	800 00	
Magnetic observatory	350 00	
		$2,650 00

GENERAL EXPENSES.

Meetings of the Board	250 00
Lighting and heating	1,000 00

Postage	$600 00
Transportation and exchange	1,500 00
Stationery	300 00
General printing	600 00
Apparatus	800 00
Laboratory	100 00
Incidentals general	500 00
Salaries.—Secretary	3,500 00
Chief clerk, messenger, book-keeper, laborers, &c	3,000 00
Extra clerk hire	500 00
	$12,650 00

PUBLICATIONS, RESEARCHES, AND LECTURES.

Smithsonian Contributions	6,000 00
Reports on progress	500 00
Miscellaneous collections	1,000 00
Meteorology	3,000 00
Investigations	700 00
Lectures	800 00
	12,000 00

LIBRARY, MUSEUM, AND GALLERY OF ART.

Library.—Cost of books	2,500 00
Pay of assistants	1,500 00
Transportation	250 00
Incidentals	50 00
Museum.—Salary of Assistant Secretary	2,000 00
Explorations	300 00
Collections	200 00
Incidentals	1,000 00
Assistants and labor	2,000 00
Transportation	600 00
Gallery of Art	300 00
	10,700 00
	38,000 00

The committee have carefully examined all the books and accounts of the Institution for the past year, and find them to be correct.
Respectfully submitted.

<div align="right">

J. A. PEARCE,
A. D. BACHE,
*Executive Committee.**

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WASHINGTON.

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* General Totten, the other member of the Executive Committee, is temporarily absent in California on official duty.

JOURNAL OF PROCEEDINGS.

OF THE

BOARD OF REGENTS

OF

THE SMITHSONIAN INSTITUTION.

WASHINGTON, *January* 18, 1860.

In accordance with a resolution of the Board of Regents of the Smithsonian Institution fixing the time of the beginning of their annual session on the third Wednesday of January of each year, the Board met this day in the Regents' room of the Institution.

Present : Hon. Jas. A. Pearce, Hon. W. H. English, and the Secretary.

No quorum being present, the Board adjourned to meet on the 28th of January.

JANUARY 28, 1860.

The Board of Regents met this day, at 10½ o'clock a. m., in the Regents' room.

Present: Hon. Mr. Breckinridge, Hon. James A. Pearce, Hon. J. M. Mason, Professor C. C. Felton, Professor A. D. Bache, Hon. J. G. Berret, W. W. Seaton, esq., Treasurer, and the Secretary.

The Secretary announced the reappointment, by the Vice-President, under a resolution of the Senate, of the Hon. S. A. Douglas, as a Regent for the term of six years, and stated that the House of Representatives not having organized, the vacancies in the Board from that body had not been filled. He regretted to state that the Chancellor of the Institution, Chief Justice Taney, was confined to his bed by temporary illness; that Mr. Hawley, of Albany, was unable to attend on account of bad health, and that since the last meeting of the Regents a vacancy had occurred in the Board by the death of the Hon. Richard Rush, of Philadelphia.

Hon. Mr. Pearce then made the following remarks:

Since the last meeting of the Board of Regents, as announced by the Secretary, one of its earliest and most distinguished members, the Hon. RICHARD RUSH, has departed this life.

The history of his public career is familiar to all the Regents, to

whom I need scarcely detail even its more prominent incidents; but I may remark that it is seldom the good fortune of any man to fill so many important offices, and to execute so many responsible public trusts, not only with credit, honor, and usefulness, but with ever-increasing reputation. Mr. Rush's life was a long one, and he entered into the service of his country while yet in the spring of manhood. He was Comptroller of the Treasury at a time when the fiscal affairs of the government were in disorder, when the public accounts were numerous and complicated, and often required difficult legal adjustment. He was next Attorney General. Soon after the peace of 1815 he was minister to England, and occupied that important post during eight years, when various national questions of difficulty and delicacy required for their proper settlement diplomatic skill, firmness, and caution. He was Secretary of the Treasury when measures of revenue were violently disputed; minister to France when the monarchy was a second time overthrown and a republic again proclaimed. To these great and varied employments he brought integrity, ability, intelligence, firmness, courtesy, and a directness of purpose which scorned all finesse, and which served his country to the full extent of all that could have been demanded or hoped. He was a good scholar, having graduated at Princeton College, and cultivated literature, as well as the severer studies of his profession, with great zeal and success.

Withal he was remarkable for the kindness of his temper, the amenity of his manners, and the charms of his conversation.

With this establishment he had the earliest connexion, having, under the authority of the government, caused the institution of legal proceedings in England for the recovery of the fund with which it was founded and endowed, and superintended their progress to the close.

The act of Congress of 1846 having established the Smithsonian Institution, he was appointed one of its first Regents, and was constantly continued by Congress a member of their Board. His zeal for the increase and diffusion of knowledge among men, and his sound judgment, contributed to the adoption of the system of operations which, so far, has borne the happiest fruits; and his interest in and care for its successful management furnished one of the enjoyments of a tranquil old age, "attended by reverence and troops of friends."

I offer the following resolutions:

Resolved. That the Board of Regents have learned with deep regret the death of the Hon. Richard Rush, one of their members, whose long and distinguished career of public usefulness commanded their entire respect, and whose moral and social worth won their highest esteem and regard.

Resolved, That a copy of this resolution be transmitted to the family of the deceased.

The resolutions were unanimously adopted.

On motion of Mr. Mason, it was ordered that a copy of the remarks of Mr. Pearce be included in the proceedings, and also transmitted to the family.

The Treasurer presented the account of receipts and expenditures

for the year 1859, and a general statement of the finances, which were read and referred to the Executive Committee.

The Secretary read the following letter from the Duke of Northumberland, and presented the books to which it refers:

NORTHUMBERLAND HOUSE, *July* 4, 1859.

SIR: Permit me to present to the Smithsonian Institution some books which I have had privately printed as materials for the history of the county of Northumberland. There is a survey of the Roman wall which was built across the north of England; coins of the Roman families, some of which were found in this country; and an account of some ancient castles which have historical interest.

I again beg to express my thanks to the members of the Smithsonian Institution for the valuable publications which they have had the kindness to send me.

I am, sir, your obedient servant,

NORTHUMBERLAND.

The Secretary exhibited a burning lens and a condensing air-pump, which had been presented to the Institution by J. R. Priestley, esq., of Northumberland, Pa., a grandson of the celebrated Dr. Priestley, and made the following remarks:

This lens is undoubtedly connected with the history of one of the most important chemical discoveries of the latter part of the last century. Dr. Priestley, who has been styled the father of pneumatic chemistry, made a series of experiments on different kinds of air, which greatly extended the science of chemistry, and has been of material importance in the improvement of various practical arts.

"At the time of my first publication," [says Dr. Priestley,]* "I was not possessed of a burning lens of any considerable force, and for want of one I could not possibly make many of the experiments which I had projected, and which in theory appeared very promising. But having afterwards procured a lens of *twelve* inches diameter and *twenty* inches focal distance, I proceeded with great alacrity to examine by the help of it what kind of air a great variety of substances, natural and factitious, would yield, putting them into glass vessels, which I filled with quicksilver, and kept them inverted in a basin of the same. With this apparatus, after a variety of other experiments, on the 1st of August, 1774, I endeavored to extract air from *mercurius calcinatus per se*, and I presently found that by means of this lens air was expelled from it very readily. Having got three or four times as much [air] as the bulk of my materials, I admitted water to it, and found that it was not imbibed by it. But what surprised me more than I can well express was that a candle burned in this air with a remarkably vigorous flame."

The gas thus discovered, to which he gave the name of "dephlogisticated air," was what is now known as OXYGEN.

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* Experiments and observations on different kinds of air, &c., by Jos. Priestly : vol. ii, pp. 106–112. Birmingham, 1790.

Dr. Priestley, however, though he made a large number of experiments in regard to it, remained in ignorance of its true nature until March, 1775; but in the course of this month, says he, "I not only ascertained the nature of this kind of air, though very gradually, but was led by it, as I then thought, to the complete discovery of the constitution of the air we breathe."

That the lens now exhibited to the Board is the one with which this important discovery was made cannot be doubted, since, according to the statement of his grandson, it has never been out of the family—is *twelve* inches diameter, and has a focal length of precisely *twenty* inches.

The annual report of the operations and condition of the Institution was presented by the Secretary, and read in part.

On motion of Mr. Pearce, the Board then adjourned to meet on Saturday next, at 10 o'clock.

SATURDAY, *February* 4, 1860.

A meeting of the Board of Regents was held this day, at 10 o'clock a.m.

Present: Hon. John C. Breckinridge, Hon. James A. Pearce, Hon. S. A. Douglas, Professor C. C. Felton, Professor A. D. Bache, Hon. J. G. Berret, and the Secretary.

Mr. Breckinridge was called to the chair.

The minutes were read and approved.

The Secretary announced the death of the following persons who had been connected officially and otherwise with the operations of the Institution: Washington Irving, an honorary member; Professor Parker Cleaveland, also an honorary member; Professor W. W. Turner, Professor James P. Espy, and G. Würdemann, esq.

Professor Felton then addressed the Board as follows:

Mr. CHANCELLOR: The year 1859 will be memorable in the history of civilization for the number of illustrious men who have passed away from the scene of their earthly labor in its course. The year 1769 was remarkable for the number of men born in it, who have changed the whole aspect of science and letters and the political condition of the world. Of the great men born in that year, one, Humboldt, the most eminent of all, lived to the year 1859, thus spanning over the interval between them by a life of 90 years consecrated to the highest objects of human pursuits.

The Smithsonian Institution has to lament an unusual number of those connected with it among the distinguished dead of the past year. The venerable Mr. Rush has already been fitly commemorated by a member of the Board. I take the liberty of offering a few remarks upon two others whose death the country deplores.

Professor W. W. Turner was born in England in 1810. At the age of five years he was brought by his father to the United States. The fortunes of his family being humble, he learned the trade of a carpen-

ter; but at the age of nineteen he became a printer. During his
youth and early manhood he exhibited an ardent love of knowledge,
and devoted every moment he could spare from the necessary labors
of his trade to its acquisition. His taste led him especially to the
study of philology, and his acquisitions in this department of knowl-
edge were surprising. He studied not only the ancient languages,
including the Hebrew, Chaldee, Syriac, Samaritan, Coptic and San-
scrit, but the modern European and Oriental tongues. To these rich
and varied accomplishments he added an extensive knowledge of the
dialects of the American aborigines, which form a group so peculiar
in their characteristics, and so important in their bearings upon com-
parative philology. But Mr. Turner possessed not merely the talent
of learning languages. His mind was of a philosophical cast; he
mastered easily and rapidly the general principles of the science of
comparative philology, which has become within the present age one
of the surest guides in tracing the history and affinities of the different
branches of the human race. This science but few men of his age
have so thoroughly explored as our departed friend.

In 1842 Mr. Turner was elected professor of Oriental literature in
the Union Theological Seminary of the city of New York. The duties
of this office he discharged with signal ability for ten years. In 1852
the Commissioner of Patents invited him to Washington to take charge
of the library in that department. His labors in forming a library
for the special use of the department and adequate to its wants have
been highly appreciated by those who knew them best.

His literary activity has been various and effective. He assisted
the learned Dr. Nordheimer in the preparation of his Hebrew gram-
mar. He executed the greater part of the translation of Freund's
Latin Lexicon from the German for the American edition. He wrote
many valuable papers for the "Bibliotheca Sacra" and other kindred
periodical publications. A few years ago an inscription was found
near the ancient Sidon, cut on the lid of the sarcophagus of an ancient
king of that city, and copies of it having been transmitted to this
country by the American missionaries, it attracted the earnest atten-
tion of Oriental scholars, and among the rest, of Professor Turner.
The discovery was important, because the inscription contains the
longest continuous text yet known in the Phenician language: a lan-
guage closely connected with the Hebrew. The labors of Professor
Turner upon this curious document were among the last of his life.

Two of the principal philological works published by the Smith-
sonian Institution were moulded into their present shape by Professor
Turner: the Dacota grammar and dictionary, and the grammar of
the Yoruba language. The materials furnished him were elaborated
with great skill and learning; and these two admirable volumes form
an interesting addition to philological science—the Dacota grammar
illustrating in a philosophical manner the characteristic peculiarities
of the American type of the agglutinating or polysynthetic languages,
and the Yoruba grammar illustrating the African type of the same
great division in the classification of human speech.

The unremitting labors of Professor Turner gradually undermined
his constitution. In October last he visited New York, partly for the

benefit of his impaired health, and partly to attend a meeting of the American Oriental Society, of which he was an active member. On his return to Washington, in November, he rapidly declined, and on Tuesday, the 29th of that month, expired, without pain, at the age of 49 years.

Professor Turner was not only distinguished for his abilities as a scholar, his extraordinary capacity for labor, his great power of grasping the generalization of the science to which he was devoted, but his private life was marked by singular purity. His manners were simple and cordial; his conversation lively and instructive. He was modest, without reserve; he was unobtrusive, but always ready to impart his affluent knowledge whenever the occasion seemed to call for it. The death of such a man is a loss to science and the country. I move the adoption of the following resolution:

Resolved, That this Board have learned with deep regret of the death of Professor W. W. Turner, a scholar of rare gifts and large acquirements, whose abilities and learning have in many ways been of great value to the Smithsonian Institution. As a philologist, he had but few equals; as an earnest laborer in the pursuit of knowledge, he was a high example to American students. As a public officer, he was upright, conscientious, and prompt in the discharge of every duty. His social virtues endeared him to his friends in no common measure. By his death American scholarship has sustained a heavy loss, this Institution has been deprived of an efficient collaborator, and the community at large of a virtuous and distinguished citizen.

On motion of Hon. J. G. Berret, it was

Resolved, That a copy of this resolution, with the introductory remarks, be transmitted to the family of the deceased.

The resolutions were adopted.

Professor Felton then addressed the Board as follows:

I have also, Mr. Chancellor, to call the attention of the Board to the death of an honorary member of the Smithsonian Institution—the beloved and illustrious Washington Irving, the most venerated representative of American literature. He was born April 3, 1783, in New York, and died at his residence, at Sunnyside, on the banks of the Hudson, November 28, 1859, in the 77th year of his age. His literary career extends over a period of more than half a century. For many years he has stood undoubtedly at the head of American literature. He enjoyed only the common opportunities of education in his youth; but the oldest universities of England and America honored themselves by conferring their highest honors on him in his manhood. At an early age he commenced the study of the law. His health failing, he travelled two years in Europe, and resuming his professional studies on his return, was admitted to the bar. Not finding the practice of the profession congenial to his tastes, he relinquished it, and became a partner in a mercantile house with his brother. But he was not destined to remain long in the career of trade; the failure of the house in the crisis that followed the peace of 1815 turned his attention to literature as a permanent pursuit. He had already shown by the

most decided proofs that nature had endowed him with the richest gifts of genius. His early writings, especially his contributions to Salmagundi, and Knickerbocker's History of New York, exhibit the keenest power of observation, the most brilliant wit, and an English style at once pure, copious, and expressive. But when he resolved to devote himself to letters as the business of his life, instead of the amusement of his leisure hours, he gave to the culture of style the thought, care, and labor that the painter and the sculptor expend in acquiring a mastery over the materials, principles, and processes of their respective arts. In the choice of his words and the structure of his sentences he exercised a refined taste and a delicate discrimination, allowing nothing to escape him which was not justified by the most fastidious judgment. He studied the best authors of the best ages in English literature, and disciplined his genius by a strict conformity to the established idiom of the mother tongue. Oddity and extravagance of expression, which some writers of our age mistake for originality of genius, found no favor with him. His genial nature, his sensibility to all that is beautiful in the works of God, his ready sympathy with the best affections of the human heart, were thus embodied in a style of marvellous grace, purity, and harmony. His imagination, gentle yet powerful, brightened everything it fell upon; his wit exhilarated and gladdened; his humor charmed by its sparkling play; his pathos, so true, so tender, colored with the unforgotten sorrow of his own early bereavement, touched the chords of sympathy in every heart. He was an elegant essayist, a delightful biographer, a profound and brilliant historian, and his whole life was loyal to the highest interests of humanity. In private friendships he was faithful and generous. He had all the excellencies of the literary character, with none of its defects. He had no rivalries to disturb the serenity of his days, no jealousies to irritate his temper. While enjoying his own brilliant success, with a modest appreciation of its value, he rejoiced in the successes of others, and delighted to aid them with his powerful influence. He never had an enemy, for all men were his friends. He never uttered a word that could wound the feelings of the most sensitive; he never wrote a sentence that could offend the most delicate; he never printed a line which, dying, he could wish to blot. His genius has been recognized throughout the civilized world; his works are read and his name revered wherever a cultivated language has been the organ of a national literature. The legends of Spain and Italy have furnished congenial subjects for his pen. The manners and life of England have been more brilliantly illustrated by him than by any English writer of our time. His native land, however, has been crowned by the richer and mature products of his genius. The picturesque banks of the Hudson have been made classical by the charm with which his creations—poetical in all but the form—have invested them. It is his peculiar felicity to have built the most enduring monument to the discoverer of America and to the Father of his Country, with the latter of whom he was associated by his baptismal name.

Mr. Irving took a lively interest in all that concerned the intellectual progress of the country; in all that concerned humanity, beyond the

circle of his own literary interests. He was the first named trustee of the Astor Library under the will of its munificent founder, and for many years acted as the president of the board. He served as a director in the Savings Bank in the place of his residence until his death; and he was an officer of the village church, from which his own lifeless remains were borne to their final resting place by his mother's side. He had the prospects of this Institution much at heart, and gave his constant attendance to its proceedings during a whole season passed by him in Washington. Ripe in age, crowned with the most enduring honors of the world and with the warmest affections of his countrymen, having finished the work which was given him to do and laid aside his pen forever, after a short period of repose in the midst of his friends, at the close of an evening of social and domestic enjoyment, he passed away in a moment by a blessed euthanasia. We cannot be surprised at such an event, though it excites our sensibility. His death was in beautiful harmony with his life, for he died as he had lived, the beloved of men and the favored of Heaven.

Thinking thus, Mr. Chancellor, of Mr. Irving's life, character, and death, I offer the following resolutions:

Resolved, That the Board of Regents of the Smithsonian Institution recognize in the character of their late associate, Washington Irving, a conspicuous example of the noblest virtues and the most generous qualities that belong to human nature.

Resolved, That while lamenting his death with the peculiar sorrow of countrymen and associates in this Institution, yet, in common with the whole civilized world, they gratefully appreciate the services he has rendered to literature, and hold in reverent remembrance his long career of labors as an author no less loyal to truth and virtue than brilliant with the gift of genius and graced with the amenities and courtesies that are the fairest ornaments of social life.

On motion of Senator Douglas, it was

Resolved, That a copy of the above resolutions, together with the remarks that preceded them, be transmitted to the family of the deceased.

The resolutions were then adopted.

Professor Bache made the following remarks:

James P. Espy, one of the most original and successful meteorologists of the present time, died in Cincinnati, Ohio, on the 24th of January, 1860, in the seventy-fifth year of his age, after an illness of a week, at the residence of his nephew, John Westcott.

The early career of Mr. Espy as an instructor was marked by the qualities which led to his later distinction in science. He was one of the best classical and mathematical instructors in Philadelphia, which at that day numbered Dr. Wylie, Mr. Sanderson, and Mr. Crawford among its teachers.

Impressed by the researches and writings of Dalton and of Daniell on meteorology, Mr. Espy began to observe the phenomena, and then to experiment on the facts which form the groundwork of the science.

As he observed, experimented, and studied, his enthusiasm grew, and his desire to devote himself exclusively to the increase and diffusion of the science finally became so strong that he determined to give up his school, and to rely for the means of prosecuting his researches upon his slender savings and the success of his lectures, probably the most original which have ever been delivered on this subject. His first course was delivered before the Franklin Institute of Pennsylvania, of which he had long been an active member, and where he met kindred spirits, ready to discuss the principles or the applications of science, and prepared to extend their views over the whole horizon of physical and mechanical research. As chairman of the committee on meteorology, Mr. Espy had a large share in the organization of the complete system of meteorological observations carried on by the institute under the auspices and within the limits of the State of Pennsylvania.

Mr. Espy's theory of storms was developed in successive memoirs in the Journal of the Franklin Institute, containing discussions of the changes of temperature, pressure, and moisture of the air, and in the direction and force of the wind and other phenomena attending remarkable storms in the United States and on the ocean adjacent to the Atlantic and Gulf coast. Assuming great simplicity as it was developed, and founded on the established laws of physics and upon ingenious and well-directed original experiments, this theory drew general attention to itself, especially in the United States. A memoir submitted anonymously to the American Philosophical Society of Philadelphia gained for Mr. Espy the award of the Magellanic premium in the year 1836, after a discussion remarkable for ingenuity and closeness in its progress, and for the almost unanimity of its result.

Mr. Espy was eminently social in his mental habits, full of bonhommie and of enthusiasm, easily kindling into a glow by social mental action. In the meetings and free discussions in a club formed for promoting research, and especially for scrutinizing the labors of its members—and of which Sears C. Walker, Professor Henry, Henry D. Rogers, and myself were members—Mr. Espy found the mental stimulus that he needed, and the criticism which he courted, the best aids and checks on his observations, speculations, and experiments. But there was one person who had more influence upon him than all others besides, stimulating him to progress, and urging him forward in each step with a zeal which never flagged—this was his wife. Having no children to occupy her care, and being of high mental endowment and of enthusiastic temperament, she found a never-failing source of interest and gratification in watching the development of Mr. Espy's scientific ideas, the progress of his experiments, and the results of his reading and studies; the collection and collation of observations of natural phenomena in the poetical region of the storm, the tornado, and of the aurora. Mrs. Espy's mind was essentially literary, and she could not aid her husband in his scientific inquiries or experiments: her health was delicate, and she could not assist him in his out-door observations; but she supplied what was of more importance than these aids—a genial and loving interest ever manifested in his pursuits and successes, and in his very failures. *Alere flam-*

mam was the office of her delicate and poetical temperament. Younger than Mr. Espy, she nevertheless died several years before him, (in 1850,) leaving him to struggle alone in the decline of life without the sustaining power of her devoted and enthusiastic nature.

Having in a great degree matured his theory of storms; having made numerous inductions from observations, and having written a great deal in regard to it, Mr. Espy took the bold resolution, though past middle age, to throw himself into a new career, laying aside all ordinary employments, and devoting himself to the diffusion of the knowledge which he had collected and increased, by lecturing in the towns, villages, and cities of the United States. This proved a successful undertaking, and by its originality attracted more attention to his views than could have been obtained, probably, in any other way. He soon showed remarkable power in explaining his ideas. His simplicity and clearness enabled his hearers to follow him without too great effort, and the earnestness with which he spoke out his convictions carried them away in favor of his theory. The same power which enabled him to succeed in his lecturing career procured subsequently for Mr. Espy the support and encouragement of some of the leading men ·in Congress, and especially in the Senate, and also in the executive departments. Their attention was arrested by the originality of his views and his warmth in presenting them, and he imparted so much of his conviction of their truth as to induce many of our statesmen and official persons to exert themselves to procure for him, under the patronage of the government, continued opportunities for study, research, and the comparison of observations. To the consistent support of his scientific friends, and particularly of the Secretary of this Institution, Mr. Espy owed also much in obtaining the opportunities of keeping in a scientific career. His reports to the surgeon general of the army, to Congress, and to the Secretary of the Navy, are among his latest efforts in this direction.

The earnest and deep convictions of the truth of his theory in all its parts, and his glowing enthusiasm in regard to it; perhaps, also, the age which he had reached, prevented Mr. Espy from passing beyond a certain point in the development of his theory. The same constitution of mind rendered his inductions from observation often unsafe. His views were positive and his conclusions absolute, and so was the expression of them. He was not prone to examine and re-examine premises and conclusions, but considered what had once been passed upon by his judgment as finally settled. Hence his views did not make that impression upon cooler temperaments among men of science to which they were entitled—obtaining more credit among scholars and men of general reading in our country than among scientific men, and making but little progress abroad.

Feeling that his bodily vigor was failing, and that his life must soon close, the Secretary of the Smithsonian Institution induced him to re-examine the various parts of his meteorological theories of storms, tornadoes, and water-spouts, and to insert in his last report, while it was going through the press, an account of his most mature views. I trust that the Secretary will, in one of his reports, give us a thorough and critical examination of the works and services of this remarkable

contributor to a branch of science, the knowledge of which the Smith-
sonian Institution has already done so much to advance and to diffuse.

On motion of Professor Bache, the following resolutions were adop-
ted:

Resolved, That the Regents of the Smithsonian Institution have
learned with deep regret the decease of James P. Espy, one of the
most useful and zealous of the meteorologists co-operating with the
Institution, and whose labors in both the increase and diffusion of
knowledge of meteorology have merited the highest honors of science
at home and have added to the reputation of our country abroad.

Resolved, That the Regents offer to the relatives of Mr. Espy their
sincere condolence in the loss which they have sustained.

On motion of Mr. Pearce, it was resolved that the remarks of Pro-
fessor Bache be entered in the proceedings.

The Secretary introduced the subject of warming the Smithsonian
building, stating that it was important to provide better means for
this purpose, to insure the safety of those parts of the building which are
not fire-proof. The subject was referred to the Executive Committee,
and the Secretary was instructed to procure estimates for the intro-
duction of steam or hot-water apparatus.

The reading of the report of the Secretary was continued.

The Board then adjourned.

SATURDAY, *March* 17, 1860.

The Board of Regents met this day, at 10 o'clock a. m.

Present: Hon. John C. Breckinridge, Hon. James M. Mason, Hon.
James A. Pearce, Hon. S. A. Douglas, Hon. William H. English, Hon.
Benjamin Stanton, Hon. J. G. Berret, Prof. A. D. Bache, Mr. Seaton,
Treasurer, and the Secretary.

Mr. Breckinridge was called to the chair.

The minutes were read and approved.

The Secretary announced the reappointment, by the Speaker of the
House of Representatives, of Hon. William H. English, of Indiana;
Hon. Benjamin Stanton, of Ohio; and Hon. L. J. Gartrell, of Georgia,
as Regents for the term of two years.

The Secretary presented the following letter from Edward Cunard,
Esq.:

NEW YORK, *February* 25, 1860.

DEAR SIR: I have to acknowledge the receipt of your letter of the
16th instant, and, in reply, I beg to inform you that I shall have much
pleasure in conveying in our steamers from New York to Liverpool
every fortnight one or more cases from the Smithsonian Institution to
the extent of half a ton or 20 cubic feet measurement. The cases
to be addressed to your agent in Liverpool, or to his care. The

arrangement of free cases is intended only to apply to those shipped by you from this side of the water.

<div style="text-align:center">Your obedient servant,</div>

<div style="text-align:right">E. CUNARD.</div>

JOSEPH HENRY, Esq.,
  Secretary Smithsonian Institution, Washington.

The Secretary presented the following letter from Sir W. E. Logan:

<div style="text-align:right">MONTREAL, *March*, 1860.</div>

MY DEAR SIR: Understanding that the shells of the United States exploring expedition are being arranged, and that there are many duplicates, I should be rejoiced if a set of them could be obtained for our Provincial Museum. It may be the case that what we may be able to return for them may not equal their value; but the Canadian territory is a large one, and we shall have duplicates of our fossils from various parts, extending from Labrador to Lake Superior.

In our geological expeditions to the eastern part of the province, advantage has been taken of the opportunity to dredge in the Gulf of St. Lawrence, and we shall undoubtedly have duplicates of many of the specimens obtained. This season I hope to send an exploring party to the Straits of Belle Isle.

We are so much pressed with work at present that it may be a little time before our duplicates are ready, particularly as the protracted want of Professor Hall's third volume of the palæontology of New York disables us from naming many of our fossils according to his authority, while a regard for him prevents us from naming them for ourselves. Our Lower Silurian fossils will be the first that will be ready.

<div style="text-align:center">I am, my dear sir, very truly yours,</div>

<div style="text-align:right">W. E. LOGAN.</div>

Professor HENRY,
  Smithsonian Institution, Washington.

A letter was read from Hon. Alfred Ely, chairman of the Committee of Claims of the House of Representatives, relative to an application of an officer of the navy for remuneration for specimens of natural history, &c., collected by the United States exploring expedition.

The subject was discussed, and referred to the Secretary and the Executive Committee.

A letter was read from Sir George Simpson, governor of the Hudson's Bay Territory, offering to aid the Institution in collecting meteorological and other information.

A letter was read from C. Zimmerman, of Columbia, South Carolina, on the subject of the preparation by the Institution of manuals on entomology.

The Secretary stated that a proposition had been made by Lieu-

tenant Gilliss relative to an expedition to the coast of Labrador to observe the total eclipse of July 18, if the necessary means could be secured to defray the expenses, towards which, if the Institution would subscribe $500, the balance, it was believed, could be secured from individuals.

Professor Bache addressed the Board, commending highly the proposed expedition, and stating the advantages which would result to science if the observations could be made.

On motion of Professor Bache, it was

Resolved, That an appropriation be made, not exceeding $500, to aid in the proposed expedition to observe the eclipse of July 18, 1860.

The Secretary called the attention of the Board to another expedition, proposed by Dr. I. I. Hayes to the Arctic regions, and suggested the propriety of aid in furnishing that gentleman with the requisite instruments of observation.

On motion of Mr. Pearce, it was

Resolved, That the Secretary of the Institution be authorized to furnish such aid to the expedition of Dr. Hayes, in the way of instruments, as may be deemed advisable.

The Secretary introduced the subject of the Stanley gallery of Indian paintings, and stated that Mr. Stanley asked for an allowance of one hundred dollars a year to pay the interest on a debt he had incurred to prevent the sacrifice of the paintings by sale.

The subject was referred to the Secretary and the Executive Committee.

A letter from Professor Secchi, of Rome, was read, stating that he had obtained permission for the Institution to procure casts or moulds of celebrated works of art in that city.

The Secretary stated that Mr. Corcoran, of Washington, was about to found a gallery of art, and it was very desirable that the Institution should co-operate with him, especially in relation to copies of works of art from Italy.

The subject was referred to the Secretary and the Executive Committee.

The Secretary presented the continuation of his annual report; which was read.

The opinion was expressed by several of the Regents that a less number of lectures should be given than heretofore, twelve being considered sufficient for each season.

The Board then adjourned.

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SATURDAY, *April* 7, 1860.

The Board of Regents met this day at 10 o'clock a. m.   Present: Hon. James A. Pearce, Hon. S. A. Douglas, Hon. W. H. English Hon. Benjamin Stanton, Hon. George E. Badger, Professor Bache, and the Secretary.

Mr. Pearce was called to the chair.

The minutes were read and approved.

Mr. Pearce presented the Report of the Executive Committee which was accepted, and the estimates for the year 1860 adopted.

On motion of Mr. Douglas, it was

Resolved, That the Executive Committee invest the five thousand dollars now in the hands of the Treasurer, belonging to the extra fund.

The Secretary laid before the Board the eleventh volume of Smithsonian Contributions to Knowledge, which had just been issued.

The Secretary brought before the Board the subject of the pay of the assistants; which, after some remarks, was referred to the Secretary and the Executive Committee.

Professor Bache made the following remarks:

Mr. Gustavus Wurdemann, in charge of the tidal observations of the Coast Survey on the Florida reefs and Gulf of Mexico, died at his home in New Jersey on the 30th of September.   His health had been failing for some years, and during the last year he had discharged his duties with great difficulty, owing to great physical debility.   Mr. Wurdemann entered the survey under my predecessor, and served, throughout a somewhat extended career, with a fidelity and singleness of purpose that has never been exceeded.   Exact truthfulness was the leading trait of his character, and his observations, even the most minute, were always reliable.   It is easily seen that it is no exaggeration to say that such a man was invaluable in his place, and an example worthy to be held up as the type of faithfulness.   During the discharge of his laborious duties he found time and opportunity to make collections in natural history, which have been acknowledged by the Smithsonian Institution as among the most valuable contributions to the knowledge of the fauna of Florida.

On motion of Professor Bache, the following resolution was unanimously adopted:

Resolved, That the Regents of the Smithsonian Institution have learned with regret the decease of Gustavus Wurdeman, tidal observer in the Coast Survey, whose collections of specimens from the coast of the Gulf of Mexico, and especially of the birds of Florida, liberally furnished to the Smithsonian Institution, have proved of great importance in increasing our knowledge of the natural history of the southern part of the United States.

Resolved, That this resolution be communicated to the widow of Mr. Wurdemann.

The Secretary read the following authentic notice, which had appeared in a recent periodical, respecting the late Professor Cleaveland :

"Professor Parker Cleaveland died on the 15th of October, 1858. He was born in Rowley (Byfield parish) Massachusetts, January 15, 1780, graduated at Harvard College in 1799, taught school and studied law until 1803, when he was appointed tutor in mathematics in Harvard College. He was made professor of mathematics and natural philosophy, chemistry and mineralogy in Bowdoin College in 1805, and discharged with distinguished ability the extended duties of that professorship until 1828, when a professor of mathematics was appointed, and he was relieved from that part of his labor. He continued to be the professor in the other departments until his death. He became widely known in the United States, and in Europe, by his early and successful treatise on mineralogy and geology, published in 1816, and in a second edition in 1822. A third was called for, and he labored in its preparation more or less for thirty-five years, leaving it nearly ready for the press. His high reputation as a lecturer was spread through the country by a succession of graduates of Bowdoin College of more than fifty years. He was a member of the American Academy of Arts and Sciences, and of many literary and scientific societies in this country and in Europe. In 1824, the honorary degree of Doctor of Laws was conferred on him by Bowdoin College. In private life he was universally respected for his unblemished moral character, and his genial and affable disposition. His death called forth unusual and remarkable demonstrations of respect to his character and memory. In June, 1853, he was elected an honorary member of the Smithsonian Institution."

On motion of Mr. Douglas, the following resolutions were adopted:

Resolved, That the Regents of this Institution have learned with deep regret of the decease of Professor Parker Cleveland, of Bowdoin College, one of the honorary members of this establishment, who was highly esteemed on account of his labors as a man of science and a teacher, and whose memory will be held in grateful remembrance.

Resolved, That the Regents offer to the family of the deceased their sincere condolence at the loss which they and the country have sustained.

The Secretary presented the following letter from Mr. Ross, chief factor of the Hudson's Bay Company:

FORT SIMPSON,
McKenzie's river, 30*th November,* 1859.

DEAR SIR: At the period of the departure of our usual winter express I sit down to write you a few lines upon the subjects mentioned

in your communication of the 2d of April, 1859. I trust that the various cases sent you last summer from Portage La Loche reached you in safety, and that the contents proved satisfactory and of interest. It will be my endeavor during the present and succeeding seasons to collect the animals mentioned as being wished for by the Smithsonian Institution, but I will not merely restrict myself to these particular objects of research, the whole field of either science or curiosity will be considered in all contributions which I may hereafter forward to your collection.

The Meteorological Register for the months of September, October, and November, will be forwarded by this conveyance, and I will endeavor to organize a systematic series of observations at all the posts throughout this district. These of course will vary as to completeness and accuracy according to the tastes and acquirements of the officer who conducts the registry, as there are very wide differences in the education and talents of the various persons in the progressive grades of our service. A series of spirit thermometers of assured correctness would be useful, in fact are absolutely necessary for this purpose.

As my attention will hereafter be particularly directed to ethnological pursuits; and my public duties in conducting the affairs of this large district are not very light, it will be impossible to keep the regular series of meteorological observations here myself, but I will delegate this duty to Mr. Andrew Flett, a very careful and intelligent person, though not of a finished eduction; but any extraordinary phenomena I will note myself in addition.

By the usual summer boats a packet will be forwarded to your address, containing such observations as I can collect in our journals, and a complete Auroral and Weather Register taken by myself for Colonel Lefroy in 1850 –'51, if I can find the latter.

In conclusion I will merely say that all that lies in my power will be done to oblige you in any way. Every facility will be given to Mr. R. Kennicott to collect and forward specimens of natural history; free passage will be allowed him from post to post throughout the district, and to all his plans the various officers under my command will, I am sure, gladly render assistance.

I have the honor to remain, dear sir, yours faithfully,

BERNARD R. ROSS.

PROFESSOR HENRY,
Smithsonian Institution.

The reading of the report of the Secretary was then continued.

On motion of Mr. Badger, the following resolution was adopted:

Resolved, That the thanks of the Board of Regents are hereby given to the various companies and individuals who have generously aided in advancing the objects of the Smithsonian Institution and the promotion of science, by the facilities they have afforded in the transportation of books, specimens, &c., free of charge.

The Board then adjourned to meet at the call of the Secretary.

GENERAL APPENDIX

TO THE

REPORT FOR 1859.

The object of this Appendix is to illustrate the operations of the Institution by the reports of lectures and extracts from correspondence, as well as to furnish information of a character suited especially to the meteorological observers and other persons interested in the promotion of knowledge.

LECTURES

ON AGRICULTURAL CHEMISTRY.

BY PROFESSOR SAMUEL W. JOHNSON, OF YALE COLLEGE, CONNECTICUT.

LECTURE I.

THE COMPOSITION AND STRUCTURE OF THE PLANT.

The objects of agriculture are the production of certain plants and certain animals which are employed to feed and clothe the human race. The first object in all cases is the production of plants.

Nature has made the most extensive provision for the spontaneous growth of an immense variety of vegetation; but, except in rare cases, man is obliged to employ art to provide himself with the kinds and quantities of vegetable produce which his necessities or luxuries demand. In this defect, or rather neglect of nature, agriculture has its origin.

The art of agriculture consists in certain practices and operations which have grown out of an observation and imitation of the best efforts of nature, or have been hit upon accidentally.

We distinguish here between agri-*culture*, or the culture (improvement) of the field, and farming, which may be anything but the imitation of nature, which often is the grossest violation of her plain precepts.

The science of agriculture is the rational theory and exposition of the successful art.

Nothing is more evident than that agricultural art impedes its own growth by holding aloof from science. In many respects the Egyptians, the Romans, and the Chinese, had, centuries ago, as perfect an agricultural practice as we now possess; but this fact so demonstrates the extreme slowness with which an empirical art progresses, that incalculable advantage must be anticipated from yoking it with the rapidly-developing sciences. In fact, the history of the last fifty years has proved the benefits of this union; and no farmer who by the help of science has mastered but one of the old difficulties of his art that for all time have been tormenting the thoughtful with doubt and misleading every one into a wasteful expenditure of labor or material, would willingly return to the days of pure empiricism. On the other hand, those who attempt to unfold the laws of production from considerations founded merely in the pure sciences, without regard to, or knowledge of, the truths of practice, are sure to go astray and bring discredit on their efforts.

Agriculture, *i. e.* field culture, not husbandry or farm management in the widest sense, is a natural science, and is based principally upon physics, chemistry, and physiology.

By physics (natural philosophy) is meant the science of matter considered in relation to those forces which act among masses, or among

particles, (atoms,) in such a manner as not to alter their essential characters.

The forces of cohesion, gravitation, heat, light, electricity, and magnetism, are physical forces. A thousand fragments of iron, for example, may be made to *cohere* together or *gravitate* to the earth, may be changed in temperature, illuminated, electrified or magnetized, without any permanent change in that assemblage of properties which constitutes this metal.

Chemistry is the science of chemical force or affinity, which causes two or more bodies to unite with the production of a compound possessing essentially new characters. Thus a hard lump of quicklime when brought in contact with water greedily absorbs it, with the production of great heat, and falls to powder. In slacking, it has *combined* chemically with water.

Physiology is the science of the processes of life, which require, in addition to the chemical and most of the physical forces, the co-operation and superintendence of the vital principle.

The first inquiries in the natural science of agriculture are: What is the plant? Out of what materials, and under what conditions is it formed?

The plant is the result of an organism, the germ, which under certain influences begins an independent life, and grows by constructively adding to itself or assimilating surrounding matter.

The simplest plant is a single *cell*, a microscopic vesicle of globular shape, which, after expanding to a certain size, usually produces another similar cell division either by lateral growth or by its own.

In the chemist's laboratory it is constantly happening that, in the clearest solutions of salts, like the sulphates of soda and magnesia, a flocculent mould, sometimes red, sometimes green, most often white, is formed, which, under the microscope, is seen to be a vegetation consisting of single cells. The yeast plant (fig. 1) is nothing more

Fig. 1.

than a collection of such cells now existing singly, now connected in one line or variously branched.

The cell is the type of all vegetation. The most complex plant, a stalk of cane or an oak, is nothing more than an aggregation of myriads of such cells, very variously modified indeed in shape and function, but still all referable to this simple typical form.

In the same manner that the yeast plant enlarges by budding or splitting into new cells, so do all other plants increase in mass; and thus growth is simply the formation of new cells.

So far as the studies of the vegetable physiologist enable us to judge, all vegetable cells consist, at least *in the early stages of their existence*, of an external, thin, but continuous (imperforate) membrane, the *cell-wall*, consisting of a substance called *cellulose*, and an interior lining membrane of slimy or half liquid character, variously called the *protoplasm*, the *formative layer*, or the *primodial utricle*, (fig. 2.)
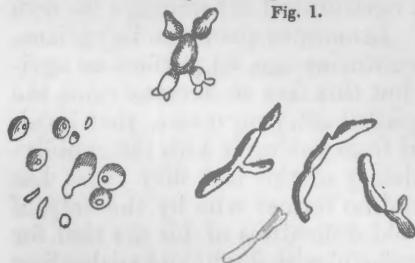
At one or several places, the formative
layer is thickened to the so-called *nucleus*,
(*a* fig. 2) the point from which growth
and transformations proceed.   Within
the cell thus constructed exists a liquid,
the *cell-contents*, from which, in course
of time, solid cell contents of various
character are found to develope.

Fig. 2.
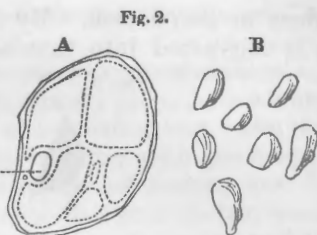
In a chemical sense, not less than in a structural, the single globu-
lar cell is the type of all vegetation.

The outer wall of the cell is formed of that material which is
itself the most abundant product of vegetable life, and which rep-
resents an important group of bodies, that are familiar to all, as large
ingredients of our daily food.

The table which here follows gives the names and the chemical
formulæ of what we may term the CELLULOSE GROUP or the VEGETAL
CARBO-HYDRATES.

Cellulose	C_{12}	H_{10}	O_{10}
Starch	C_{12}	H_{10}	O_{10}
Inulin	C_{12}	H_{10}	O_{10}
Dextrin	C_{12}	H_{10}	O_{10}
Gum	C_{12}	H_{11}	O_{11}
Cane sugar	C_{12}	H_{11}	O_{11}
Fruit sugar	C_{12}	H_{12}	O_{12}
Grape sugar	C_{12}	H_{14}	O_{14}

Cellulose is the body already alluded to as constituting the material of
the outer coating of the cells.   It often accumulates in some parts of
the plant by the thickening of the cell
walls, thus forming the greater share
of the wood (fig. 4) of trees and shrubs.
Linen, hemp, (B fig. 3) and cotton (A fig.
3) are nearly or quite pure cellulose. It
exists largely in the stones or shells of
fruits and nuts.   The so-called vege-
table ivory is chiefly a very compact
form of cellulose.   In general, this
proximate organic element is the
frame-work of the plant, and the
material that gives toughness and
solidity to its parts.

Fig. 3.

Fig. 4.
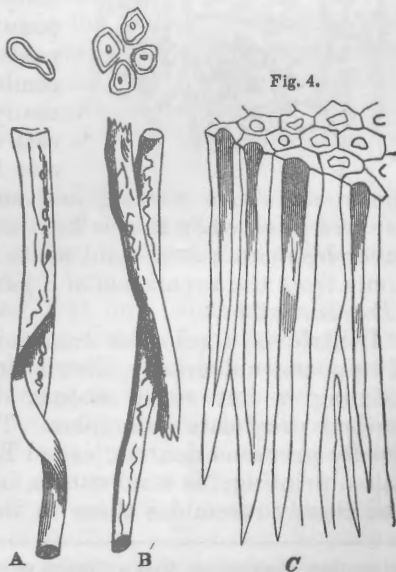
Cellulose is characterized by its
great indifference to most ordinary
solvents.   Water, alcohol, &c., do
not dissolve it, and the stronger rea-
gents of the chemist rarely take it
up without occasioning essential
changes in its constitution.*   With
strong nitric acid it yields nitro-cel-

* According to Pelouze, cellulose is dissolved by strong hydrochloric acid, and separates
again in part (part is converted into sugar) on dilution.   Schweitzer has recently made the

lulose or gun cotton.   By the continued action of oxydizing agents it is converted into that series of brown bodies known under the name of Humus, or finally into oxalic and carbonic acid.

Fig. 5.
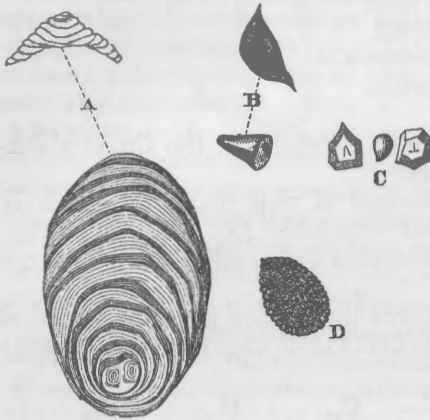
Next to cellulose, starch (fig. 5) is the most abundant vegetable body.   It usually occurs as microscopic grains, which for many species of plants possess a characteristic form and size, being sometimes angular as in maize, but most often oval or spherical as in the other grains, the potato, &c.

Starch is insoluble in and unaffected by cold water; in hot water it swells up and forms a translucent jelly, and in this state is employed for stiffening linen.

Starch is always enclosed in the cells of the plant as seen in the accompanying figure 6, and is exceedingly abundant, existing not only in the grains and esculent roots, but also in the trunks of trees, especially the sago-palm, and throughout nearly the whole tissue of the higher orders of plants.

Fig. 6.
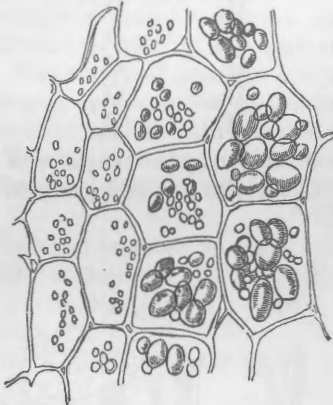
Inulin closely resembles starch in many points, appearing to replace that body in the roots of the artichoke, elecampane, dahlia, dandelion, and other composite plants. It occurs in the form of small round transparent grains, which dissolve easily in boiling water, and mostly separate again as the water cools.   Unlike starch, inulin exists in a liquid form in the roots above named, and separates in grains from the clear pressed juice when this is kept some time.   The juice of the dahlia tuber becomes a semi-solid white mass in this way, after reposing 12 hours from the separation of 8 per cent. of this interesting substance, (Bouchardat.)

Dextrin is a colorless transparent body, soluble in water, and it appears universally distributed in the juices of plants, though existing in but small amount compared with the previously described proximate principles.   The solution of an impure and artificially prepared dextrin, called British gum, is largely employed in calico printing, as a substitute for the more expensive natural gums, and closely resembles them in its adhesive properties.   It is an im-

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interesting observation, that a solution of oxyd of copper in ammonia dissolves cellulose to a clear liquid, from which the cellulose may again be thrown down by an acid.

portant ingredient of bread, being formed in the loaf by the process of baking, from the transformation of starch.

Gum is a generic term, and includes a number of substances, as gum tragacanth, gum Arabic, gum Senegal, cherry gum, &c., which, though unlike in some respects, agree in composition, and have the property either of dissolving or swelling up in water with the formation of an adhesive mucilage or paste. In the bread grains there is usually found a small quantity of gum soluble in water, and in meal from the seed of millet it has been observed to the amount of 10 per cent.

The sugars are so familiar that they scarcely require special notice.

Cane sugar or sucrose is the intensely sweet soluble crystallizable principle found in the juice of the cane, maple, and sugar beet. It is found, besides, in many other plants.

Fruit sugar or fructose is uncrystallizable, and exists in the juice of acid fruits, in honey and in the bread grains.

Grape sugar or glucose is found solid and crystallized in dried fruits, especially in the grape. It gradually separates from honey as the latter candies.

In the young cell this group of bodies is represented by cellulose, as the cell wall, and by dextrin and the sugars existing in its fluid contents.

The machinery of the vegetable organism, which all the while operates as perfectly in the single cell as in the complex mass of cells, has the power to transform most if not all these bodies into every other one, and we find them all in every individual of the higher orders of plants—at least in some stage of its growth. From dextrin, which is dissolved in the juice of a parent cell, is moulded the cellulose which envelopes a new cell.

From starch, and perhaps cellulose in the stem of the maple, cane sugar is formed in the changeful temperature of spring, and, as the buds swell, this sugar is reorganized again into cellulose and starch.

The analysis of the cereal grains oftentimes reveals the presence of dextrin, but no sugar or gum; while at other times the latter are found, but not the former.

It is easy to imitate many of these transformations outside of the vegetable organism. By the agency of heat, acids, and ferments, either singly or jointly, we may effect a number of remarkable changes.

Cellulose and starch are converted, first, into dextrin, and finally into grape sugar, by boiling with a dilute acid. In this way glucose is largely manufactured from potato starch, and has, in fact, been made from saw-dust. This transformation is also effected by the digestive apparatus of herbivorous animals, and in case of starch by a roasting or baking heat. So, too, in the sprouting of seed, the same changes occur, as exemplified in the preparation of malt.

By heat and acids inulin is also converted into a kind of sugar, but without the intermediate formation of dextrin. The same is true of the gums. By these agencies cane sugar is converted into fruit sugar, and this spontaneously passes into grape sugar.

Grape sugar is thus seen to be the final product of the transformation of the carbo-hydrates, either in the vessels of the chemist or in the digestive process of animals. It is the form in which the carbo-hydrates of the food pass into the blood, and, in consequence, it is a constant ingredient of the latter.

It will be noticed that while physical and chemical agencies produce these metamorphoses in one direction, it is only with the assistance of the vital principle that they can be accomplished in the reverse manner.

In the laboratory we can only reduce from a higher, organized, or more complex constitution, to a lower and simpler one. In the vegetable cell, however, all these changes, and many more, take place with the greatest facility.

The ready convertibility of one member of this group into another is to some extent explained by the identical or similar composition of these bodies. It will be observed by reference to the table that *they are all composed of carbon, hydrogen, and oxygen.*

That they contain carbon is made evident by their yielding *charcoal* when heated with imperfect access of air. When heated they also yield *water*, which, as all know, is a compound of oxygen and hydrogen.

Furthermore, several of these bodies *contain the same proportions of these elements.* The formulæ of cellulose, starch, inulin, and dextrin are identical. The remaining compounds only differ by the elements of one or several atoms of water.

The term carbo-hydrates (very convenient for our present purpose, though to the chemist absurd) was applied here because we may in a certain sense consider all these substances as hydrates of carbon. They are, in fact, composed of carbon and the elements of water.

These bodies in their transformations have merely to undergo a rearrangement of atoms, just as the rearrangement of a few blocks enables the child to build a variety of toy-houses; or at most they need only lose or assume a few atoms of water—an omnipresent body, characterized by the facility with which it enters into all manner of combinations—and the work is accomplished.

To furnish a more complete illustration of the typifying of all vegetation by the single cell, and at the same time to extend our inquiry into the composition of the plant, we may now advert to the lining membrane of the cell-wall, or, as physiologists term it, the protoplasm, formative-layer, or primordial utricle. This consists chiefly of some body that differs in chemical composition from the group just described by containing, in addition to the three elements that form the carbo-hydrates, about 16 per cent. of a fourth element, *nitrogen*, and small quantities of sulphur, and perhaps sometimes phosphorus.

The following table gives the names and percentage composition of the most important.

Albuminoids or Nitrogenous Vegetal Principles.

	Carbon.	Hydrogen.	Oxygen.	Nitrogen.	Sulphu.
Vegetal albumin · · · · · · · · · ·	54.8	7.3	21.1	15.9	0.9
Vegetal fibrin, or gluten · · ·	54.0	7.2	22.3	15.7	0.7
Vegetal casein · · · · · · · · · · ·	54.6	7.4	21.7	15.8	0.5

These bodies differ considerably in certain characters, though their similarity in others is very strongly marked. In composition they are almost identical. From the difficulty of obtaining them in the pure state their precise composition is not definitely known. The figures in the table represent the mean results of the best analyses.

The names albumin and casein originated from animal substances, and in fact we find in the animal kingdom a series of bodies corresponding almost perfectly with the vegetable nitrogenous principles.

In the white of the egg, in the serum of blood, and in many diseased animal secretions, we meet with albumin which has the property of passing from its usual fluid condition into the solid form on the application of heat. It is said to coagulate.

In the vegetable, albumin exists in much smaller relative quantity than in the animal; but it may be found in the juice of nearly all plants. If potatoes, turnips, or flour be digested for some time in water and the liquid then allowed to clear by settling, it contains a minute quantity of albumin in solution, as may be made evident by heating it, when a coagulum of this body separates.

Casein is an ingredient of the milk of animals. Heat does not coagulate it, but acids have this effect. Cheese has casein for its characteristic constituent.

In the seeds of leguminous plants, as the pea and bean; in the peanut and almond, this body exists very abundantly.

If crushed peas are soaked some hours in warm water, to which a little ammonia is added, they yield casein to the liquid, and on the addition of an acid it is separated as a curdy matter like the casein of milk. In China a kind of cheese is thus largely manufactured. In smaller quantity casein is found in all the grains and seeds used as food.

Gluten exists in wheat, and may be obtained by slowly washing a dough made from wheat flour, whereby the starch is removed and a glutinous mass remains which is the substance in question. As thus seen, it is mingled with more or less albumin and casein, as well as oil and starch. Gluten is the characteristic ingredient of those grains from the flour of which a light raised bread may be made. Liebig has given to gluten the name vegetable fibrin, from its analogies with the fibre of flesh or animal fibrin.

The albuminoids, like the carbo-hydrates, are easily susceptible of mutual transformation. In the animal the casein of milk or beans, the albumin of eggs or of vegetables, and gluten, are converted first into albumin and liquid fibrin in the shape of blood, and afterward into the solid flesh. So, too, in the plant, similar changes, without doubt, occur.

To some extent these conversions may take place outside the organism. If, for example, animal fibrin be exposed with water to the

air for some days in a warm place, it disappears or dissolves; if now

Fig. 7.

the liquid be heated to near boiling, a coagulum separates, having all the characters of albumin. After removing the albumin, the addition of an acid causes another coagulation, separating a body that agrees in its properties with casein.

As has been already stated, the albuminoid bodies form the lining membrane of the young cell and are diffused in the dissolved state throughout its liquid contents. In those parts of the plant where these bodies accumulate, they are found nearly filling entire cells and series of cells. (e, fig. 7.)

According to Hartig (Entwickelungsgeschichte des Planzen Keims) the albuminoids exist in the seed in an organized form, usually in

Fig. 8.
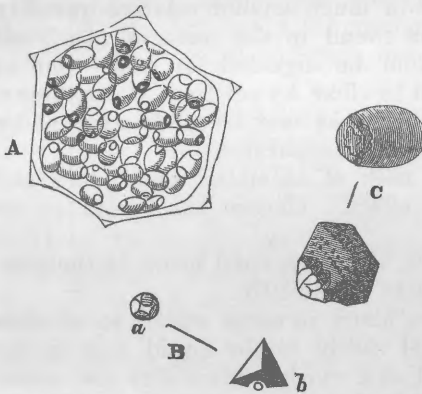
grains that are scarcely to be distinguished from starch by the eye, (A, fig. 8) often, however, in perfect polyhedral crystals. (Fig. 8, B and C.) This *aleuron*, as Hartig terms it, is not a pure albuminoid; as according to an analysis made from material prepared by him, it contains but 9.46 per cent. of nitrogen. The aleuron grains during the life of the plant suffer metamorphosis into starch and other organized matters, of course undergoing radical chemical changes at the same time.

While the two great classes of organic proximate elements, just considered, make up the larger share of vegetation, and suffice to show in the most beautiful manner how the single cell represents the whole plant, both structurally and chemically, we should stop short of the object of these lectures did we not consider some other vegetal principles of great importance both to the vegetable and animal economy, which agree with the cellulose group in consisting only of carbon, hydrogen, and oxygen, but differ again from the carbo-hydrates in the fact that their hydrogen and oxygen are not in the proportions to form water.

We may notice these substances under three divisions, viz:

Pectose and its derivatives.

The vegetal acids.

The oils and fats.

The *pectose group* includes pectose, pectin, pectosic, and pectic acids. These bodies exist principally in fleshy fruits and berries, and in the roots of the turnip, beet, and carrot. They are an important part of the food of men and domestic animals.

Pectose is the designation of a body which occurs with cellulose in

the flesh of unripe fruits, and of the roots just mentioned. Its properties in the pure state are quite unknown, since we have no method of separating it from the associated cellulose. Nearly the entire mass of green fruits and of these roots consists of pectose, which is recognized to be a special organic body by the products which it yields when submitted, either naturally or artificially, to the action of various chemical or physical agents.

Pectin is prepared from pectose in a way analogous to that by which cellulose or starch yields dextrin, viz: by the action of heat, acids, and ferments. When the fruits or roots that contain pectose are subjected to the action of gentle heat and an acid, the cellulose they contain is more or less changed into dextrin and sugar, and at the same time the firm pectose begins to soften, and in a little time becomes soluble in water, being converted into pectin. In the baking or roasting of apples and pears, and in the boiling of turnips and beets, it is precisely this transformation that occurs. When fruit ripens, either on the tree or, as happens with winter apples and pears, after being gathered, the same metamorphosis takes place. The hard pectose, under the influence of the acid (or ferment) that exists in greater or less quantity in the fruits, gradually softens and passes into pectin. If the clear juice of ripe pears be mixed with alcohol the pectin, which cannot dissolve in the latter liquid, is separated as a stringy gelatinous mass, that, on drying, remains as a white body, easily reducible to a fine white powder. The concentrated solution of pectin in water has a viscid or gummy consistence as seen in the juice that exudes from baked apples.

Under the further action of heat, acids, and ferments, pectin itself undergoes other transformations. We shall only notice its conversion into pectosic and pectic acids. These bodies, chiefly the first, together with sugar and flavoring matter, compose the delicious fruit jellies, which, as is well known, are prepared by gently heating for some time the expressed juice of strawberries and raspberries, or the juice obtained by stewing apples, pears, grapes, currants, gooseberries, plums, &c. They are both insoluble in cold water, and remain suspended in it as a gelatinous mass. Pectosic acid is soluble in boiling water, and hence most fruit jellies become liquid when heated to 212°. On cooling, its solution gelatinizes again. Pectic acid is insoluble even in boiling water. It is also formed when the pulp of fruits or roots containing pectose is acted upon by alkalies or by ammonia-oxyd of copper. This reagent (which dissolves cellulose) converts pectose directly into pectic acid that remains in insoluble combination with oxyd of copper.

Our knowledge of the composition of the bodies of the pectose group is very imperfect, from the difficulty or impossibility of preparing them in a state of purity. Below is a table of their composition according to the most recent investigations:

Pectose . . . . Unknown.
Pectin . . . . C_{32} H_{20} O_{28} + 4 HO.
Pectosic acid . . . . C_{32} H_{20} O_{28} + 3 HO.
Pectic acid . . . . C_{32} H_{20} O_{28} + 2 HO.

From the best analyses, and from analogy with cellulose, it is probable that pectose has the same composition as pectin, or, like the pectic and pectosic acids, differs from it only by one or more equivalents of water. This relatedness of composition assists us here, as in case of the preceding groups of organic principles, to comprehend, in some measure, the ease with which the transformations of these bodies are effected.

It will be perceived, by a glance at the composition of the pectose group, that their oxygen exceeds the quantity necessary to form water with their hydrogens by eight equivalents.

The *vegetal acids* are exceedingly numerous. They are found in all classes of plants, and nearly every family in the vegetable kingdom has one or more acids peculiar to itself.

Those we shall now notice are few in number, but of almost universal distribution. They are oxalic, tartaric, citric, and malic acids. In plants they never occur in the free or pure state, but always combined with lime, potash, ammonia, &c. They are most often accumulated in large quantity in fruits.

Oxalic acid exists largely in the common sorrel, and, according to the best observers, is found in greater or less quantity in nearly all plants. The pure acid presents itself in the form of colorless brilliant transparent crystals not unlike Epsom salts in appearance, but having an intensely sour taste. It is prepared for commerce by subjecting starch or cane and grape sugar to rapid oxydation, generally by means of nitric acid. Salt of sorrel, employed to remove ink-stains from cloth and leather, is an oxalate of potash and water.

Tartaric acid is especially abundant in the grape, from the juice of which during fermentation it is deposited in combination with potash, as *argol*, which, by purification, yields the cream of tartar of commerce. Tartaric acid, when pure, occurs in large glassy crystals very sour to the taste. It has recently been observed by Liebig as one of the products of the artificial oxydation by nitric acid, of the peculiar sugar found in milk, and is also probably a result of the oxydation of *gum* by the same reagent.

Malic acid is the chief sour principle of apples, currants, gooseberries, and many other fruits. It exists in large quantity in the garden rhubarb, in the berries of the mountain ash and barberry, and in the leaves of the beet and tobacco plants.

Citric acid is most abundant in the juice of the lemon, lime, and cranberry.

All these acids usually occur together in our ordinary fruits, and in some cases it is certain that they are converted the one into another during the development of the plant.

Their composition is expressed in the following table:

Oxalic acid .... C_4 O_6 + 2 HO.
Malic acid .... C_8 H_4 O_8 + 2 HO.
Tartaric acid .... C_8 H_4 O_{10} + 2 HO.
Citric acid .... C_{12} H_5 O_{11} + 3 HO.

The vegetal acids exert an important influence in the plant as

well as in the food of animals, by effecting the transformation of cellulose and starch into dextrin and sugar, and of pectose into pectin.

In all plants, and in nearly all parts of plants, we find some *fixed oil, fat* (or *wax;*) but it is chiefly in certain seeds that they occur most abundantly. Thus the seeds of maize, oats, hemp, flax (fig. 7 *f*), colza, cotton, pea-nut, beech, almond, sunflower, &c., contain from 6 to 70 per cent. of oil, which may be in great part removed by pressure. In some plants, as the African palm and the Nicaraguan tallow-tree, the oil is solid at ordinary temperatures, while many plants yield small quantities of wax, which either coats their leaves or forms a "bloom" upon their fruit. The oils differ exceedingly in taste, odor, and consistency, as well as in their chemical composition.

They all contain much carbon, and *less* oxygen than is requisite to form water with their hydrogen.

The oil or fat of plants appears to be, in many cases, a product of the transformation of starch or other member of the cellulose group, for the oily seeds when immature contain starch, which vanishes as they ripen, and in the sugar-cane the quantity of wax is always largest when the sugar is least abundant, and *vice versa.*

It has long been known that the brain and nervous tissue of animals contain several oils of which phosphorus is an essential ingredient. Recently Knop has discovered that the sugar-pea yields a similar oil containing 1.25 per cent. of phosphorus, in addition to carbon, hydrogen, and oxygen.

The bodies to which attention has thus been briefly directed, constitute by far the larger share of the solid matter not only of the young cell but of all vegetation. They comprise, nearly all, those vegetable substances which are employed as food or otherwise possess any considerable agricultural significance. The numberless acids, alkaloids, resins, volatile oils, coloring matters, and other principles existing in small quantity in the vegetable world, are unimportant to our present purpose.

We find under the microscope that certain of these bodies have an organized structure; such are cellulose, starch, inulin, and gluten, (aleuron of Hartig;) while others, as dextrin, gum, sugar, albumin, and casein, are the products of the disorganization of those above mentioned—the structureless materials, out of which the organized portions of the plant renew themselves.

To return to the cell. As the life of the plant progresses, not only does the form of the cell greatly change in many cases, but it undergoes very marked internal transformations. The liquid that fills the young cell contains both dextrin and albumin ; from the former is elaborated the walls of new cells, or else the existing cells are filled up more or less completely with some solid carbo-hydrate resulting from the transformation of dextrin. Thus in the potato tuber the cells are almost entirely occupied with starch. In the stem of trees the cells are lengthened and thickened by the continuous deposit of cellulose with other ill-defined bodies, and the result is wood. In the seeds of the cereal grains and numerous other plants we find the cells densely crowded, (hence polyhedral in figure,) and filled with

starch and gluten, the latter often crystallized. In leguminous seeds casein accumulates; while in the exterior portions of most seeds occur cells containing, in addition to these bodies, numerous droplets of fixed oil.—(See the figures already given.) Some cells are largely occupied with *coloring matter*, which is green in leaves, red, yellow, &c., in petals. In many cells we meet with crystals of salts; some are compounds of vegetal acids with lime and magnesia; others are phosphates and sulphates.

In every plant, and in each cell, there may be found, by chemical analysis, though generally not by the microscope alone, a certain, never-failing content of mineral matters, which remains as ash when the vegetable is burned. The ashes of all agricultural plants contain the following mineral matters, to which in the table are appended their chemical symbols:

Ingredients of the ash of plants.

Alkalies	{ Potash	KO
	{ Soda	Na O
Alkaline earths.	{ Lime	Ca O
	{ Magnesia	Mg O
Metallic oxyds	{ Oxyd of iron	$Fe_2 O_3$
	{ Oxyd of manganese	$Mn_3 O_4$
Acids	⌈ Carbonic	CO_2
	| Sulphuric	SO_3
	| Phosphoric	PO_5
	⌊ Silicic	$Si O_3$
Radical	Chlorine	Cl.

These matters taken together form but a small part of the plant— usually from one to five per cent. of its weight—yet they are indispensable to its development, as is evident from their constant presence, and as has been likewise proved by the most careful and extended synthetic experiments. Without the co-operation of all these earthy and saline matters it is impossible for plants of the higher orders to develop themselves.

The Prince Salm Horstmar, of Brunswick, has made the function of the mineral food of the plant the subject of a most extended and laborious investigation. In experiments with the oat he found that when *silica* was absent from the soil, everything else being supplied, the plant remained smooth, pale, dwarfed, and prostrate.

Without *lime* the plant died in the second leaf.

Without *potash* or *soda* it reached a height of but three inches.

Without *magnesia* it was very weak and prostrate.

Without *phosphoric acid* it remained very weak, but erect and of normal figure, bearing fruit.

Without *sulphuric acid* it was still weaker; was erect and of normal figure, but without fruit.

Without *iron* it was very pale, weak, and disproportioned.

Without *manganese* it did not attain perfect development, and bore but few flowers.

Other experiments proved that *chlorine* is essential to the growth of wheat.

Wiegmann and Polstorff found that when seeds of cress (*Lepidium sativum*) were sown in minced platinum wire, contained in a platinum crucible, and moistened with distilled water, the experiment being conducted under a glass shade, out of reach of dust, they germinated and grew naturally during twenty-six days, when, having reached a height of three inches, they began to turn yellow and to die down. On burning the plants thus produced, their ash was found to weigh *exactly as much* as was obtained from a number of seeds equal to that sown. Prince Salm Horstmar found that oats grown with addition of fixed mineral matters (ash ingredients) only, gave four times the mass of vegetable matter that was obtained when these were withheld.

The plant, as we have seen, is an assemblage of cells, which are situated in more or less close contact with each other. The plants that consist of but a few cells, like yeast, simply lie or float in the medium in which they are naturally found. Agricultural plants, however, and the higher orders generally, possess roots, whose functions are performed underground, and stems, leaves, and flowers, that exist in the air.

The yeast plant finds its food in the fermenting solution, and the cells have a power of absorbing their nutriment out of this solution.

Marine plants wholly immersed in the ocean abstract their food from the sea water.

The higher land plants derive the materials from which their cells are multiplied, partly from the *soil*, by their roots, and partly from the *atmosphere*, by their foliage.

In the living plant, then, there is provision for the access of liquids into the cells from without, and for the transmission of the same from one end of the plant to the other, or in any direction; for if we plant a seed in pure sand mingled with ashes and duly watered, we shall find in a few weeks that a plant has resulted containing in every portion of it carbon, hydrogen and nitrogen, which could only have been derived from the atmosphere, and also saline and earthy matters, which must have been imbibed from the ashes and carried upward to the points of its branches and leaves.

The young cell, though its wall reveals no perforation to the most powerful magnifier, is porous; and though the older cells, which form the cuticle of a somewhat developed plant, are often impermeable to water and air, from the fact that they are indurated or glazed by the formation of a corky or waxy coating, yet the young cells that are continually forming at the extremities of the advancing rootlets, and those of the still fresh leaves, are highly porous, and no more oppose resistance to the passage of water or of air than does a sieve.

We have only to immerse the roots of a vigorous plant in a solution colored with some harmless pigment, and in a short time we can trace its diffusion throughout the plant.

If liquids thus easily permeate these tissues, there is every reason to suppose that they may admit the vastly more subtle particles of a gas; and of this we have abundant experimental evidence, as will be set forth bye and bye.

LECTURE II.

THE ATMOSPHERE AND WATER IN THEIR RELATIONS TO VEGETABLE LIFE.

In the former lecture we have seen that the plant is a collection of cells, and the residence of an organizing up-building agency—the vital principle. We have seen that the cells are composed of, or occupied with, carbo-hydrates, albuminoids, fats, and salts. The structure of the plant admits the entrance of gases and liquids, and their diffusion throughout its mass.

We are now prepared to inquire what are the materials employed by the plant in its development—what is the food of vegetation?

A seed sown in a moist sand may grow into a perfect plant, and produce a hundred new seeds, each as large and complete as the first, although the sand, the water, and the air, which only can have nourished the plant, contain no traces of cellulose or starch, of albumen or oil.

These proximate elements of vegetation are then obviously constructed by the plant out of other forms or combinations of matter belonging to the mineral world, and to be sought in the *atmosphere*, in *water*, and in the *soil*.

Of the entire mass of the plant, but a small portion is derived from the soil, ninety-five to ninety-nine per cent. of it coming originally from the atmosphere.

The general composition of the pure and dry atmosphere, according to the most reliable data is, by weight, as follows: (To the names of the ingredients are appended their chemical symbols.)

Oxygen, O	23.18
Nitrogen, N	76.82
	100.00

Besides the above ingredients, whose proportion is very constant, there occur in it the following substances in more variable quantity:

Water, (as vapor,) HO, average 1-hundredth.
Carbonic acid, CO_2, average 6 ten-thousandths.
Ammonia, NH_3, average 23 billionths.
Nitric acid, NO_5?
Carburetted hydrogen, CH?
Nitrous oxyd NO?

Let us now inquire with reference to each of these substances, how is it related to the nourishment of the plant? A number of exceedingly ingenious experiments have been instituted from time to time for the purpose of throwing light on this subject, and we are thus fortunately able to present it in a quite satisfactory manner.

As to *oxygen*, we have no evidence that it directly feeds the plant, or is assimilated, so as to increase the mass of its organic matter. On the contrary, plants when growing exhale oxygen, separating it from the carbon and hydrogen of their proper food.

The presence of oxygen in the atmosphere is, however, in many

ways, essential to the perfection of the plant; for in its absence seeds cannot germinate, flowers cannot yield fruit, and fruits cannot ripen. In germination the larger bulk of the seed, the *cotyledons*, by the absorption of oxygen, are disorganized and converted into structureless and soluble bodies, which become the food of the smaller part of the seed, the *embryo*, and by its vital operations are again organized as the young plant. In the process of flowering, matters stored in organized form in other parts of the plant are transported to the blossom to serve for its rapid development. The flower itself cannot absorb food from without; and in the transformation of the already elaborated food from the stem and leaves of the plant into the new forms required by the flowers, oxygen plays an essential part. The reawakening of life in the tree at spring time, and the ripening of fruits, are accompanied with changes of a similar character, and from them result many *oxydized products*. Vegetable physiologists have furnished microscopic evidence that similar alternations of the organizing and disorganizing processes take place in the individual cells, so that we are warranted in assuming that oxygen (whether that of the free atmosphere or that evolved in the cells themselves is indifferent) plays an important and unceasing part in the development of vegetation.

Nitrogen in the free state also appears to be incapable of direct assimilation. Within a few years the subject has been studied by various investigators, but with contradictory results. Ville, of Paris, in 1853, published a volume describing his experiments, which led to the conclusion already arrived at by Priestley, in 1779, viz: that nitrogen is assimilated. Other investigators, however, by means of trials carried out under conditions less complicated and more adapted to yield reliable evidence, have uniformly been conducted to the opposite view.

Especially to Boussingault do we owe a most careful investigation of this question. His plan of experiment was simply to cause plants to grow in circumstances where, every other condition of development being supplied, the only source of nitrogen at their command, besides that contained in the seed itself, should be the free nitrogen of the atmosphere. For this purpose he prepared a soil consisting of pumice stone and the ashes of clover, freed by heat and acid from all compounds of nitrogen. This soil he placed at the bottom of a large glass globe, (see figure 9,) of 15 to 20 gallons capacity. Seeds of cress or of other plants were deposited in the soil, and pure water supplied to them. After germination, a small glass vessel (D) filled with carbonic acid (to supply carbon) was secured

Fig. 9.
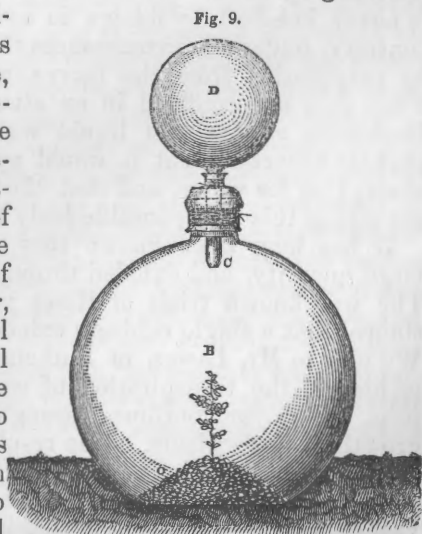
air-tight to the mouth of the large globe, and, the apparatus being
disposed in a suitably lighted place, was left to itself until the plants
began to turn yellow and show signs of decay.   Then they were re-
moved, separated from the soil, and, by chemical analysis, the amount
of nitrogen in them was ascertained.   It was found in every instance
(the experiment being several times repeated) that the nitrogen in the
plants thus raised was no more than that contained in the seed from
which they had grown.   Our ingenious countryman, Dr. Evan Pugh,
now president of the Farmers' College of Pennsylvania, while resi-
dent in England a few years since, made an elaborate investigation
of this subject, with results confirming those of Boussingault.

So far from the external free nitrogen being assimilated, it appears,
especially from the researches of Dr. Draper, of New York, that
plants constantly evolve this substance in the gaseous form; al-
though, according to the investigations of Unger and Knop, made
more recently, and with more exact methods, the nitrogen found by
various observers in the exhaled air of plants comes only from the
atmospheric air absorbed by them.

It thus appears that the two gases which, together, make up
ninety-nine per cent. or more of the atmosphere, do not constitute
in any way the direct food of vegetation.   It is, in fact, in the small
quantity of other and somewhat variable ingredients that we must
look for the atmospheric nutriment of the vegetable kingdom.

Water in the vaporized form we find never absent from the air,
and it is especially abundant in the warm period of the year when
vegetation is active.   Its presence is made evident by its deposition
in the states of dew, fog, rain, and snow, when the temperature of
the atmosphere is reduced.

It has been universally taught that the watery vapor which is thus
in perpetual contact with the leaves of plants is readily and largely
absorbed by them.   According to Unger and Duchartre, however, it
is never imbibed by foliage in even the slightest degree.   On the
contrary, under all circumstances there occurs a constant loss of water
by evaporation from the leaves, which does not wholly cease even
when they are confined in an atmosphere saturated with moisture.
Duchartre admits that liquid water in contact with the leaves is
slightly absorbed; but it would appear that the root is the organ of
absorption for water, and that the soil must perform the function of
supplying this indispensable body to the plant.

It has long been known that water is absorbed by the roots in
large quantity, and exhaled through the leaves into the atmosphere.
The well-known trials of Hales prove this.   He found, in one in-
stance, that a single cabbage exhaled 25 ounces of water in 24 hours.
We owe to Mr. Lawes, of Rothamstead, England, a series of experi-
ments on the transpiration of water through wheat, barley, beans,
peas, and clover, continued throughout nearly the whole period of the
growth of these plants.   The result was, that for every grain of solid
matter added to the mass of the plant 150 to 270 grains of water
passed through it.   From these, and especially from very recent in-
vestigations of Knop and Sachs, it is seen that the transpiration is

very variable, as might be anticipated. It takes place most rapidly in a dry, warm air, but is not absolutely checked when the atmosphere is saturated with moisture. Transpiration is remarkably diminished by the presence of many soluble salts, and of the alkalies, in the water of the soil; while free acids increase its rapidity and amount.

As is well known, water is a compound of hydrogen and oxygen; although we have no direct evidence, the inference is fully warranted that a portion of the water which enters the plant by the roots is arrested in its upward path, to become itself a part of the tissues. It is either held in the form of hygroscopic moisture, or is united chemically to carbon; or, finally, it is decomposed, its hydrogen being retained, and its oxygen eliminated wholly or in part. In fact, we must regard water as the chief source of the hydrogen which is a component of almost every vegetable principle.

Carbonic acid is a compound of carbon and oxygen. It exists in immense quantities in solid combination with lime in the various marbles, limestones and marls, and in chalk. Separated from these bodies by pouring on them sulphuric or nitric acid, it may be collected as a gas, which, unrecognizable by the other senses, is agreeably sour to the taste; is two and a half times heavier than common air, and considerably soluble in water. This gas is never absent from the air, and although it occurs there in relatively small quantity, its absolute amount is so great that, taking the atmosphere up to its entire height, we have no less than seven tons of carbonic acid over every acre of surface.

A plant confined in an atmosphere free from this gas cannot enlarge itself.* Some plants will live and grow in a confined space, as for example, sealed up in a bottle ; but in this case the carbonic acid consumed by the growing parts of the plant is supplied by the decay of the lower leaves.

Priestly and Saussure long ago furnished experimental evidence that carbonic acid is absorbed by growing plants, and Boussingault has described the following illustration of the rapidity with which the gas is imbibed by the foliage of vegetation. Into one of the orifices in a three-necked glass globe he introduced the branch of a living vine bearing twenty leaves ; with another opening he connected an apparatus by means of which a slow current of air, containing a small, accurately known proportion of carbonic acid could be passed into the globe. This air after streaming over the vine leaves, escaped by the third neck into an arrangement for collecting and weighing the carbonic acid that remained in it. The experiment being set in process in the sun-light, it was found that the enclosed foliage removed from the current of air three-fourths of the carbonic acid it at first contained.

The absorption of the gas in question *by the leaves* is found to take place only under the influence of the light of the sun, or of the accompanying chemical rays. Through the *roots*, carbonic acid, when held in solution of water, may be absorbed at all times.

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* Unless, indeed, as is probable, *carburetted hydrogen* may, to a small extent, be an actual source of carbon to plants, a point not yet satisfactorily determined.

It is, however, only in the sun-light, and with many plants (according to the recent researches of Corenwinder) only in *direct* sun-light that carbonic acid or, more properly, carbon is assimilated. We have already alluded to the fact that oxygen is exhaled by the plant. This oxygen comes from the decomposition of carbonic acid (and water) in the interior of the plant. The vegetable cell aided by the sun has the power of separating the elements of this compound with the greatest ease, and it retains the carbon to add to its structure while the oxygen escapes entirely or in part into the general atmosphere.

As already mentioned, however, oxygen itself, under certain circumstances, more particularly at certain stages of vegetable development, is absorbed; and as a consequence of this and at just the same time, carbonic acid is evolved. This separation of carbonic acid may be observed in all young plants (still depending upon the disorganization of the parent seed) when situated in the shade; and some plants exhale it at all periods of their growth when not exposed to direct sun-light.

All plants exhale carbonic acid during the night or in the entire absence of sun-light; but the amount of this gas that is absorbed and decomposed by day vastly exceeds that evolved by night. In fact, one hour or half hour of direct sunshine enables it to absorb and decompose more than has escaped from it in a whole night.

Carbonic acid gas is unquestionably the chief source of the carbon of agricultural plants. Some writers, with Liebig, consider it to be practically the exclusive means of supplying this element. Others, after Saussure and Mulder, regard the slightly soluble compounds re-resulting from the decay of vegetable matter *(humus)* in the soil, as capable of directly supplying a portion of carbon to a new generation of plants. While there is perhaps no satisfactory evidence that humus is entirely excluded from immediately nourishing vegetation, it is plain from considerations founded in the growth of forests and prairie grasses that the atmosphere, and indeed carbonic acid is now entitled to rank as the great storehouse of carbon for this purpose, as once, before humus existed, it must have been the exclusive source of this element.

From what has been already remarked with regard to the composition of the vegetable carbo-hydrates, it is seen that a certain general theoretical view of their formation in the plant may be at once gathered from the facts now set forth. In order to form the members of the cellulose group, it is only needful that the carbon retained by the cells from the carbonic acid which they decompose so readily, should enter into union with a due amount of the water that perpetually streams upward through them. By the elimination of a portion of oxygen from the water itself, we have remaining the elements that form the fats and fixed oils. To yield the vegetable acids and the pectose group, it suffices that a portion of oxygen be retained or be reabsorbed. These considerations are purely hypothetical, yet, although the real processes of decomposition and organization are in many cases vastly more complex, they possess great interest in a survey of the economy of vegetation.

For the elaboration of the albuminoids, a source of nitrogen must be present to the plant. This essential element is supplied, so far as the

atmosphere is concerned, almost entirely in the form of *ammonia*. This substance, familiar under the common name of hartshorn or spirits of hartshorn, is a compound of nitrogen and hydrogen, and is characterized by its alkaline or basic properties, having a caustic burning taste and uniting with avidity to acids, forming a large class of salts.

In the atmosphere, in presence of an excess of carbonic acid, it cannot occur in the free state, but always exists as bicarbonate of ammonia, the same form in which it usually constitutes "salt of hartshorn" or "smelling salts."

Bicarbonate of ammonia may not only occur in the solid state as a white powder, but also readily assumes the condition of a gas, as is evident from the volatile pungency of smelling salts. It is readily dissolved to a very great extent by water ; but as readily evaporates from solution again, leaving the water almost entirely free from it. For this reason its amount in the atmosphere is so variable and so small, it being removed by every shower of rain or deposition of dew and again restored by warmth and wind, or such causes as favor vaporization.

In fact it is not by examining the air itself that we gain any adequate idea of the amount of ammonia it may furnish to vegetation. We must rather look to the atmospheric waters, to dews, rains, and fogs, in order to estimate this matter rightly. In rain water (the entire fall) the quantity of ammonia is also quite variable, ranging in the country from 4 to 19 parts in ten millions; while in the rain falling in cities a 10 times larger amount has been observed.—(Boussingault, Bineau, and Way.)

In the first portions of rain or in slight showers, as well as in fog and dew, the proportion of ammonia is considerably larger. Thus, in the first 10th of a slow falling rain, Boussingault found 66 parts in 10 million of water; in dew, he found 62, and in fog 72, and in one extraordinary instance 497 parts of ammonia in 10 million of water.

Way has determined the entire amount of ammonia contained in the rain water that fell during the years 1855 and 1856 at Rothamstead, 20 miles from Linden. He found that the water which fell on an acre of surface contained in 1855, 7.11 pounds, and in 1856, 9.53 pounds of ammonia.

The evidence that ammonia is capable of absorption and assimilation by the plant is as various as it is conclusive. Numerous field experiments made with artificial ammonia-compounds, as well as the fact that all animal manures in the very process of decay, whereby they appear first to acquire their full activity, yield this body in abundance—practically establish the point; nor are there wanting more precise investigations.

Ville especially, also Chlebodarow, have shown that the addition of ammonia to the ordinary atmosphere, as well as watering with its dilute solution greatly increases the mass of vegetation produced, and makes the same much richer in albuminoids. Ville has introduced the use of ammonia into conservatories with quite striking effect, diffusing into the air of the green-house from two to four 10-thousandths of its weight of carbonate of ammonia by placing a lump of this salt upon the steam pipes that supply the space with heat.

Nitric acid, the well-known compound of nitrogen and oxygen, occurs in the atmosphere in very minute quantity, usually in the form of nitrate of ammonia. This body being incapable of existing in vapor, and readily soluble in water, is brought to the earth in dews and rains. Its quantity is even more minute than that of ammonia. The most trustworthy estimations are those of Way, who found in the waters that fell upon an acre at Rothamstead, in 1855, 2.98 pounds, and in that of 1856, 2.80 pounds of nitric acid, or about one part of nitric acid to two million parts of water.

In the soil, nitric acid often occurs in considerable quantity, (the result of chemical processes which we shall presently notice.) Here it exists in combination with various bases, usually as nitrates of lime, soda, and potash. The fertility of soils in which nitrates accumulate, and the remarkable effects of their application as fertilizers, are evidence that nitric acid feeds vegetation.

Fig. 11.
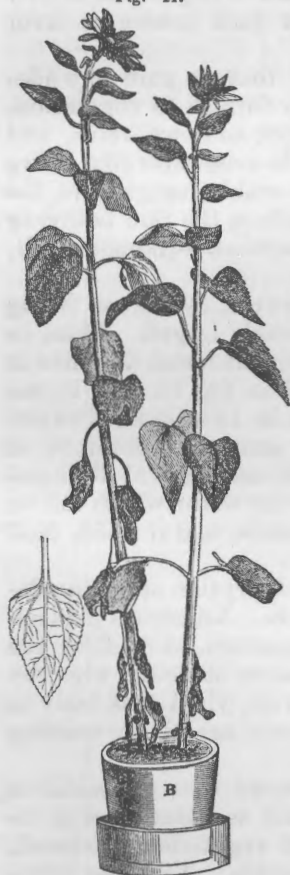
Fig. 10.
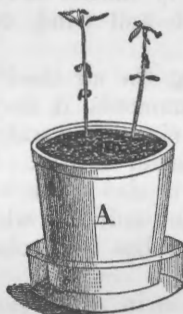
Fig. 12.
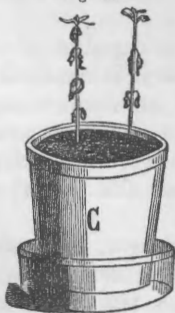
It is again to Boussingault that we owe the more careful study of its effects. Among other experiments he made the following : Two seeds of *Helianthus argophyllus* were planted in each of three pots, the soil of which, consisting of a mixture of brick-dust and sand, as well as the pots themselves, had been thoroughly freed from all nitrogenous compounds by ignition and washing with distilled water. To the soil of the pot A, fig. 10, nothing was added save the two seeds, and distilled water, with which all the plants were watered from time to time. With the soil of pot C (fig. 12) were incorporated small quantities of phosphate of lime, of ashes of clover and bicarbonate of potash, in order that the plants growing in it might have an abundant supply of all the mineral matters they needed. Finally, the soil of pot B, fig. 11, received the same mineral matters as pot C, and in addition, a small quantity of nitric acid as nitrate of potash. The seeds were sown on the 5th of July, and on the 30th September, the plants had the relative size and appearance seen in the figures, reduced to one-sixth of the natural dimensions.

This striking experiment demonstrates that nitric acid directly serves to supply nitrogen to plants. In fact, it appears to equal ammonia in its assimilability.

Liebig was formerly of the opinion that ammonia was the only form in which vegetation could be supplied with nitrogen, and that nitric acid was not appropriated by the plant until after it had become converted into ammonia in the soil. We know that under the influence of certain bodies having strong affinities for oxygen, nitric acid is transformed into ammonia, hydrogen displacing oxygen. This change was supposed to occur in the soil by virtue of the action of the carbonaceous matters (humus) there present. Now, while this may actually happen under certain circumstances, it is well ascertained that the soil and natural waters more generally contain nitrates than salts of ammonia, and the actual conversion of ammonia into nitrates in the soil has been experimentally traced.

The presence of *nitrous oxyd* in the atmosphere is not as yet directly proved, from want of a proper method of detecting it when forming but a small proportion of a gaseous mixture; but Knop has shown the probability of its occurrence there, and has proved that it may serve as a source of nitrogen to plants. What may be its significance in the actual nourishment of vegetation remains to be determined.

The important questions now arise, what are the sources of the water, carbonic acid, ammonia, and nitric acid, that exist in the atmosphere? are these minute quantities liable to exhaustion? are they sufficient to supply vegetation with carbon, hydrogen and nitrogen?

The time was—so the reasonings of geology convince us—when the soil, having scarcely cooled down from a state of fusion by fire, could contain no carbon, or at least no nitrogen, in a form capable of feeding plants. Consequently, at this period all the nitrogen, and by far the larger share of the carbon, destined to aid the growth of plants must have existed in the air; and although processes subsequently came into operation whereby portions of these substances were incorporated with the soil, the final result of natural operations is to restore them in great part to the atmosphere.

The effect of oxygen, as manifested in the processes of *decay, combustion,* and *animal nutrition,* is to bring down the vegetable organism to the inorganic level—to convert the carbo-hydrates, the albuminoids and other proximate principles of vegetation, into carbonic acid, water, ammonia, and nitric acid, the very materials out of which, under the influence of the vital principle, they were constructed.

These three varieties of chemical disorganization, which were parallel with the vital up-building of vegetation, deserve a somewhat extended notice.

Decay is a general term expressing the wasting or destruction of organic bodies under the influence of warmth, oxygen, and water.

The carbo-hydrates, when perfectly pure and dry, may be preserved indefinitely without undergoing any change. This we know in the case of cellulose, (cotton, paper,) sugar, starch, &c. In presence of a certain amount of water, and exposed to the air at a warm temperature, they undergo change; which, in case of cellulose, is very slow,

in sugar is more rapid. The oils in the pure state, as well as the organic acids, are extremely slow in alteration. The albuminoids, also, when dry may be kept at ordinary temperatures for an indefinite time with no symptoms of change. If, however, they be exposed to warm air in the moist state, they speedily undergo the process of putrefaction; they decay with great rapidity, and with the production of volatile bodies having a most intense and noisome odor.

The albuminoids are highly complex in their chemical composition; their atoms are, so to speak, delicately poised, and in a condition of unstable equilibrium, and, for this reason, liable to easy disturbance by any external agency. Hence, they at once break up into several less complex and more stable compounds when heat and oxygen act upon them with the intervention of water. Not only so, but in their fall they entangle the carbo-hydrates, the oils, the acids, and in fact all the organic constituents of the plant. In this way the soluble albuminoids act as *ferments*. Sugar dissolved in water is slow to change, until a decaying albuminoid, furnished by yeast, be added, when it is rapidly transformed into alcohol or acetic acid and carbonic acid. Butter, if carefully made, keeps sweet a long time; but if the casein of the milk be not thoroughly removed, it speedily becomes rancid, when air and warmth act upon it.

It is possible that the carbo-hydrates, if they could be *absolutely* separated and protected from matter containing nitrogen, would be found capable of perpetual preservation, even in contact with water, for in the presence of a minute quantity of some metallic salt, or other body that makes insoluble (inactive) compounds with the albuminoids, they do remain unaffected for long periods. Thus wood, which, though chiefly consisting of cellulose or other non-nitrogenous bodies, is not free from albumin, when exposed to the weather—*i. e.*, to oxygen, water, and warmth—undergoes that form of slow decay known as mouldering or humifaction, the immediate visible result of which is vegetable mould or humus. If, however, the wood be first saturated with corrosive sublimate (kyanized) or blue vitrol, it resists decay for a long time.

As it happens naturally, with very few exceptions, the organic matter of vegetation, or of animals that have subsisted upon and been formed from vegetation, falling upon the surface of the earth or buried a little way beneath it, find just the conditions of decay; and their nitrogenized ingredients yielding first to the sway of oxygen, involve with them the whole organism, so that nothing but their mineral matters, which are already oxyds, escape the destruction.

The process of decay, thus sketched in outline, includes, however, numberless intermediate stages. Thus wood in its decay yields a large series of bodies which have the collective name of humus, but are distinguished into several groups, as the humic acids, ulmic acids, and geic acids, the latter comprising crenic and apocrenic acids. These bodies all differ from wood, out of which they originated, by containing much less hydrogen and oxygen compared to carbon. We can, in fact, trace the gradual removal of these elements up to a certain point, after which other products arise from the simple oxydation of those first

formed. The fact that hydrogen is more susceptible of oxydation than carbon, to a certain extent explains the production of these bodies.

In the decay of sugar under the action of ferments there appears in an analogous manner a series of intermediate bodies, which differ in character according to the circumstances under which the fermentation is conducted.

The intermediate products of the decay of vegetable matters, wood, &c., accumulate in large masses, especially where submersion in water cuts off the free access of oxygen and keeps the temperature reduced. In this way the peat of swamps and bogs is formed, and the immense coal beds now buried in the rock strata of the earth are doubtless nothing but the peat of a former geological epoch altered in its character by further chemical agencies.

The final products of complete decay are universally the same, whatever may be the intervening stages. *The carbon of organic bodies is oxydized to carbonic acid. The hydrogen is mainly converted into water. Nitrogen unites more or less with hydrogen, forming ammonia; in part, however, it escapes in the free state. Sulphur and phosphorus are converted into sulphuric and phosphoric acids. The fixed mineral matters remain.*

Combustion, or burning, is likewise a process of oxydation. It differs from decay in the rapidity with which it occurs, and in the different intermediate products that result; but otherwise it is the same, its final issues being identical with those of decay. Combustion may, in fact, be called a quick decay, as decay has been termed by Liebig, a slow burning—*eremecausis*. It is easy to illustrate, by simple experiments, the formation of water, carbonic acid, and ammonia in the burning of organic substances.

When burning non-nitrogenous bodies, as the wax and cotton of a lighted taper, are looked upon, under ordinary circumstances, we gain the impression, indeed, that they themselves waste away, but we perceive no result of the fire except light and heat. If, however, an inverted dry glass bottle be lowered over the flame, and held so for a time, a mist presently gathers on its interior walls, and after a little drops of a liquid may be collected which are pure *water*. The bottle still being kept in the same position, we shortly see the flame becoming smaller, and its light dimmer, making it evident that something in the air which feeds the flame is being exhausted from the limited space that surrounds it. If, now, the bottle be removed, and have a little clear lime-water agitated in it, there will at once be formed a copious *precipitate*, as the chemist technically designates it, of carbonate of lime, the lime-water having served to make the invisible *carbonic acid* that resulted from the union of atmospheric oxygen with the carbon of the wax, evident to the sense of sight.

During the combustion of a nitrogenous body, in addition to water and carbonic acid, we may detect ammonia. Thus, if the smoke of a cigar be puffed against moist turmeric paper, (paper saturated with the yellow coloring principle of the turmeric or curcuma root, which is turned *brown* by alkalies,) the change of color at once shows the presence of ammonia. This body is always found in the soot of chimneys, where

wood is burned. It is collected, too, in great quanties in the gas-works of large cities, being formed from the nitrogen of the bituminous coal which is distilled and imperfectly burned in the gas retorts. The common name, spirits of hartshorn, originated in the fact of the preparation of this substance from the horn of deer, (hart.)

When combustion goes on with full access of oxygen a good deal of nitrogen escapes in the free state, the hydrogen it might unite with to form ammonia being chiefly appropriated by the more active oxygen. In proportion as the burning proceeds with a limited supply of oxygen, and at a lower temperature, more ammonia is probably formed. In presence of a fixed alkali, as potash, soda, or lime, *all* the nitrogen of organic bodies may be converted into ammonia; and it is by an ingenious use of this fact that the chemist is enabled to determine with the greatest ease and accuracy the proportions of nitrogen which organic bodies contain.

As in decay, so in combustion, it easily happens that numerous intermediate products occur, especially when the supply of oxygen is deficient. The oil, tar, smoke, and soot of ordinary fires are examples. All these substances, however, by access of more oxygen at the proper temperature, may be fully consumed into the same final products as mentioned under decay. The mineral matters of organic bodies remain in this process as ashes.

The third means of restoring to the gaseous state the elements solidified by vegetation is found in the results of the animal functions, viz: in *nutrition* and *respiration*.

The non-nitrogenous food of animals, consisting always of the vegetable carbo-hydrates, oils, &c., or of certain products of their transformation, are chiefly burned in the body by the oxygen that the lungs inhale, and the carbonic acid and water thus formed are thrown out of the system with the exhaled air. If one breathes through a tube into a glass bottle, the deposition of moisture proves the existence of water in the expired air; and by forcing the breath through lime-water the formation of the white precipitate of carbonate of lime reveals the presence of carbonic acid.

The lungs and the skin, which also constantly throw off the same substances by perspiration, are the agencies whereby the gaseous products of the oxydation of the food are restored to the atmosphere. The kidneys and lower intestines remove a portion of the waste; the former in the liquid, the latter in the solid form. A small part of the nitrogen, of which animals require constant supplies in their nutriment, is exhaled as ammonia from the lungs and skin; but as the caustic characters possessed by this body, even when in combination with carbonic acid, would not be compatible with its copious separation in the gaseous form, we must look to the liquid or solid dejections for the excretion of nitrogen. In animals we find that the kidneys dispose of this element. In the blood of man and quadrupeds there may be detected a substance which the kidneys collect and discharge from the body in large quantities through the urine. This substance, from its occurrence, is termed *urea*. When pure, it is a colorless or white body that may be procured in beautiful crys-

tals, and has a not unpleasant saline taste. In a state of purity its solution in water may be kept indefinitely without change; but in presence of a ferment, (an oxydizing albuminoid,) with which it is always naturally associated, it speedily undergoes decomposition; and by simply involving a certain amount of water in its change falls into the same substances which we have so often referred to as among the termini of vegetable and animal disorganization, viz: carbonic acid and ammonia. The following scheme illustrates this change:

$$\begin{array}{ll}
\text{One atom urea} \cdots\cdots\cdots\cdots\cdots\cdots = & C_2 \ N_2 \ H_4 \ O_2 \\
\text{Two atoms water} \cdots\cdots\cdots\cdots\cdots = & H_2 \ O_2 \\
\hline
\text{Sum} \cdots\cdots\cdots\cdots\cdots\cdots = & C_2 \ N_2 \ H_6 \ O_4
\end{array}$$

Equal to—

$$\begin{array}{ll}
\text{Two atoms carbonic acid} \cdots\cdots\cdots = & C_2 O_4 \\
\text{Two atoms ammonia} \cdots\cdots\cdots\cdots = & N_2 \ H_6 \\
\hline
\text{Sum} \cdots\cdots\cdots\cdots\cdots\cdots = & C_2 \ N_2 \ H_6 \ O_4
\end{array}$$
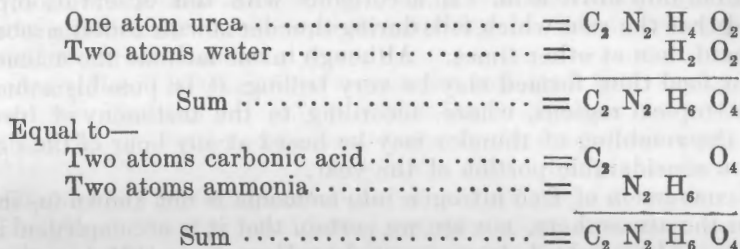
Besides urea there occurs in the urine of man, and in large quantity in that of herbivorous animals, a body containing nitrogen which bears the name *hippuric acid*. In the urine of carnivorous animals, and especially in the solid urine of birds and reptiles, is found *uric acid*. Both these substances readily undergo conversion into carbonate of ammonia.

In the solid excrement of animals are found other bodies containing nitrogen, which by decay shortly restore the same to the atmosphere.

The processes we have thus briefly noticed do not, as already intimated, *fully* and *immediately* change the organized matter of vegetables and animals back again into the substances which, according to our present knowledge, are to be regarded as the food of the plant. In the immense coal beds of former epochs and in the vast deposits of peat and sunken drift-wood that are now accumulating in marshes and river deltas, an enormous quantity of carbon and of nitrogen too, is, so far as the historical age is concerned, permanently set aside from the great circulation of matter.

What is of more agricultural importance, a large amount of nitrogen escapes in the free state into the atmosphere, and thus becomes lost to the stores of nutriment for plants. But there are other resources provided in nature's economy to maintain the requisite equilibrium.

The numerous volcanoes from which the smoke of central fires is perpetually escaping, pour daily into the atmosphere vast volumes of carbonic acid and not a little ammonia. In many regions, not in the usual sense volcanic, the earth is full of fissures that give forth unceasing streams of these gases. In the district of the Eifel, on the western shore of the Rhine, it has been estimated that 100,000 tons of carbonic acid are annually thrown into the atmosphere.

But the principal means of resupplying carbonic acid and ammonia consists in the combustion of the coal and peat that represent the

vegetation of former times, or, indeed, of pre-Adamite epochs. It it is calculated that the carbonic acid yearly produced by the consumption of bituminous and anthracite coals in Great Britain amounts to fifty millions of tons, a quantity capable of supplying carbon to seven-eighths of all the cultivated crops of that country.

The deficit of nitrogen-compounds is made up in part by electrical discharges in the atmosphere. Cavendish was the first to notice that the electric spark causes nitrogen and oxygen, in a state of mixture, to combine into nitric acid. In accordance with this observation, it is found that the rain which falls during thunder storms contains more nitric acid than at other times. Although in our latitude the amount of plant food thus formed may be very trifling, it is possibly otherwise in tropical regions, where, according to the testimony of travellers, the rumbling of thunder may be heard at any hour of the day during a considerable portion of the year.

The conversion of free nitrogen into ammonia is not known to take place in the atmosphere, nor are we certain that it is accomplished in the soil. It has, indeed, been asserted by Hermann and Mulder that, during the decay of wood, hydrogen is evolved, which, at the moment of liberation, unites itself to nitrogen with the production of ammonia, but the experiments on which this assumption was based do not now appear to be worthy of confidence.

In the soil, however, there does occur a constant formation of nitric acid, especially where lime or other alkaline bodies are present that may combine with it. It appears, indeed, that the greater share of this nitric acid results from the oxydation of the nitrogen of organic *debris*, but it is probable that the free atmospheric nitrogen is, to some extent, involved.

When electrical discharges are made to pass through the air or through pure oxygen gas, the latter shortly acquires entirely new properties. The most remarkable change it undergoes consists in its obtaining a powerful and peculiar odor, the same which is so often perceived near where lightning has struck. The oxygen, thus modified, is found to be capable of much more rapid and intense action upon other bodies than is exerted by ordinary oxygen. It at once oxydizes ammonia to nitric acid and water, and also, in presence of an alkali or lime, unites direct with free nitrogen producing a nitrate. Oxygen thus altered, and intensified in its affinities, is termed *ozone* or active oxygen, and not only is it produced by electricity, but likewise by certain processes of oxydation. When phosphorus slowly oxydizes in the air, when the oils of turpentine and bitter almonds are exposed to the atmosphere for a time, the same ozonization occurs. Schœnbein, to whose assiduous researches we owe these highly interesting facts, is of the opinion that all instances of nitrification are due to the action of ozone. Although we are not as yet able to make a probable estimate of the amount of free nitrogen that is thus oxydized in any given time, or to form any notion of the quantitative importance of the effects of this agent, we have the satisfaction of standing on a threshold which promises us an entrance into the full

understanding of the processes by which the element nitrogen is made assimilable to vegetation.

Within a few years numerous investigations relative to this subject have been made. Luca has observed that when air (freed from ammonia) is passed through a solution of potash, nitrate of potash is formed, in case the air has been in previous contact with the foliage of plants, but not otherwise; thus indicating that the oxygen is ozonized by the oxydations going on in or about living vegetation (especially when ethereal oils are exhaled?) and, according to Pless and Pierre, ozone is also produced in the decay of the organic matters of the soil.

If, as thus appears probable, it is the case that the very existence of living plants, and certain later stages of their destruction by decay, are means of recombining nitrogen to an extent equal to or slightly greater than that to which this element is placed beyond the reach of vegetable assimilation in the earlier steps of organic decomposition, we see that the vegetable germ carries with it, so far as this element is concerned, the possibility of an almost unlimited, reproduction or expansion.

Allusion has already been made to the possibility of the occurrence of nitrous oxide in the atmosphere, but we have as yet no positive evidence of the fact.

Having thus shown the origin of the compounds out of which, for the most part, the plant organizes itself, and explained, so far as the present state of science allows, how the supplies that are continually being consumed are as continually maintained, we now come properly to consider the question, Are the atmospheric stores sufficient for the purposes of vegetation?

To this inquiry we must undoubtedly reply, that while the quantity of carbonic acid absolutely contained in the atmosphere is so large as to feed an abundant vegetation, it being experimentally shown that some plants are able to grow well with none other than the ordinary atmospheric supplies, it appears that a concentration of this substance in the vicinity of the absorbing organs not only develops, but is essential to the intense growth which characterizes agricultural production.

Liebig and others have instanced forests and prairies as proving the sufficiency of the atmosphere in this respect, for, say they, under the occupancy of trees and grasses the soil is constantly enriched in carbon drawn from the atmosphere by these plants and annually deposited upon the soil as fallen foliage. In our view, however, the fact that a forest does not come into its most vigorous growth before the soil has been covered with decaying leaves, proves that the general atmosphere is insufficient, not indeed in the amount, but in the *rapidity* of its provision, and that an atmosphere more highly charged than usual with the products of vegetable decay, near or in the soil, is essential to the full supply of carbonic acid. The same doctrine must obtain with reference to the other forms of plant-food. It is, in fact, needful that the *soil* become a medium for the condensation and more speedy transmission into the plant of the originally purely atmospheric supplies. We shall recur to this subject in subsequent pages.

Closing here our study of the atmosphere considered as a source of the food of plants, we still need to remark somewhat upon the physical properties of gases in relation to vegetable life; so far, at least, as may give some idea of the means by which they gain access into the plant.

Whenever two or more gases are brought into contact in a confined space, they instantly begin to intermingle, and continue so to do until, in a short time, they are each equally diffused throughout the room they occupy or pass into a condition of *osmotic* equilibrium. If two vessels, one filled with carbonic acid, the other with hydrogen, be connected by a tube no wider than a straw, and be placed so that the heavy carbonic acid is below the fifteen times lighter hydrogen, we shall find after the lapse of a few hours that the two bodies are in a state of uniform mixture. On closer study of this phenomenon it has been discovered that gases diffuse with a rapidity proportioned to their lightness. Hence, by interposing a porous membrane between two gases of unequal density, the lighter passing more rapidly into the denser than the latter into the former, the space on one side of the membrane is overfilled while the other side is partially emptied of gas. This fact is taken advantage of for the visible illustration of the fact of gaseous diffusion.
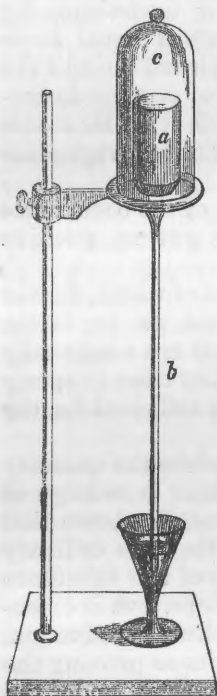
Fig. 13.

In the accompanying figure 13 is represented a long glass tube *b* widened above into a funnel, and having cemented upon this an inverted cylindrical cup of unglazed porcelain *a*. The funnel rests in a round aperture made in the horizontal arm of the support while the tube below dips beneath the surface of some water contained in the wine glass. The porous cup, funnel, and tube being occupied with common air, a glass bell *c* is filled with hydrogen gas and placed over the cup as shown in the figure. Instantly bubbles begin to escape rapidly from the bottom of the tube through the water of the wine glass, thus demonstrating that hydrogen passes into the cup faster than air can escape outward through its pores. If the bell be removed, the cup is at once bathed again externally in common air, the light hydrogen floating instantly upward, and now the water begins to rise in the tube in consequence of the return to the outer atmosphere of the hydrogen which before had diffused into the cup.

It is the perpetual action of this diffusive, or, as it is scientifically termed, *osmotic* tendency, which maintains the atmosphere in a state of such uniform mixture that accurate analyses of it give for oxygen and nitrogen almost identical figures, at all times of the day, at all seasons, all altitudes, and all situations, except near the central surface of large bodies of still water. Here the fact that oxygen is more largely absorbed by water than nitrogen diminishes by a minute amount the usual proportion of the former gas.

If into a limited volume of several gases be placed a body in the solid or liquid form, which is capable of uniting with chemically, or otherwise destroying the gaseous condition of one of the gases, it will at once absorb those particles of this gas which lie in its immediate vicinity and thus disturb the osmotic equilibrium of the remaining mixture. This equilibrium is at once restored by diffusion of a portion of the unabsorbed gas into the space that has been deprived of it and thus the absorption and the diffusion keep pace with each other until all the absorbable air is removed from the gaseous mixture and condensed or fixed in the absorbent.

In this manner a portion of the atmosphere enclosed in a large glass vessel may be perfectly freed from watery vapor and carbonic acid by a small fragment of caustic potash. A piece of phosphorus will in a few hours absorb all its oxygen, and an ignited mass of the rare metal titanium will remove its nitrogen.

A few words will now suffice to apply these facts to the absorption of the nutritive gases by vegetation.

The cells of plants are permeable to gases, as is especially manifest from what has been stated regarding the separation or evaporation of gaseous water from leaves. They too, or some portions of their contents, absorb or condense carbonic acid and ammonia in a similar way, or at least with the same effect as potash absorbs carbonic acid. As fast as these bodies are removed from the atmosphere surrounding or occupying the cells, so fast they are re-supplied by diffusion from without; so that although the quantities of gaseous plant-food contained in the air are, relatively considered, very small, they are by this grand natural law made to flow in continuous streams toward every growing vegetable cell.

LECTURE III.

THE SOIL AS RELATED TO AGRICULTURAL PRODUCTION.

No agricultural plant flourishes naturally except its roots are situated in a soil. The soil is that upon which the farmer spends his labor; the atmosphere, the weather, he cannot control. His art enables him, however, so to modify and adapt the soil that all the deficiencies of the atmosphere or the vicissitudes of climate cannot deprive him of a reward for his exertions.

The soil has a two-fold function. In the first instance, it forms the appropriate support and home of the plant, is its birthplace, the station where it runs through all the stages of its development, and the protection beneath which its roots or seeds survive the desolation of winter to gladden every spring-time with renewed growth. In the second place, it is the exclusive source of an indispensable part of the food of all agricultural plants, and the medium through which another larger share of their nutriment is accumulated and presented to them.

In nature we observe a vast variety of soils, which often differ as much in their fertility as they do in their appearance. We find large

tracts of country covered with barren, drifting sands, on whose arid bosom only a few stunted pines or shrivelled grasses find nourishment. Again there occur in the highlands of Scotland, Bavaria, Prussia, and other temperate countries, enormous stretches of moorland, bearing a nearly useless growth of heath or moss. In Southern Russia occurs a vast tract, two hundred millions of acres in extent, of the *tschornosem*, or black earth, which is remarkable for its extraordinary and persistent fertility. The prairies of our own west, the bottom lands of the Scioto and other rivers of Ohio, are other examples of peculiar soils; while on every farm, almost, may be found numerous gradations from clay to sand, from vegetable mould to gravel—gradations in color, consistence, composition, and productiveness.

Some consideration of the origin of soils is adapted to assist in understanding the reasons of their fertility. Geological studies give us reasons to believe that what is now soil was once, in chief part, solid rock. We find in nearly all soils fragments of rock, recognizable as such by the eye, and by help of the microscope it is often easy to perceive that those portions of the soil which are impalpable to the feel are only minuter grains of the same rock.

We have space for only the merest general outline of what was probably the original condition of the earth, and of the successive changes that have wrought it to its present state. During the lapse of the uncounted ages that have been forming our globe, rocks have been ground to soil, and soil has been recemented into rock, and to-day the same transformations are slowly and silently proceeding. When the earth first cooled down from the primal heat, it had no soil, in the proper sense of that word, but was a mass of crystalline granitic rocks and volcanic scoriæ, incapable of supporting vegetation. When the vapors condensed upon its surface, began that strife between fire and water which, under the mild forms we call weather, has never since ceased. Rains then began to fall upon the mountain wrinkles produced by the contraction of the cooling crust. Streams flowed downward into the valleys, cracking the still hot rock, whirling fragments along in their courses until they settled as gravel, sand, or finer powder to the bottom of some quiet sea, or were dissolved in boiling wells. In later epochs vegetation began to flourish; then, after slow centuries had passed, animal life was set in process; each department of organized existence, in its own way, adding to the list of changes. From the first the atmospheric oxygen was omnipresent, and carbonic acid too, began to act upon the rocks; and as the result of the solvent, decomposing, breaking-up, and commingling course of operations, thus carried on through long periods of continual action, we have the soil in its present characters and aspects.

The mechanical force of running water has been among the most effective agencies in the pulverization of rocks. During what is termed the diluvial or drift period, a current of water passed from north to south over the northern portion of this continent, wearing down the rocks, and bearing with it an enormous mass of solid matters, which now remain as then deposited, constituting gravelly hills, and

soils which are filled with pebbles or boulders, that were then rounded and polished in their transit from distant nothern latitudes.

Since the opening of the human epoch, lesser local floods in the waters of rivers have made numberless so-called alluvial deposits (river bottoms and deltas) in like manner.

Changes of temperature, especially the alternate freezing and thawing of water, exercise great influence in the pulverization of rocks. Water, as is well known, expands with great force in the act of freezing, and by insinuating itself while liquid into the fine cleavage rifts of rocks, and there congealing, breaks asunder the particles. The dense limestone of the Jura formation, as found in polished nodules in the soil near Munich, in Bavaria, if moistened with water and exposed to frost a single night is so disintegrated that, as the ice melts, it yields a water turbid with the loosened atoms of rock.

Oxygen exerts a perpetual disintegrating effect, by uniting with the protoxyd of iron, which occurs in nearly all rocks, setting free the acids and bases before in combination with it, and yielding peroxyd of iron. Sulphid (sulphuret) of iron is an exceedingly abundant ingredient of rocks, and, under the influence of oxygen, is readily converted into soluble sulphate of iron—a product which, in turn, reacts upon other constituents of rocks to dissolve or alter them. Carbonic acid, especially in conjunction with water, dissolves or combines with the alkalies and earths existing in rocks, and thus destroys their integrity and causes them to crumble away to soil.

The composition and chemical characters of soils depend upon the kind of rock or rocks from which they originate. A glance at the nature of these will therefore be of service to us. As to chemical ingredients, we find that the most abundant and widely diffused are precisely those which are found in the ash of plants. They mostly occur in certain definite combinations, and form the minerals, quartz, feldspar, hornblende, augite, mica, serpentine, kaolin, zeolite, carbonate of lime, carbonate of magnesia, and numerous others of less importance. The composition of specimens of these minerals is given in the annexed table. They occur, however, in very numerous varieties, and vary greatly in the kind as well as proportions of their ingredients.* It is seen from the table that many of them contain nearly all the inorganic ingredients of plants.

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* This fact may appear to stand in contradiction to the statement above made that these minerals are *definite combinations*. In the infancy of mineralogy great perplexity arose from the numberless varieties of minerals that were found—varieties that agreed together in certain characteristics, but widely differed in others. In 1830, Mitscherlich, a Prussian philosopher, discovered that a number of the elementary bodies are capable of replacing each other in combination, from the fact of their natural crystalline form being identical; they being, as he termed it, *isomorphous*, or *of like shape*. Thus, magnesia, lime, protoxyd of iron, and protoxyd of manganese; potash, soda; silica, and alumina may replace each other in such a way as to greatly affect the composition without altering the constitution of a mineral. Of the mineral hornblende, for example, there are known a great number of varieties; some pure white in color, containing, in addition to silica, magnesia and lime; others pale green, a small portion of magnesia being replaced by protoxyd of iron; others black, containing alumina in place of a portion of silica, and with oxides of iron and manganese in large proportion. All these minerals, however, admit of one expression of their constitution, for the amount of *oxygen* in the bases, no matter what they are, or what their proportions, bears a constant relation to the oxygen of the silica (and alumina) they contain, the ratio being 4 : 9.

Table of the percentage composition of Minerals.

| | Quartz. | Feldspar. | Feldspar. | Mica. | Hornblende. | Augite. | Serpentine. | Kaolin. | Pipe clay. | ZEOLITES. | |
										Natrolite.	Chabasite.
Silica..................................	100	63.70	65.21	40.00	42.24	50.90	42.50	46.80	77.03	46.94	52.02
Alumina...............................	....	23.95	18.13	12.67	13.92	5.37	1.00	37.30	14.06	27.00	17.88
Lime...................................	....	2.05	....	....	12.24	22.96	0.25	....	0.35	1.80	4.24
Magnesia..............................	....	0.65	....	15.70	13.74	14.43	38.63	....	0.47	....	....
Potash................................	....	1.20	16.66	5.61	....	....	....	2.50	1.26	....	3.03
Soda	....	8.11	....	....	....	....	....	....	....	14.70	4.07
Protoxyd of iron	....	....	....	19.03	14.59	6.25	....	....	....	....	....
Peroxyd of iron.......................	....	0.50	....	....	....	....	1.50	....	1.35	....	....
Oxyd of manganese	....	....	....	0.63	0.33	....	0.62	....	....	....	....
Water..................................	....	....	....	1.63	....	....	15.20	13.00	5.17	9.60	18.30

These minerals, while they make up the chief bulk of rocks or of the soil, are always associated with minute quantities of other compounds, such as phosphates, chlorids, sulphates, or bodies yielding sulphates, &c., upon which the geologist scarcely bestows attention, which are, however, for the scientific agriculturist of great moment.

In consequence of this wise provision and of the beneficent intermingling of the fragments of rock from widely distant regions, during the drift period and by alluvial agencies, it has resulted that, almost everywhere, there exist in the soil all those mineral bodies which are found in plants. Some one has been, indeed, so impressed with the universality of the distribution of each elementary form of matter as to offer the opinion that all the sixty simple bodies which constitute the globe might be found in every handful of soil or cup of water existing on its surface did we but possess sufficiently delicate tests.

It sometimes happens, indeed, that where a soil is *in place*, i. e., has not been transported, but lies covering the rock from which it has been formed, it is very poor and supports only a sparse vegetation, or, perhaps, is totally naked and destitute of all organic life. But these instances are comparatively rare, and their infertility is more often due to want of water, or some external cause, than to the absolute deficiency of those ingredients which are needful in a productive soil.

It often happens that a close connexion exists between the rock and the overlying soil; as often, however, the one serves as no indication to the value of the other.

The mechanical analysis of any soil separates it into portions of different fineness. A coarse sieve removes *gravel*, consisting of the larger fragments of rock ; a finer one, *coarse sand;* by washing with water, *fine sand* is left, while the turbid washings deposit after a time a quantity of impalpable matter which may consist in part of the exceedingly fine particles of rock, and in part of *clay*, or it may be entirely formed of the latter.

In most inferior soils the gravel and sand, when abundant, are angular fragments of quartz, feldspar, hornblende, augite, and mica, or of rocks consisting of these minerals. It is only these harder and less easily decomposable minerals that can resist the pulverizing agencies through which a large share of our soils have passed. In the more fertile soils, formed from secondary limestones and slates, the fragments of these stratified rocks occur as flattened pebbles and rounded grains.

The *fine portion* of the soil bears, either in quantity or composition, the most direct relation to its fertility. It is this which is capable of yielding to the growing plant the food it requires. The coarser parts of the soil are a vast store of materials in reserve for the distant future, since, by their slow disintegration, they themselves gradually become so comminuted as to serve the wants of vegetation.

Clay, which is almost invariably a chief part of the impalpable matter of the soil, has been marked by us as a mineral, and its general composition indicated in the table (p. 150.) It is a product of the action of water and carbonic acid upon such minerals as feldspar, mica, hornblende, and augite. Under the influence of these agents,

the silicates of alumina and potash, lime, &c., yield carbonates of potash, lime, &c., which dissolve and wash away, while a silicate of alumina and water, mingled with free silica, and mechanically retain· ing more or less of the other substances remains, and this is clay.

When formed from feldspar alone it is often pure white in color, and bears the name kaolin. This, the purest form of clay, is the material which constitutes the basis of porcelain. In mines, excavated through feldspathic rocks, nothing is more common than to find masses of the whitest kaolin in the fissures or cavities, which give a down-ward passage to the percolating water. The clay of ordinary soils, is, however, a material greatly admixed with other substances, and therefore exceedingly different and variable in its composition, and all the better adapted by this for its agricultural applications.

Many soils contain much carbonate of lime in an impalpable form, having been derived chiefly from the mechanical wearing down of lime rocks, as marble and chalk—from the shells of mollusks or coral branches, or, finally, being clays that have originated by the chemi-cal decomposition of feldspathic rocks containing much lime.

Organic matter, especially the *debris* of former vegetation, is almost never absent from the impalpable portion of the soil, existing there in some of the many forms assumed by the Protean humus.

From consideration of the relative proportions of the principal me-chanical ingredients has chiefly arisen the customary classification and nomenclature of soils. Silicious sand (grains of quartz, feldspar, &c.) and clay make up the chief proportion of many soils. The mixture of the two forms a loam which may be sandy (light) or clayey (heavy.) A further division is into loamy sand and loamy clay. When, in ad-dition to these, lime is present the soil is said to be a marl, either sandy, clayey, loamy, &c., according to the relative quantities of the ingredients.

Soils containing organic matter to the amount of 5 to 10 per cent. are termed vegetable moulds; if this ingredient exceeds 10 per cent., which rarely occurs, except in wet situations, we have a *peaty* soil.

If coarse rounded fragments of rock be present in large quantity the soil is *gravelly*. Where much oxyd of iron exists, as evinced by a red or yellow color, the soil is *ochery*. The epithets peaty, gravelly, ochery, come then, in many cases, to further modify the designations of sands, clays, marls, and moulds.

Other divisions are current among practical men, as, for example, surface and sub-soil, active and inert soil, tilth, and hard pan. These terms mostly explain themselves. When, at the depth of four inches to one foot or more, the soil assumes a different color and texture, these distinctions have meaning. The surface soil, active soil, or tilth, is the portion that is wrought by the instruments of tillage—that which is moistened by the rains, warmed by the sun, permeated by the atmosphere, in which the plant extends its roots, gathers its soil food, and which, by the decay of the subterranean organs of vegetation, acquires a content of humus. Where the soil originally had the same characters to a great depth, it often becomes modified down to a certain point, by the agencies just enumerated, in such a

manner that the eye at once makes the distinction into surface-soil and sub-soil. In many soils, however, such distinctions are entirely arbitrary, the earth changing its appearance gradually or even remaining uniform to a considerable depth.

Again, the surface soil may have a greater downward extent than the active soil, or the tilth may extend into the sub-soil.

Hard-pan is the appropriate name of a dense, almost impenetrable, crust or stratum of ochery clay or compacted gravel, often underlying a fairly fruitful soil. It is the soil reverting to rock. The particles once disjointed are being cemented together again by the solutions of lime, iron or alkali-silicates that descend from the surface soil. Such a stratum often separates the surface soil from a deep gravel bed, and peat swamps thus exist in basins formed on the most porous soils by a thin layer of moor-bed-pan.

With these general notions regarding the origin and characters of soils, we may proceed to a somewhat extended notice of the properties of the soil as influencing fertility. These divide themselves into *physical characters*—those which externally affect the growth of the plant; and *chemical characters*—those which provide it with food.

Among the physical characters* we first notice *the state of division* in which the soil is found.

On the surface of a block of granite only a few lichens and mosses can exist; crush the block to a coarse powder and a more abundant vegetation can be supported on it; if it is reduced to a very fine dust and duly watered, even the cereal grains will grow and perfect fruit on it. Thus two soils may have the same chemical composition, and yet one be almost inexhaustibly fertile, and the other almost hopelessly barren. There are sandy soils in the Eastern States, which, without manure, yield only the most meagre crops of rye or buckwheat; and there are sandy soils in Ohio, which, without manure, yield on an average 80 bushels of Indian corn per acre, and have yielded this for twenty to fifty years in unbroken succession. According to David A. Wells, (American Journal of Science, July, 1852,) these two kinds of soil yield very similar, practically identical, results on chemical analysis, so far as their inorganic ingredients are concerned. What is the cause of the difference of fertility? Our present knowledge can point to no other explanation than is furnished by the different fineness of the particles. The barren sandy soils consist in great part of coarse grains, while the Ohio soil is an exceedingly fine powder.

It is true, as a general rule, that all fertile soils contain a large proportion of very fine or impalpable matter. How the extreme division of the particles of the soil is connected with its fertility is not difficult to understand. The food of the plant must enter it in a state of solution, or if undissolved, the particles must be smaller

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* In treating of the physical characters of the soil, the writer employs an essay on this subject, contributed by him to vol. XVI of the Transactions of the N. Y. State Agricultural Society.

than we can discover with the best optical aids, because the pores of
the roots of plants are not discernible by any microscope. The
mineral matters of the soil must be dissolved or diffused in water.
The rapidity of their solution is in direct proportion to the extent of
their surface. The finer the particles, the more abundantly will the
plant be supplied with its necessary nourishment. In the Scioto
valley soils the water which is transpired by the crops comes in con-
tact with such an extent of surface that it is able to dissolve the soil-
ingredients in as large quantity and as rapidly as the crop requires.
In the coarse-grained soils this is not the case. Soluble matters
(manures) must be applied to them by the farmer, or his crops refuse
to yield handsomely.

It is furthermore obvious, that, other things being equal, the
finer the particles of the soil the more space the growing roots have
in which to expand themselves, and the more numerously are they
able to present their absorbent surfaces to the supplies which the
soil contains.

It will presently appear that other very important properties of the
soil are more or less related to its state of mechanical division.

The soil has, secondly, *a power of withdrawing from the air vapor of
water and condensing the same in its pores.* It is, in other words,
hygroscopic.

This property of a soil is of the utmost agricultural importance,
because, 1st, it is connected with the permanent moisture which is
necessary to vegetable existence, and, 2d, since the absorption of
water-vapor determines the absorption of other vapors and gases.

In the following table from Schübler we have the results of a
series of experiments carried out by that philosopher for the pur-
pose of determining the absorptive power of different kinds of earths
and soils.

The column of figures gives in thousandths the quantity of moisture
absorbed by the previously dried soil, under the same circumstances,
in twenty-four hours:

Quartz sand, coarse ·   0
Gypsum ·   1
Lime sand ·   3
Plough land ·   23
Clay soil, (60 per cent. clay) ·   28
Slaty marl ·   33
Loam ·   35
Fine carbonate of lime ·   35
Heavy clay soil, (80 per cent. clay) ·   41
Garden mould, (7 per cent. humus) ·   52
Pure clay ·   49
Carbonate of magnesia, (fine powder) · · · · · · · · · · · · · · · · · · ·   82
Humus · 120

An obvious practical result follows from the facts expressed in the
above table, viz: that sandy soils which have little attractive force
for watery vapor, and are therefore dry and arid, may be meliorated
in this respect by admixture with clay, or better with humus, as

is done by green manuring. The table gives us proof that gypsum does not exert any beneficial action in consequence of directly attracting moisture. Humus, or decaying vegetable matter, it will be seen, surpasses every other ingredient of the soil in absorbing moisture. This is doubtless in some degree connected with its extraordinary porosity or amount of surface. How the extent of surface alone may act is made evident by comparing the absorbent power of carbonate of lime in the two states of sand and of an impalpable powder. The latter it is seen, absorbed twelve times as much vapor of water as the former. Carbonate of magnesia stands next to humus, and it is worthy of note that it is a very light and fine powder.

Finally, it is a matter of observation that "silica and lime in the form of coarse sand make the soil in which they predominate so dry and hot that vegetation perishes from want of moisture; when, however, they occur as fine dust, they form too wet a soil, in which plants perish from the opposite cause."—(*Hamm's Landwirthschaft.*)

In the fact that soils have a physical absorbing power for the vapor of water, we have an illustration of a general principle, viz: *That the surfaces of liquid and solid matter attract the particles of other kinds of matter.* In the same way that water is absorbed, oxygen gas is condensed, especially in certain highly porous bodies. Platinum, copper, lead, and iron, when in the state of fine sponge, exert a remarkable condensing power on oxygen, and it is probable that thereby this element is ozonized. Platinum sponge exhibits the characters of a body charged with ozone, and it is to be anticipated that investigation will shortly demonstrate the occurrence of gaseous condensations in the soil, the effect of which is to produce chemical changes of the most important character. It is not unlikely that the organic matters of the soil, which possess the extremest porosity may thereby acquire their power of ozonizing the oxygen which combines so readily with them, and thus accomplish the formation of nitric acid from atmospheric nitrogen.

Of exceeding influence on the fertility of the soil is, thirdly, its *permeability to liquid water.*

A soil is permeable to water when it allows that liquid to soak into or run through it. To be permeable is of course to be porous. On the size of the pores depends its degree of permeability. Coarse sands, and soils which have *few* but *large* pores or interspaces, allow water to run through them readily—water *percolates* them. When, instead of running through, the water is largely absorbed and held by the soil, the latter is said to possess great *capillary power;* such a soil has *many* and *minute* pores. The cause of capillarity is the same surface attraction which has been already mentioned.

When a narrow vial is partly filled with water, it will be seen that the liquid adheres to its sides, and if it be not more than one-half inch in diameter, the surface of the liquid will be curved or concave. In a very narrow tube the liquid will rise to a considerable height. In these cases the surface attraction of the glass for the water neutralizes or overcomes the weight of (earth's attraction for) the latter.

The pores of a sponge raise and hold water in them, in the same

way that these narrow (capillary*) tubes support it. When a body has
pores so fine (surfaces so near each other) that their surface attrac-
tion is greater than the gravitating tendency of water, then the body
will suck up and hold water—will exhibit capillarity; a lump of salt or
sugar, a lamp-wick, are familiar examples. When the pores of a
body are so large (the surfaces so distant) that they cannot fill them-
selves or keep themselves full, the body allows the water to run
through or to *percolate*.

Sand is most easily permeable to water, and to a higher degree
the coarser its particles. Clay, on the other hand, is the least pene-
trable, and the less so the purer and more plastic it is.

When a soil is too coarsely porous it is said to be leachy or
hungry. The rains that fall upon it quickly soak through, and it
shortly becomes dry. On such a soil, the manures that may be ap-
plied in the spring are to some degree washed down below the
reach of vegetation, and in the droughts of summer plants suffer and
perish from want of moisture.

When the texture of a soil is too fine, its pores too small, as happens
in a heavy clay, the rains penetrate it too slowly; they flow off the
surface, if the latter be inclined, or remain as pools for days and even
weeks in the hollows. In a soil of proper texture the rains neither
soak off into the under earth nor stagnate on the surface, but the soil
always (except in excessive wet or drought) maintains the moistness
which is salutary to most of our cultivated plants.

The part which the *capillarity* of the soil plays in the nutrition of
the plant deserves a moment's notice.

If a wick be put into a lamp containing oil, the oil, by capillary
action, gradually permeates its whole length, that which is above as
well as that below the surface of the liquid. When the lamp is set
burning, the oil at the flame is consumed, and as each particle disap-
pears its place is supplied by a new one, until the lamp is empty or
the flame extinquished.

Something quite analogous occurs in the soil, by which the plant
(corresponding to the flame in our illustration) is fed. The soil is at
once lamp and wick, and the *water of the soil* represents the oil. Let
evaporation of water from the surface of the soil or of the plant take
place of the combustion of the oil from a wick and the matter stands
thus: Let us suppose dew or rain to have saturated the ground with
moisture for some depth. On recurrence of a dry atmosphere with
sunshine and wind, the surface of the soil rapidly dries; but as each
particle of water escapes (by evaporation) into the atmosphere, its
place is supplied (by capillarity) from the stores below. The ascending
water brings along with it the soluble matters of the soil, and thus the
roots of plants are situated in a stream of their appropriate food. The
movement proceeds in this way so long as the surface is dryer than
the deeper soil. When, by rain or otherwise, the surface is saturated,
it is like letting a thin stream of oil run upon the apex of the lamp-

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* From *capillus* the Latin word for hair, because as fine as hair; (but a hair is no tube, as
is often supposed.)

wick—no more evaporation into the air can occur, and consequently there is no longer any ascent of water; on the contrary, the water, by its own weight, penetrates the soil, and if the underlying ground be not saturated with moisture, as can happen where the subterranean fountains yield a meagre supply, then capillarity will aid gravity in its downward distribution.

The most rational conclusion from all the facts at our command is that all the mineral matters, as well as a portion of the organic bodies, which feed the plant, are carried into it by water. So long as evaporation goes on from the surface of the soil, so long there is a constant upward flow of saline matters. Those portions which do not enter vegetation accumulate on or near the surface of the ground; when a rain falls, they are washed down again to a certain depth, and thus are kept constantly changing their place with the water, which is the vehicle of their distribution. In regions where rain falls periodically or not at all, this upward flow of the soil-water often causes an accumulation of salts on the surface of the ground. Thus in Bengal many soils which in the wet season produce the most luxuriant crops, during the rainless portion of the year become covered with white crusts of saltpetre. Doubtless the beds of nitrate of soda that are found in Peru have accumulated in the same manner. So in our western caves the earth sheltered from rains is saturated with salts—epsom salts, glauber salts, and saltpetre, or mixtures of these. Often the rich soil of gardens is slightly incrusted in this manner in our summer weather; but the saline matters are carried into the soil with the next rain.

It is easy to see how, in a good soil, capillarity thus acts in keeping the roots of plants constantly immersed in a stream of water or moisture that is now ascending, now descending, but never at rest, and how the food of the plant is thus made to circulate around the organs fitted for absorbing it.

The same causes that maintain this perpetual supply of water and food to the plant are also efficacious in constantly preparing new supplies of food. As before explained, the materials of the soil are always undergoing decomposition, whereby the silica, lime, phosphoric acid, potash, &c., of the insoluble fragments of rock, become soluble in water and accessible to the plant. Water charged with carbonic acid and oxygen, is the chief agent in these chemical changes. The more extensive and rapid the circulation of water in the soil, the more matters will be rendered soluble in a given time, and, other things being equal, the less will the soil be dependent on manures to keep up its fertility.

No matter how favorable the structure of the soil may be to the circulation of water in it, no continuous upward movement can take place without evaporation. The ease and rapidity of evaporation, while mainly depending on the condition of the atmosphere and on the sun's heat, are to a certain degree influenced by the soil itself. We have already seen that the soil possesses a power of absorbing watery vapor from the atmosphere, a power which is related both to

the kind of material that forms the soil and to its state of division.
This absorptive power opposes evaporation. Again, different soils
manifest widely different capacities for imbibing liquid water—capa-
cities mainly connected with their porosity. Obviously too, the
quantity of liquid in a given volume of soil affects not only the
rapidity, but also the duration of evaporation.

The following tables by Schübler illustrate the peculiarities of dif-
ferent soils in these respects. The first column gives the *per cents* of
water absorbed by the completely dry soil. In these experiments the
soils were thoroughly wet with water, the excess allowed to drip off,
and the increase of weight determined. In the second column are
given the *per cents* of water that evaporated during the space of one
hour from the saturated soil spread over a given surface:

Quartz sand	25	88.4
Gypsum	27	71.7
Lime sand	29	75.9
Slaty marl	34	68.0
Clay soil, (sixty per cent. clay,)	40	52.0
Loam	51	45.7
Plough land	52	32.0
Heavy clay, (eighty per cent. clay,)	61	34.9
Pure gray clay	70	31.9
Fine carbonate of lime	85	28.0
Garden mould	89	24.3
Humus	181	25.5
Fine carbonate of magnesia	256	10.8

It is obvious that these two columns express nearly the same thing
in different ways. The amount of water retained increases from quartz
sand to magnesia. The rapidity of drying in the air, diminishes in the
same direction.

The want of retentive power for water in the case of coarse sand is
undeniably one of the chief reasons of its unfruitfulness. The best
soils possess a medium retentive power. In them, therefore, are best
united the conditions for the regular distribution of the soil-water under
all circumstances. In them this process is not hindered too much either
by wet or dry weather. The retaining power of humus is seen to be
more than double that of clay. This result might appear at first sight
to be in contradiction to ordinary observations, for we are accustomed
to see water standing on the surface of clay but not on humus. It
must be borne in mind that clay, from its imperviousness, holds water
like a vessel, the water remaining apparent; but humus retains it
invisibly, its action being nearly like that of a sponge.

One chief cause of the value of a layer of humus on the surface of
the soil doubtless consists in this great retaining power for water, and
the success that has attended the practice of green manuring, as a
means of renovating almost worthless shifting sands, is in a great
degree to be attributed to this cause. The advantages of mulching
are explained in the same way.

The relations of the soil to heat are of the utmost importance in affecting its fertility. The distribution of plants in general, is determined by differences of mean temperature. In the same climate and locality, however, we find the farmer distinguishing between cold and warm soils.

The temperature of the soil varies to a certain depth with that of the air; yet its changes occur more slowly, are confined to a narrower range of temperature, and diminish downward in rapidity and amount, until at a certain depth a point is reached where the temperature is invariable.

In summer the temperature of the soil is higher in day time than that of the air; at night the temperature of the surface rapidly falls, especially when the sky is clear.

In temperate climates, at a depth of three feet, the temperature remains unchanged from day to night; at a depth of 20 feet the annual temperature varies but a degree or two; at 75 feet below the surface, the thermometer remains perfectly stationary. In the vaults of the Paris Observatory, 80 feet deep, the temperature is 50° Fahrenheit. In tropical regions the point of nearly unvarying temperature is reached at a depth of one foot.

The mean annual temperature of the soil is the same as, or in higher latitudes a degree above, that of the air. The nature and position of the soil must considerably influence its temperature.

The *sources* of that heat which is found in the soil are two, viz: first, an internal one, the chemical process of oxydation or decay; second, an external one, the rays of the sun.

The heat evolved by the decay of organic matters is not inconsiderable in porous soils containing much vegetable remains; but this decay cannot proceed rapidly until the external temperature has reached a point favorable to vegetation, and therefore this source of heat probably has no appreciable effect one way or the other on the welfare of the plant. The warmth of the soil, so far as it favors vegetable growth, appears then to depend exclusively on the heat of the sun.

The earth has within itself a source of heat, which maintains its interior at a high temperature; but which escapes so rapidly from the surface that the soil would be constantly frozen but for the external supply of heat from the sun.

The direct rays of the sun are the immediate cause of the warmth of the earth's surface. The temperature of the soil near the surface changes progressively with the season; but at a certain depth the loss from the interior and the gain from the sun compensate each other, and, as has been previously mentioned, the temperature remains unchanged throughout the year.

During a summer day the heat of the sun reaches the earth directly, and it is absorbed by the soil and the solid objects on its surface, and also by the air and water. But these different bodies, and also the different kinds of soil, have very different ability to absorb or become warmed by the sun's heat. Air and water are almost incapable of being warmed by heat applied above them. Through

the air especially, heat radiates without being scarcely absorbed. The soil and solid bodies become warmed according to their individual capacity, and from them the air receives the heat which warms it. From the moist surface of the soil goes on a rapid evaporation, which renders latent* a large amount of heat, so that the temperature of the soil is not rapidly but gradually elevated. The ascent of water from the sub-soil to supply the place of that evaporated, goes on as before described. The liquid water of the soil has combined with (rendered latent) a vast amount of heat therefrom, and passed as gaseous water (vapor) into the air. When the sun declines, the process diminishes in intensity, and when it sets, the reverse takes place. The heat that had accumulated on the surface of the earth radiates into the cooler atmosphere and planetary space, the temperature of the surface rapidly diminishes, and the air itself becomes cooler by convection.† As the cooling goes on, the vapor suspended in the atmosphere begins to condense upon cool objects, while its latent heat becoming free hinders the too sudden reduction of temperature. The condensed water collects in drops—it is dew; or in the colder seasons it crystallizes as hoar-frost.

The special nature of the surface of the soil is closely connected with the maintenance of a uniform temperature, with the prevention

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* When a piece of ice is placed in a vessel whose temperature is increasing, by means of a lamp, at the rate of one degree of the thermometer every minute, it will be found that the temperature of the ice rises until it attains 32°. When this point is reached, it begins to melt, but does not suddenly become fluid: the melting goes on very gradually. A thermometer placed in the water remains constantly at 32° so long as a fragment of ice is present. The moment the ice disappears, the temperature begins to rise again, at the rate of one degree per minute. The time during which the temperature of the ice and water remains at 32° is 140 minutes. During each of these minutes one degree of heat enters the mixture, but is not indicated by the thermometer—the mercury remains stationary; 140° of heat have thus passed into the ice and become hidden, *latent;* at the same time the solid ice has become liquid water. The difference, then, between ice and water consists in the heat that is latent in the latter. If we now proceed with the above experiment, allowing the heat to increase with the same rapidity, we find that the temperature of the water rises constantly for 180 minutes. The thermometer then indicates a temperature of 212, (32+180,) and the water boils. Proceeding with the experiment, the water evaporates away, but the thermometer continues stationary so long as any liquid remains. After the lapse of 972 minutes, it is completely evaporated. Water in becoming steam renders, therefore, still another portion, 972°, of heat latent. The heat latent in steam is indispensable to the existence of the latter. If this heat be removed by bringing the steam into a cold space, water is reproduced. If, by means of pressure or cold, steam be condensed, the heat originally latent in it becomes sensible, *free,* and capable of affecting the thermometer. If, also, water be converted into ice, as much heat is evolved and made sensible as was absorbed and made latent. It is seen thus that the processes of liquefaction and vaporization are *cooling* processes; for the heat rendered latent by them must be derived from surrounding objects, and thus these become cooled. On the contrary, solidification, freezing, and vapor-condensation are *warming* processes, since in them large quantities of heat cease to be latent and are made sensible, thus warming surrounding bodies.

† Though liquids and gases are almost perfect non-conductors of heat, yet it can diffuse through them rapidly, if advantage be taken of the fact that by heating they expand and therefore become specifically lighter. If heat be applied to the upper surface of liquids or gases, they remain for a long time nearly unaffected; if it be applied *beneath* them, the lower layers of particles become heated and rise, their place is supplied by others, and so currents upward and downward are established, whereby the heat is rapidly and uniformly distributed. This process of *convection* can rarely have any influence *in* the soil. What we have stated concerning it shows, however, in what way the atmosphere may constantly act in removing heat from the surface of the soil.

of too great heat by day and cold by night, and with the watering or
vegetation by means of dew. It is, however, in many cases only for
a little space after seed-time that the soil is greatly concerned in
these processes. So soon as it becomes covered with vegetation the
character of the latter determines to a certain degree the nature of
the atmospheric changes. In case of many crops the soil is but par-
tially covered, and its peculiarities are then of direct influence on the
vegetation it bears. Among these qualities the following may be
noticed:

1. *The color of the soil.*—It is usually stated that black or dark co-
lored soils are sooner warmed by the sun's rays than those of lighter
color, and remain constantly of a higher temperature so long as the
sun acts on them. An elevation of several degrees in the tempera-
ture of a light colored soil may be caused by strewing its surface
with peat, charcoal powder, or vegetable mould. To this influence may
be partly ascribed the following facts. Lampadius was able to ripen
melons even in the coolest summers in Friberg, Saxony, by strewing
a coating of coal dust an inch deep over the surface of the soil. In
Belgium and on the Rhine, it is found that the grape matures best
when the soil is covered with fragments of black clay slate. Girar-
din found in a series of experiments on the cultivation of potatoes,
that the time of their ripening varied eight to fourteen days, accord-
ing to the color of the soil. He found on August 25th, in a very dark
humus soil, twenty-six varieties ripe; in sandy soil, twenty; in clay,
nineteen; and in white lime soil, only sixteen. It is not difficult to
assign other causes that will account in part for the results here men-
tioned; and although it has been observed that dark soils range from
three to eight degrees higher in temperature than contiguous soils
having a lighter color, it is not to color so much as to other qualities
that the soil owes its peculiar temperature, as is proved by the recent
observations of Malaguti and Durocher. They found that the tem-
perature of a garden soil, just below the surface, was on the average
6° Fahrenheit higher than that of the air, but that this higher tem-
perature diminished at a greater depth. A thermometer buried four
inches indicated a mean temperature only 3° above that of the atmos-
phere. Besides the garden earth, just mentioned, which had a dark
gray color, and was a mixture of sand and gravel containing but little
clay, with about five per cent. humus, the thermometric characters
of the following soils were observed, viz: a grayish-white quartz
sand; a grayish-brown granite sand; a fine light-gray clay (pipe clay;)
a yellow sandy clay; and, finally, four lime soils of different physical
qualities.

It was found that when the exposure was alike, the dark-gray
granite sand became the warmest, and next to this the grayish-white
quartz sand. The latter, notwithstanding its lighter color, often ac-
quired a higher temperature when at a depth of four inches than the
former, a fact to be ascribed to its better conducting power. The
black soils never became so warm as the two just mentioned, demon-
strating that color does not influence the absorption of heat so much
as other qualities. After the black soils, the others came in the fol-

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lowing order: Garden soil; yellow sandy clay; pipe clay; lime soils having crystalline grains; and, lastly, a pulverulent chalk soil.

To show what different degrees of warmth soils may acquire, under the same circumstances, the following maximum temperatures may be adduced: At noon of a July day, when the temperature of the air was 90°, a thermometer placed at a depth of a little more than one inch, gave these results:

In quartz sand ·· 126°
In crystalline lime soil ·· 115°
In garden soil ·· 114°
In yellow sandy clay ··· 100°
In pipe clay ··· 94°
In chalk soil ·· 87°

Here we observe a difference of nearly 40° in the temperature of the coarse quartz and the chalk soil. The experimenters do not mention the influence of water in affecting these results; they do not state the degree of dryness of these soils. It will be seen, however, that the warmest soils are those that retain least water, and doubtless something of the slowness with which the fine soils increase in warmth is connected with the fact that they retain much water, which, in evaporating, appropriates and renders latent a large quantity of heat.

The chalk soil is seen to be the coolest of all, its temperature in these observations being three degrees lower than that of the atmosphere at noon day. In hot climates this coolness is sometimes of great advantage as appears to happen in Spain, near Cadiz, where the Sherry vineyards flourish. "The Don said the Sherry wine district was very small, not more than twelve miles square. The sherry grape grew only on certain low chalky hills where the earth being light-colored, is not so much burnt; did not chap and split so much by the sun as darker and heavier soils do. A mile beyond these hills the grape deteriorates."—(*Dickens' Household Words, November* 13, 1858.)

In explanation of these observations we must recall to mind the fact that all bodies are capable of absorbing and radiating as well as reflecting heat. These properties, although never disassociated from color, are not necessarily dependent upon it. They chiefly depend upon the character of the surface of bodies. Smooth polished surfaces absorb and radiate heat least readily; they reflect it most perfectly. Radiation and absorption are opposed to each other, and the power of any body to radiate is precisely equal to its faculty of absorbing heat. It must be understood, however, that bodies may differ in their power of absorbing or radiating *heat of different degrees of intensity*. Lampblack absorbs and radiates heat of all intensities in the same degree. White lead absorbs heat of low intensity (such as radiates from a vessel filled with boiling water) as fully as lampblack, but of the intense heat of a lamp it absorbs only about one-half as much. Snow seems to resemble white-lead in this respect. If a black cloth or black paper be spread on the surface of snow, upon which the sun si shining, it will melt much faster under the cloth than elsewhere, and this too if the cloth be not in contact with, but suspended above

the snow. In our latitude every one has had opportunity to observe that snow thaws most rapidly when covered by or lying on black earth. The reason is that snow absorbs heat of low intensity with greatest facility. The heat of the sun is converted from a high to a low intensity by being absorbed and then radiated by the black material. But it is not color that determines this difference of absorptive power, for indigo and Prussian blue, though of nearly the same color, have very different absorptive powers. So far, however, as our observations extend, it appears that usually, dark colored soils absorb heat most rapidly, and that the sun's rays have least effect on light colored soils.

2. *The degree of moisture present* is of great influence on the temperature of the soil. All soils when thoroughly wet seem to be nearly alike in their power of absorbing and retaining warmth. The vast quantity of heat needful to gratify the demand of the vapor that is constantly forming, explains this. From this cause the difference in temperature between dry and wet soil may often amount from 10° to 18°. According to the observation of Dickinson, made at Abbot's Hill, Hertfordshire, England, and continued through eight years, 90 per cent. of the water falling between April 1st and October 1st, evaporates from the surface of the soil, only 10 per cent. finding its way into drains laid three and four feet deep. The total quantity of water that fell during this time, amounted to about 2,900,000 lbs. per acre; of this more than 2,600,000 evaporated from the surface. It has been calculated that to evaporate artificially this enormous mass of water, more than seventy-five tons of coal must be consumed.

Thorough draining, by loosening the soil and causing a rapid removal from below of the surplus water, has a most decided influence, especially in spring time, in warming the soil and bringing it into a suitable condition for the support of vegetation.

It is plain then that even if we knew with accuracy what are the physical characters of a surface soil, and if we were able to estimate correctly the influence of these characters on its fertility, still we must investigate those circumstances which affect its wetness or dryness, whether they be an impervious sub-soil, or springs coming to the surface, or the amount and frequency of rain-falls, taken in connexion with other meteorological causes. We cannot decide that a clay is too wet or a sand too dry, until we know its situation and the climate it is subjected to.

The great deserts of the globe do not owe their barrenness to necessary poverty of soil, but to meteorological influences—to the continued prevalence of parching winds, and the absence of mountains to condense the atmospheric water and establish a system of rivers and streams. This is not the place to enter into a discussion of the causes that may determine or modify climate, but to illustrate the effect that may be produced by means within human control, it may be stated that previous to the year 1821, the French district Provence was a fertile and well-watered region. In 1822, the olive trees which were largely cultivated there were injured by frost, and the inhabitants began to cut them up root and branch. This amounted to clearing off a forest,

and in consequence the streams dried up, and the productiveness of the country was seriously diminished.

3. *The angle at which the sun's rays strike a soil* is of great influence on its temperature. The more this approaches a right angle the greater the heating effect. In the latitude of England the sun's heat acts most powerfully on surfaces having a southern exposure, and which are inclined at an angle of 25° and 30°. The best vineyards of the Rhine and Neckar are also on hill-sides, so situated. In Lapland and Spitzbergen the southern side of hills are often seen covered with vegetation, while lasting or even perpetual snow lies on their northern inclinations.

4. *The influence of a wall or other reflecting surface* upon the warmth of a soil lying to the south of it, was observed in the case of garden soil by Malaguti and Durocher. The highest temperature indicated by a thermometer placed in this soil at a distance of six inches from the wall, during a series of observations lasting seven days, (April, 1852,) was 32° Fahrenheit higher at the surface, and 18° higher at a depth of four inches than in the same soil on the north side of the wall. The average temperature of the former during this time was 8° higher than that of the latter.

In the Rhine district grape vines are kept low and as near the soil as possible, so that the heat of the sun be reflected back upon them from the ground, and the ripening is then carried through the nights by the heat radiated from the earth.—*(Journal Highland and Agricultural Society,* July 1858, *p.* 347.)

5. Malaguti and Durocher also studied the effect of a *sod* on the temperature of the soil. They observed that it hindered the warming of the soil, and indeed to about the same extent as a layer of earth of three inches depth. Thus a thermometer four inches deep in greensward acquires the same temperature as one seven inches deep in the same soil not grassed.

It is to be remembered that the soils that warm most quickly, also cool correspondingly fast, and thus are subjected to the most extensive and rapid changes of temperature. The greensward which warms slowly, retains its warmth most tenaciously, and the sands that become hottest at noon-day, are coldest at midnight.

Of no little practical importance is the *shrinking of soils on drying.*— This shrinking is of course offset by an increase of bulk when the soil becomes wet. In variable weather we have therefore constant changes of volume occurring. Soils rich in humus experience these changes to the greatest degree. The surfaces of moors often rise and fall with the wet or dry season, through a space of several inches. In ordinary light soils containing but little humus no change of bulk is evident. Otherwise, it is in clay soils that shrinking is most perceptible; since these soils only dry superficially they do not appear to settle much, but become full of cracks and rifts. Heavy clays may lose one-tenth or more of their volume on drying, and since at the same time they harden about the rootlets which are imbedded in them, it is plain that these indispensable organs of the plant must thereby e ruptured during the protracted dry weather. Sand, on the other

hand, does not change its bulk by wetting or drying, and when present to a considerable extent in the soil, its particles being interposed between those of the clay, prevent the adhesion of the latter, so that, although a sandy loam shrinks not inconsiderably on drying, yet the lines of separation are vastly more numerous and less wide than in purer clays. Such a soil does not "cake," but remains friable and powdery.

Marly soils (containing carbonate of lime) are especially prone to fall to a fine powder during drying, since the carbonate of lime, which like sand, shrinks very little, is itself in a state of extreme division, and therefore more effectually separates the clayey particles. The unequal shrinking of these two intimately mixed ingredients accomplishes a perfect pulverization of such soils. Professor Wolff, of the Academy of Agriculture, at Hohenheim, Wirtemberg, states that on the cold heavy soils of Upper Lusatia, in Germany, the application of lime has been attended with excellent results, and he thinks that the larger share of the benefit is to be accounted for by the improvement in the texture of those soils which follows liming. The carbonate of lime is considerably soluble in water charged with carbonic acid, as is the water of a soil containing vegetable matter, and this agency of distribution in connection with the mechanical operations of tillage, must in a short time effect an intimate mixture of the lime with the whole soil. A tenacious clay is thus by a heavy liming made to approach the condition of a friable marl.

We may give a moment's notice to the *cohesiveness of the soil.*— A soil is said to be heavy or light, not as it weighs more or less, but as it is easy or difficult to work. The *state of dryness* has great influence on this quality. Sand, lime, and humus have very little cohesion when dry, but considerable when wet. Soils in which they predominate are usually easy to work. But clay has entirely different characters, and upon them almost exclusively depends the tenacity of a soil. Dry clay, when powdered, has hardly more consistence than sand, but when thoroughly moistened its particles adhere together to a soft and plastic, but tenacious mass; and in drying away, at a certain point it becomes very hard, and requires a good deal of force to penetrate it. In this condition it offers great resistance to the instruments used in tillage, and when thrown up by the plough it forms lumps which require repeated harrowings to break them down. Since the cohesiveness of the soil depends so greatly upon the quantity of water contained in it, it follows that thorough draining, combined with deep tillage, whereby sooner or later the stiffest clays become readily permeable to water, must have the best effects in making such soils easy to work.

The English practice of burning clays speedily accomplishes the same purpose. When clay is burned and then crushed the particles no longer adhere tenaciously together on moistening, and the mass does not acquire again the unctuous plasticity peculiar to unburned clay.

Mixing sand with clay, or incorporating vegetable matter with it,

serves to separate the particles from each other, and thus remedies too great cohesiveness.

When water freezes its volume increases, as is well known. The alternate freezing and thawing of the water which impregnates the soil during the colder part of the year plays thus an important part in overcoming its cohesion. The effect is mostly apparent in the spring, immediately after "the frost leaves the ground," but is usually not durable, the soil recovering its former consistence by the operations of tillage. Fall-ploughing of stiff soils has been recommended, in order to expose them to the disintegrating effects of frost.

In turning now to the *chemical characters* of the soil, we have first to notice its composition. It being understood that the soil is the exclusive source of mineral food to the plant, we of course expect to find all the ingredients of the ash of plants in every soil that is able to maintain vegetation. Great differences however, are found to exist in the proportions, and especially in the condition as regards solubility of these matters, as seen from the following analyses:

1st. Analysis of a productive wheat soil (clay) from Renfrewshire, Scotland, by Dr. Anderson:

Soluble in water—

Silica	0.0221	
Lime	0.0475	
Chlorid of calcium	0.0205	
Chlorid of magnesium	0.0061	
Chlorid of potassium	0.0003	
Chlorid of sodium	0.0015	
Sulphuric acid	0.0309	
Organic matter	0.2084	
		0.3373

Soluble in acid—

Silica	0.0838	
Alumina	1.6104	
Peroxyd of iron	3.4676	
Lime	1.0771	
Magnesia	0.1262	
Potash	0.0469	
Soda	0.0920	
Sulphuric acid	0.0039	
Phosphoric acid	0.0749	
		6.5828

Insoluble in acids—

Silica	74.4890	
Alumina	7.2540	
Peroxyd of iron	1.4167	
Lime	0.3150	
Magnesia	0.4043	
		83.8790

Organic matter—

Insoluble organic matter	6.1209
Humic acid	0.8924
Apocrenic acid	0.1280
Crenic acid	0.0128
Water	2.0930
	9.2471

99.9462

Amount of carbon, oxygen, nitrogen, and hydrogen in 100 parts of soil—

Carbon	3.1400
Oxygen	3.5060
Nitrogen	0.1428
Hydrogen	0.4200

2d. Analysis, by the writer, of sterile soil from the upper Palatinate, Bavaria—

Water	0.535
Organic matter	1.850
Silica	0.016
Oxyd of iron and alumina	1.640
Lime	0.096
Magnesia	trace.
Carbonic acid	trace.
Phosphoric acid	trace.
Chlorine	trace.
Alkalies	none.
Quartz and insoluble silicates	95.863

100.000

In fertile soils there is always to be found a quantity of fixed mineral as well as organic matters that are soluble in pure water. In the wheat soil this quantity amounted to but three parts in 1,000, and in this Dr. Anderson found no phosphoric acid and no oxyd of iron, although all the other mineral ingredients of plants were present. In the sterile soil nothing weighable, when, as was the case, but a small sample was operated on, could be separated by water alone, but as even this soil supported some vegetation—the whortleberry and various grasses as well as lichens, all the minerals found in vegetation might have been detected by exhausting a sufficiently large quantity. In the fertile soil is found a larger amount of matters soluble in acid, in the above instance six and a half *per cent.*; and here the analyst had no difficulty in finding *all* the mineral food of vegetation. In the sterile soil but little more than four per cent. of matters were dissolved by acids, and in this phosphoric acid and alkalies were not present in appreciable quantity. Finally, the larger share of the soil in both cases resists the solvent action of acids nearly altogether.

The portion soluble in water represents the presently available stock of plant food in the soil. As already intimated, plants receive their nutriment either as gas or as liquid. The fixed mineral matters of the soil are taken up by the plant from solution in water. If we examine the soil with sufficient care, we do not fail to find everything in it in a soluble state that is needed by vegetation.

Quite recently, Grouven and Stöckhardt have given renewed proof of this statement. Below is a tabular view of the matters found by these chemists in three soils—one poor, the others very productive:

1,000 parts of soil yielded to water.	Grouven.		Stöckhardt.
	Poor sandy soil, from Bickendorf.	Rich garden soil, from Heidelberg.	Very rich clover soil, from St. Martin, Tyrol.
Carbonic acid	0.0920	0.110	-----
Silica	0.1992	0.384	0.110
Sulphuric acid	0.0152	0.009	0.055
Chlorine	0.0007	0.015	0.012
Phosphoric acid	trace.	0.014	0.021
Oxyd of iron	0.0104	-----	⎫ 0.052
Alumina	0.0078	-----	⎭
Lime	0.0840	0.234	0.182
Magnesia	0.0062	0.016	0.020
Potash	0.0050	0.069	0.131
Soda	0.0357	0.046	0.083
Organic matter containing nitric acid and ammonia	⎱ 0.1010	0.306	0.530
Nitrogen	------	-----	0.160
	0.529	1.156	1,356

That portion which comes into solution only by the use of strong acids represents the reserve forces of the soil. Here we find stores of plant food, which, under natural agencies, require many years to become fully available to vegetation; but which are, nevertheless, constantly, though very slowly, contributing to the fertility of the soil. The least soluble matters, again, do not wholly escape slow alteration and partial solution, and, as analyses show, often contain alkalies, lime, &c.

As to the solubility of the food of the plant in water, it may be remarked that while the analyses quoted sufficiently demonstrate the general fact, science enables us to comprehend, to some extent, the detail of the processes which bring about this result. The chemist is in the habit of considering certain bodies, viz: silica, oxide of iron, and phosphate of iron or phosphoric acid in presence of oxide of iron, as absolutely insoluble, and under most circumstances they are so, in pure water, when alone. But in presence of other bodies, especially when the mixture is so complicated as in the soil, they manifest a very different action. Many bodies which do not yield to the solvent action of pure water are very perceptibly taken up by car-

bonated water, (*i. e.*, water saturated with carbonic acid.)   Thus, to
use a well-known instance, carbonate of lime is as good as insoluble
in pure water, but in carbonated water it dissolves quite readily.
Salts of ammonia dissolve phosphate of lime to a very appreciable
extent, as has long been known, and as Liebig has recently shown by
quantitative trials.   Silica is not absent from natural waters, although
the conditions of its solution are not well understood.   The chemist
has succeeded in preparing strong solutions of silica in pure water,
artificially, and the so-called infusoria of all fresh water streams,
which sometimes have accumulated to form beds of many miles in ex-
tent and many feet in depth, are but the silicious skeletons of micro-
scopic vegetable organisms that collected their silica from the clearest
and purest water.   Phosphate of iron is soluble, or at least yields its
phosphoric acid, under the conjoint action of carbonate or silicate of
lime and carbonated water.   Sulphate of baryta, even, is decomposed
in the soil, and yields its sulphuric acid to a growing plant.

Allusion has already been made to the importance of those matters
which, originally belonging to the atmosphere, have become a portion
of the soil.

Pulverized rocks do not constitute a good soil until they have be-
come weathered—*i. e.*, chemically decomposed, so as to contain a
portion of soluble matters, and also acquire a certain content of car-
bon and nitrogen.   It happens that these two effects are conjointly
brought about.   The neighborhood of a volcano affords opportunity
for tracing the formation of a fertile soil in a manner analogous
to, or identical with what occured over all the land before the
human epoch.   The lava that lies on the slopes or fills the con-
tiguous valleys, once melted rock, remains after cooling, almost
bare for years.   Then lichens begin to cover its surface.   These
succeed each other for generations, slowly increasing in number
and size, hastening by their decay the disintegration of the rock,
and causing the accumulation of humus and nitrates.   So the weath-
ering of the rock, the use and enrichment of the sparse soil, goes
on, perhaps, for centuries before the earth is deep and fertile
enough to produce low shrubs.   After another similar period a forest
is formed, with a soil rich in all that is needed for agriculture, being
stored with the fixed minerals that have been detached or solved
from the original lava, and having gathered during these ages mate-
rials from the atmosphere to make up the complement of fertility.

We often see railroad cuttings through beds of gravel or clay which
perfectly resemble the adjacent productive soil, but which remain for
years perfectly naked and barren, and only after a long period of time
assume a state of tolerable fertility.

The humus of the fertile soil, as already stated, does not, perhaps,
act to much extent in directly feeding vegetation, although we have
no positive evidence against the assumption that it is thus useful in
some degree.   It does, however, in many indirect ways contribute to
the welfare of the plant.   Its influence on the physical characters of
the soil, its mediating agency in maintaining the proper consistency,
moisture, and warmth of the earth, has been already noticed.   The

carbonic acid resulting from its ceaseless oxydation is of vast import-
ance, both as a supply of this form of plant food, in more abundant
measure than the atmosphere alone could yield, and as the most pow-
erful means of maintaining the requisite store of solved saline and
earthy food in the soil.

The general statement that humus, or, in other words, condensed atmo-
spheric plant food, is needful in the soil, requires some qualification.
It is not essential to all, even, of the so-called higher orders of plants,
or, indeed, to all agricultural plants. The cactus has its home on
the most naked arid sands. Pines and firs flourish in soil equally
destitute of humus. Buckwheat commonly grows on light, poor soils;
and it is asserted that in Peru and Chili, maize prospers in soils free
from humus, if started by a little guano, and afterward supplied with
water. We may, however, safely assert, that in temperate climates,
for the usual course of crops, a soil to be productive, in a practical
sense, must either contain originally, or have added to it, nitrogen
and carbon in assimilable form. Natural growth, in soil, destitute of
atmospheric ingredients, either of those plants just mentioned, whose
proper habitat is such a soil, or of the grains and common agricul-
tural plants, is, other things being equal, invariably too slow for the
purposes of agriculture. Not, indeed, for all purposes of agricul-
ture, for in what is called agriculture many very inferior crops
are annually reaped; but for the general purposes of a culture which
seeks to be in a high degree remunerative, the telluric elements are
insufficient.

The same holds true of the atmospheric as of the earthy ingre-
dients of soil in respect of varying quantity and different assimila-
bility.

In the poorest sand, analysis reveals the presence of nitrogen,
often one hundred times as much as is needed by the largest grain
crop; while in good soil the quantity of this element may amount to
from one to two thousandths of the entire weight. Of this nitrogen,
a portion exists as ammonia, another as nitric acid, but another and
far larger share of it, is in a form that is insoluble in water and una-
vailable to the plant.

In a rich garden soil that had been cultivated for many years,
Boussingault found in 100 parts—

Nitrogen	0.261		
Ammonia	0.0022	Containing nitrogen	0.00181
Nitric acid	0.00034	Containing nitrogen	0.00009

By actual trial with this soil, the same distinguished experimenter
found that only the small amount of nitrogen existing as ammonia
and nitric acid was of present use to vegetation; the remainder,
96-100 of the whole, being for the time quite inert.

The inert nitrogen appears to exist chiefly in the humus of the
soil, in a form analogous to that assumed by the same element in
bituminous or anthracite coal. It is, however, most probable not
utterly unassimilable; but, as the carbon and hydrogen which are
combined with it oxydize, it appears in the form of nitric acid,

especially in presence of lime or alkalies, or perhaps under other conditions as ammonia.

As to the amount of assimilable matters needful to constitute a fertile soil, we have hardly any just notion, nor, indeed, can we easily form one.

If we assume what is as yet not altogether warranted, the right of distinguishing between the assimilable and non-assimilable parts of the soil by the solvent action of carbonated water, we still encounter the variable influence of physical characters as affecting the distribution of the plant-food, and above all, there stands in our way the capital fact that as the growth of the plant is progressive, so are its wants, and likewise those solving mediating agencies which supply its food. So that we cannot, by observations made at any one moment, determine the value of ingredients which extend their action over a considerable period of time.

The same soil may vary exceedingly at different times in its content of soluble matters, as analysis has proved. In the garden soil above alluded to the content of nitric acid given is that found in June; but Boussingault informs us that in the following September the same earth contained near thirty times as much of this ingredient.

There is doubtless a rigorous reciprocal relation between the quantities of soluble (assimilable) matters in the soil and the *mass of soil* needed to feed a plant during the vegetative period.

The greater the proportion of soluble matters, the less volume of earth is neeeded to sustain a given crop. In practice it is found that each kind of plant requires a certain and pretty large quantity of soil for its development. The farmer has his rules as to the space which shall intervene between individual plants of wheat, of potatoes, of maize, &c.; and in regions widely distant from each other these rules, adopted as the best result of experience, are more or less unlike, varying with climate, soil, and other circumstances. It is found, also, that on a given soil nearly the same crop is obtained, whether the plants be closer to, or farther from each other, within certain limits. In case of fewer plants, each one is more vigorous, and gives a larger return; while in the other instance, the smaller individual yield is made up by the greater number of plants.

Boussingault, to whose numerous and admirable researches the student of scientific agriculture must constantly make reference, found by actual measurement that, according to the rules of garden culture as practiced near Strasburg, a dwarf bean had at its disposition 65 pounds of soil; a potatoe plant, (hill?) 190 pounds; a tobacco plant, 480 pounds; and a hop plant, 3,000 pounds.

In respect to chemical composition, we may assert that the absence of several, or even of one essential form of plant-food, must stamp a soil with utter infertility, no matter how abundant its other ingredients may be. It is equally true that the absence of one ingredient in assimilable condition must constitute a soil barren and worthless.

We may likewise lay down the proposition that the *deficiency*, *up to to a certain point*, of one or several substances in available form, renders a soil infertile. On the other hand we cannot, with any

hope of success, undertake to show what is this *certain point* or define the limits which, over-passed, make the soil unproductive.

It not unfrequently happens that the presence of noxious compounds greatly injures an otherwise excellent soil. Soluble salts of iron and alumina, especially the sulphates of these bases, are, so far as we now know, the principal causes of this kind of mischief. Some soils are formed from rocks that contain numerous grains and larger masses of iron pyrites or sulphid of iron, which, exposed to the weather, oxydize to sulphate of iron (copperas) and the solution of this salt in a certain stage of concentration destroys the vegetable tissues, and thereby renders the soil in which it exists unfavorable to growth. In a specimen of peat from Brooklyn, Conn., the writer found a not inconsiderable quantity of sulphate of iron, and likewise sulphate of alumina. Both these salts have a powerful decomposing effect on the rootlets of plants.

The importance which attaches to the proper availability or solubility of the nutriment in the soil leads at once to the inquiry, may not the soluble matters be washed out and lost by rains, or may they not accumulate in too great quantity?

There are certain influences external to the soil, which, acting reciprocally, tend to maintain in it a nearly constant content of soluble matter. On the one hand the disintegration of the soil, the decay of vegetation, rain, and dew, are perpetually enriching; while vegetable growth, springs, and streams, (rain that has passed through the mould,) and evaporation, are as continually wasting the soil. Since the mass of soil is so great, and the most rapid and exhausting of these processes operate so slowly, their effect is in general to leave the soil in possession of the requisite small amount of soluble matters, and only in exceptional cases can positive excess or deficiency occur.

In the soil itself we find, however, a remarkable property which enables it to convert excess of soluble matters into an appropriate quantity, and at the same time to store up this excess against what might otherwise be a period of want. The soil has, in fact, a power of regulating its supplies to vegetation, in a manner that was not dreamed of but a decade since.

The fact has been already alluded to, in treating of the physical characters of the soil, that it has a power of absorbing vapor of water, and in general other gaseous bodies—a power shared by the soil to more or less extent with all porous bodies.

Besides this purely physical quality, we find the soil to possess another absorptive capacity, which, though not independent of physical conditions, appears to be chemical in its nature, that is, depends upon the presence of certain *kinds* or combinations of matter.

Without this chemical absorption the other quality would be of little avail in directly nutrifying the plant, because water alone is capable of nullifying the latter, and at the same time performing any office that it might appear to exercise in a much more effectual manner. Ammonia has long been known to be taken up by the soil, and to be *retained* in it. Previous to the year 1850 it was supposed that this gas underwent absorption by surface condensation, exerted by

the more porous ingredients of the soil, namely, humus, oxyd of iron and alumina; an agency which is exhibited most strikingly in case of ammonia by charcoal, which, when freshly ignited, may absorb as much as ninety times its bulk of this gas. The ammonia thus condensed is, however, easily removed. Water or exposure to moist air at once displaces it, for it is only the absolutely dry charcoal that absorbs ammonia. Common moist charcoal has no appreciable faculty of this kind, its pores being already fully occupied, having satisfied their absorptive power on vapor of water and the ingredients of the atmosphere.

Liebig, reasoning from these facts, asserted in his "Chemistry applied to Agriculture and Physiology," that "the ammonia absorbed by clay or ferruginous oxyds is separated by every shower of rain and conveyed in solution to the soil."

The chemical absorption consists in the fixation and retention in the soil of volatile or dissolved matters, by their entering into comparatively insoluble combinations. This fixation is not, however, absolute, as we shall presently see.

Thompson and Way of England, in 1850, (see Journal Royal Agricultural Society of England for that year,) first began to develope the interesting facts which relate to this subject. Since the date of their investigations Liebig, Voelcker, Henneberg & Stohmann, Eichhorn, and Brustlein, have occupied themselves with its study.

The main facts are, briefly stated, as follows:

Free ammonia and lime, and their carbonates, are absorbed and chemically retained by the organic acids, (humic, crenic, &c.,) the ammonia in a non-volatile, but to some extent soluble form. Ammonia is also absorbed by oxyd of iron and alumina, and held in a non-volatile and very slightly soluble state.

Salts of ammonia, namely, sulphate hydrochlorate and nitrate, are at once decomposed by the soil when their dilute solutions are agitated with or filtered through it; the ammonia being retained, the acid remaining in solution united to *lime*.

The same salts of potash are likewise decomposed as above; the potash being retained, the acids uniting with lime.

Salts of lime, in general, are not absorbed, especially when added alone to the soil, or when the soil is rich in lime; but in several of Voelcker's experiments the liquor from a dung-heap containing a considerable quantity of sulphate of lime lost this ingredient nearly or entirely by filtration through a sandy soil, and at the same time the amount of carbonate of lime in the solution was diminished.

Salts of soda and magnesia are also retained, though usually in a less degree.

When solutions of phosphates and silicates of the alkalies are employed in these experiments, we find that the *acids* are also retained; and from the trials of Voelcker already referred to, we have evidence that sulphuric and hydrochloric acids are also liable to absorption. In no instance has a fixation of *nitric acid* been observed.

According to Brustlein's late researches, the retention of the bases when employed in saline combinations cannot occur except in presence of carbonate of lime. This view is, however, erroneous.

Way, after studying separately as far as possible the effect of each
ingredient of the soil without arriving at any satisfactory conclusion
as to the seat of this peculiar absorptive power, as a last resort inves-
tigated the relations of the *silicates* to saline solutions.   Silicates con-
taining but one base he found ineffectual, and next had recourse to
compound silicates.   He experimented then with feldspar, but found
that it was without action on solutions of ammonia salts, and hence
concluded that the powder of granitic rocks is not the agent of these
decompositions.   His next step was a more successful one.   He attempted
to imitate the compound silicates that may occur in the soil as products
of the weathering of rocks, such as most probably exist in all soils to
a greater or less degree.   He artificially prepared silicates of alumina
with potash, soda, lime, and ammonia, respectively; and these he found
to possess the property of suffering decomposition in saline solutions,
with the mutual replacement (fixation) of isomorphous bases.

But it was reserved for Eichhorn, in 1858, to set forth in a true
light the action of the double alumina-silicates.   This experimenter,
in cognizance of the fact that Way's artificial silicates contained water
as an essential ingredient, was led to make trials with natural compounds
of a similar character.   He selected for this purpose the zeolites, chaba-
zite, and natrolite, whose composition is given among those minerals
from which soils originate in the table on page 150.   The chabazite
he employed was essentially a silicate of alumina, lime, and water.
The fine powder of this mineral being agitated and digested for some
days with hydrochlorates (chlorids*) of potash, soda, dilute solutions
of ammonia, lime, &c., fixed in the solid and nearly insoluble form a
portion of the basic ingredient of these salts, while the acid was found
in the solution combined with a quantity of lime equivalent to the
absorbed base.   In one experiment the powdered chabazite was
digested for ten days with a dilute solution containing a known
amount of pure common salt.   The mineral was then found to have a
composition, compared with that it originally possessed, as follows:

Composition of Chabazite.

	Before digestion in solution of common salt.	After digestion in solution of common salt.
Silica ·	47·44	48·31
Alumina · · · · · · · · · · · · · · · · · · ·	20·69	21·04
Lime ·	10·37	6·65
Potash ·	0·65	0·64
Soda ·	0·42	5·40
Water ·	20.18	18·33
	99·75	100.37

Comparing the two statements, we see that nearly one-half the lime
of the original mineral is replaced by soda.   A loss of water also has
occurred.   The solution separated from the mineral contained nothing
but soda, lime, and chlorine, and the latter in precisely its original
quantity.

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* In chemistry the *hydrochlorate of an oxyd* signifies the same as the *chloride of a metal;* thus
hydrochlorate of soda and chlorid or chloride of sodium mean the same thing.

By acting on chabazite with dilute chlorid of ammonium for ten days the mineral was altered, and contained 3·33 per cent. of ammonia. Digested twenty-one days, the mineral yielded 6·94 per cent. of ammonia, and also had lost water.

Eichhorn found that the artificial soda-chabazite re-exchanged soda for lime when digested in a solution of chlorid of calcium; in solution of chlorid of potassium both soda and lime were separated from it and replaced by potash. So, the ammonia-chabazite in solution of chlorid of calcium exchanged ammonia for lime, and in solutions of chlorids of potassium and sodium both ammonia and lime passed into the liquid. The ammonia-chabazite in solution of sulphate of magnesia lost ammonia but not lime, though doubtless the latter base would have been found in the liquid had the digestion been continued longer.

It thus appears that in the case of chabazite all the protoxyd bases may mutually replace each other, time being the only element of differences in the exchanges.

In experimenting on natrolite, however, Eichhorn found that it was not affected by solution of chlorid of calcium, owing perhaps to some peculiarity in the constitution of this mineral, its soda being probably more firmly combined than that of chabazite.

These valuable researches, though serving but as an introduction to the study of a highly-complicated subject, present so close an analogy to what is observed in case of the soil, no matter whether it be fertile or barren, clay or sand, that we are fully warranted in assuming the presence in all soils of hydrous double silicates which determine the absorption and retention of potash, ammonia, &c., from solutions of their salts.

As regards the fixation of the acids, we know that oxyd of iron and alumina, as well as lime and magnesia under certain conditions, form insoluble phosphates and silicates; we are also acquainted with an insoluble *chlorine* compound, viz: chloro-phosphate of lime, which occurs abundantly as the mineral *apatite*, while sulphuric acid forms insoluble combinations with excess of peroxyd of iron anda lumina. We know, however, no insoluble compounds of nitric acid with any of the bases found in the soil, excepting oxyd of iron and alumina, and these require a high temperature for their formation.

The fixation of the bases in the circumstances described, both in the soil and with hydrated aluminous silicates, is influenced by a variety of conditions, physical and chemical. The only points which further require notice are: 1st. That an ordinary soil is capable of fixing a vastly larger quantity of ammonia, potash, or phosphoric acid—the three generally most rare, and therefore most precious forms of plant food—than is ever likely to be brought into the soil either by natural or artificial means. 2d. That the soil *never completely removes* any of these bodies from even the most dilute solution. 3d. The soil which has saturated itself from a solution of these bodies *restores them again slowly* to pure water or to a weaker solution.

Way, Russell, and Liebig, from a partial apprehension of the nature of this absorption, drew the premature inference that land plants do not receive their food from solutions, but themselves attack and solve

the soil. In the light of the facts we have set forth, this view is not for a moment admissible.

In seeking the means by which the dissolved matters of the soil find entrance into the plant, we must have recourse to the same agency which accounts for the imbibition of its gaseous food. Different liquids or solutions of different solids in the same liquid, if capable of mixture at all, exhibit the osmotic or diffusive tendency, which has been considered in case of gases.

If a tall vessel be partly filled with salt and then completely with water, the salt as it dissolves forms a solution much heavier than pure water, which therefore tends to remain unmixed at the bottom of the vessel. In fact it is easy to add the water so carefully that at first no salt shall be perceptible by taste or otherwise near the surface. In time, however, although every possible means of mechanical admixture be perfectly avoided, the salt will diffuse into the pure water until every portion of the liquid be uniform in composition.

Diffusion will take place equally well through porous membranes, provided they are capable of being wetted by (have surface attraction for) at least one of the liquids.

The apparatus shown in figure 14 is one commonly employed to

Fig. 14.

illustrate the fact of liquid diffusion. The tube *a* is fastened to the neck of a bladder filled with brine, solution of sugar, or other dense liquid, and the latter is immersed in the water of the large vessel. Immediately water passes inwardly to the brine (*endosmose*) and salt passes outwardly to the water (*exosmose*.) The endosmose being more rapid than the exosmose, the brine shortly rises in the tube to a considerable height.
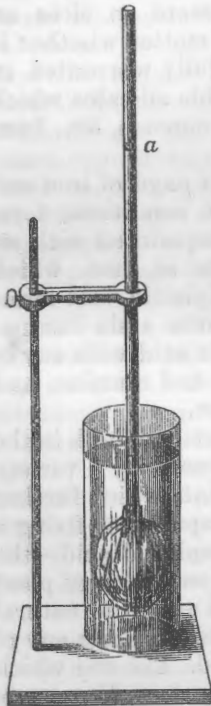
The rapidity and even the direction of the osmose is greatly dependant on the nature of the membrane. Alcohol and water diffuse into each other without difficulty when brought into direct contact; if we separate them by a bladder we find that water will rapidly pass into the alcohol, but the reverse flow will take place with great slowness, for the reason that alcohol cannot wet the surface of this membrane. On the other hand india-rubber is readily moistened by alcohol but not by water; and if a thin sheet of this substance be interposed between these liquids, it will be seen that alcohol passes the membrane into the water much more rapidly than water traverses in the opposite direction.

Schacbt has made observations on the cell-membrane of the *Caulerpa prolifera*, a plant presenting single cells of sufficient size for such purposes, and found that it admitted of all the phenomena of diffusion exactly as manifested by other membranes.

The rootlets of a plant being immersed in the water (or moisture) of the soil, act towards it as the bladder filled with brine in our

figure.   The liquids of the root-cells being of different composition
from the soil-water, and the cell-membranes admitting (having surface
attraction for) the soil-water, the latter with its contents penetrates
the cells, *so long as difference of composition or want of equilibrium in
the surface attractions*, either of the membrane for the liquid, or of
the dissolved matters for the solvent, exist.   The diffusion goes on
from cell to cell in the same manner throughout the whole plant, as
long as any cause produces inequality in the mutual surface attractions
of any two of its ingredients, whether solid or liquid.

Since perpetual changes are progressing in every part of the grow-
ing vegetable organism, we have no difficulty in finding the causes
which keep up diffusion in or into the plant.

Let us suppose that in any cell there exists at the moment a liquid
containing in solution all the food of vegetation.   If now carbonic
acid and water unite to form dextrin, and this solidifies in the shape
of starch or cellulose, there is formed in this cell a vacuum which
disturbs the osmotic equilibrium of the whole plant, and determines
a movement towards this cell of carbonic acid from the leaf cells and
of water from the root cells to restore the same.

An atom of lime coming in contact with newly formed oxalic acid
combines with it to form an insoluble salt; the lime thus removed
from solution is at once replaced from an adjacent cell; this again
supplies itself from another in the direction of the soil, until the
extremity of a rootlet is reached, and here an atom passes in from
the soil water, this again to be replaced from the surrounding stores.

The vast amount of water that is removed by evaporation (the
attraction of dry air for water) from the foliage of vegetation is in
the same manner supplied from the soil, and it traverses in its upward
way all the cells of the plant.   The supply of saline matters is
however partially or wholly independent of this ascending current of
water, for it must be very greatly checked in circumstances where
the atmosphere is saturated with moisture, as in a conservatory or
Wardian case, although here growth goes on with the greatest vigor.

It thus appears that whenever any chemical or physical change
occurs in the plant, we have the origin of a disturbance which may
set in motion the juices of the cells, the water, and dissolved matters
of the soil, and the gases of the atmosphere.

In this manner our cultivated plants are able to gather their food
from solutions like the water of springs and wells, or the aqueous
extract of soils, which are so dilute that but one part of potash or
phosphoric acid is present in one or even twenty thousand parts of
water.   So, too, we may find in plants, substances which it is im-
possible to detect in the soil, and it is not a little interesting that
iodine, a substance largely employed in medicine and photography,
is almost entirely procured from the ashes of sea-weeds, although it
has never yet been detected with certainty in sea-water, even by
the use of methods that would enable the chemist to find it, did it
form but one part in a million.

LECTURE IV.

IMPROVEMENT OF THE SOIL BY TILLAGE, DRAINAGE, AMENDMENTS, AND FERTILIZERS.

Having attempted to define at length the reasons of fertility in the soil, we may appropriately recapitulate this part of our subject in order to set in a clearer light the means of improvement.

1. A fertile soil must contain all the mineral matters (ash) of the plant.

2. It must include a certain store of atmospheric ingredients, viz: organic matters or their equivalents—ammonia or nitrates—in short, some store of nitrogen, and usually of carbon.

3. It must contain these matters in an available or assimilable form, *i. e.*, in a certain degree of solubility in water, thus yielding them to vegetation as rapidly as required.

4. The soil must be free from noxious substances.

5. Must possess favorable physical characters, be neither too porous nor compact, neither too wet nor too dry; must afford a congenial home and lodgment for the plant.

It is comparatively rare that these conditions are perfectly fulfilled in nature, or if they exist in any given place at a certain time they suffer disturbance after a longer or shorter period. Hence the ancient and wide spread art of cultivation or improving the soil. Hence, too, the immense practical importance of a scientific, *i. e.*, accurate and complete understanding of the conditions of fertility and of the means of communicating or restoring them.

The method of improvement, like the characters of the soil, fall naturally into the two classes, mechanical or physical, and chemical.

The first class of improvement comprehends tillage, drainage, and mixture.

In the second class is included whatever contributes to the nourishing qualities of the soil, either by direct addition of the food of plants, or of agents that collect, solve, or otherwise prepare this food, as manures and amendments.

This division, though warranted for convenience of study, has no practical existence, for the chemical and physical phenomena of nature are always so intimately associated that their rigorous separation is, in most cases, impossible.

In a very fertile soil it is only needful to deposit the seed in favorable circumstances as regards temperature and weather, and in due time the harvest is ready. In such a soil there is a sufficient store of plant food, and all the external conditions of rapid vegetable growth. In the poorer soil, in most soils, in fact, there is some want to be supplied, some improvement to be attempted. The first step in meliorating the soil, the one almost universally indispensable even in fertile soils, as a preparation for the seed and young plant—the step always first made in practice and the one in general first required by enlightened theory, is tillage.

The operations of tillage, viz: spading, ploughing, harrowing, &c., have the mechanical effect to break up and admix the earth. They convert the surface compacted by rain and sun into a loose and friable mould suitable for the deposition of the seed and for the enlargement of the roots of the young plant. Beyond this, these operations, really, though but to a slight extent, mechanically lessen the size and increase the number of the earthy particles.

It is chiefly the loosening of the earth and the consequent better admission of water and air, which facilitate the disintegrating effect of these atmospheric agents, whereby, as already explained, the rock fragments are decomposed and dissolved with perpetual increase of the stores of assimilable food.

Tillage likewise assists, in the same manner, in converting any poisonous matters into innocuous or even salubrious forms. Soluble salts of protoxyd of iron, which might accumulate in the deeper soil, are, by exposure to oxygen, changed into insoluble and harmless combinations. Exposure of the soil by tillage to the atmosphere also has the effect to increase the absorption of ammonia, and to hasten the process of nitrification.

Finally, the circulation of water and the consequent distribution of plant food, the removal of excessive moisture after rains, and the absorption of water vapor after droughts, as well as the regulation of the temperature of the soil, are promoted to a most advantageous degree.

In that stage of agricultural which first follows upon pastoral or migratory husbandry, the simplest modes of cultivation are the only ones practiced ; the amount of tillage is small, just sufficient to prepare way for the seed, and it is accomplished by the rudest implements.

With the progress of the arts, ploughing, harrowing, &c., are employed to a greater extent. The implements used in these operations are improved in construction, and adapted to all varieties and situations of soil, so that they may be worked at a greater depth and more frequently, as well as at a reduced cost.

A matter of great importance in tillage is to secure a proper *depth of soil*. It is obvious that, other things being equal, the deeper the soil the more space the roots of crops have in which to extend themselves, and the more food lies at their disposal. By deep culture new farms are discovered beneath the old, and it is possible to realize the apparent absurdity of "more land to the acre."

Deep culture is one of the most efficacious means of counteracting drought, as we shall notice presently in discussing drainage.

Deep tillage is not, however, always practiced. The grain fields of Germany, even in the most carefully tilled provinces, as Saxony, are to this day mostly ploughed with rude wooden tools often not unlike those figured in classical dictionaries as in vogue among the ancients, which merely score up the soil to the depth of two, three, or rarely four inches. In our country, which surpasses every other in the real merit of its agricultural implements, and where the means of deep tilth are in the hands of every farmer, tillage is notwithstanding

shallow in the main, and our agricultural journals are often occupied with discussions as to the advantage or disadvantage of deep culture.

There are, indeed, some instances in which deep ploughing is injurious, either permanently, or as most generally happens for a short period. In the latter case the temporary injury most often turns out to be a lasting benefit.

Where a thin surface soil of fair quality rests upon a gravel or other leachy stratum, too deep ploughing may, so to speak, knock the bottom out of the soil, i. e., by breaking through into the open sub-soil, may injure the retentive capacity of the upper soil for water and manures. In case the sub-soil is of a "cold" ochery, noxious character, the bringing it to the surface may occasion detriment for the time.

The plough is the instrument most extensively employed for tillage, and the one to which recourse must be had whenever large fields are to be broken up. In ordinary ploughing the soil is inverted, and according to its texture more or less pulverized and mellowed to a depth of from three to six inches. Trench ploughing consists in a similar inversing of the soil to a considerably greater depth, as far as one foot or more, and is practiced to advantage where the soil is good to this depth, especially with the view of bringing up manures which are supposed to descend and accumulate below. Sub-soil ploughing is intended merely to break up and loosen the lower soil without bringing it to the surface. The sub-soil plough is merely a narrow share or wedge that follows the furrow of the common plough, and disturbs the ordinary plough bed to the depth of several inches. Its employment is expensive and less in vogue than it was a few years ago. It is mainly useful where the sub-soil is with difficulty penetrable to water.

In garden culture, or even in field culture in certain countries, as in parts of Italy where labor is cheap, spading and forking are employed instead of ploughing, and with great advantages in heavy soils, because the tread of beasts of draught is entirely avoided, and the soil is much more throughly pulverized, intermixed and loosened up.

After ploughing and if need be cross-ploughing, the harrow, scarifier or cultivator, some form of toothed implement, is drawn over the field to accomplish a sufficiently perfect comminution and levelling of the surface for the seed-bed. On heavy clays which, especially in wet weather, are thrown up by the plough in tenacious lumps that further harden in the wind and sun, the clod crusher, a system of toothed disks revolving at a little distance from each other on a common center, at right angles to the line of draught is employed.

On very light soils the roller is used to make the earth more compact, especially above the seed.

In late years a countless number of modifications and not a few improvements in the implements and methods of tillage have been suggested, and to a greater or less degree employed, in practical agriculture; but it is not the place here to enter further into details.

In certain localities, tillage may completely and profitably replace all other means of improving the soil.

It is obvious that with each harvest there is removed from the soil

a quantity of potash, lime, phosphoric acid, and other fixed mineral matters, and likewise more or less ammonia and nitric acid. With every crop the field yields, its own stores of fertility are drawn upon, and, in fact, lessened, and after a certain number of crops are gathered, the available food of most soils is so far diminished that the succeeding crops fail of full development; in other words, the soil is exhausted. By exhaustion in a practical sense is meant, be it noticed, no absolute removal of plant food, but such a relative diminution as causes the harvests to fall below a medium or standard yield.

It is the business of culture to replace this spent material, to restore the capacity of the soil, to keep it up year after year to a remunerative degree of productiveness.

Jethro Tull, a distinguished Englishman, who worked and wrote in the last century, was led to adopt the theory—not at all improbable, viewed from the scientific stand-point of his day—that the impalpably fine particles of earth are the real food of vegetation, and accordingly he sought to fit the soil for a more rapid and perfect nutrition by pulverizing it. He introduced the horse-hoe, or cultivator, into English husbandry, and actually succeeded, by the diligent use of his improved implements, and by a peculiar mode of occupying his field, in obviating the necessity of any manures and in raising successive crops on the same field uninterruptedly for twelve years. He failed, however, in maintaining this system for a longer time, having adopted one fatal rule, "never plough below the staple." It is but just to the memory of this eminent agricultural philosopher to explain why he adhered to a notion to us so absurd. Tull was aware of the important part played by the atmosphere in the nutrition of plants. The use of stirring and pulverizing the soil was to enable the particles of earth to attract from the atmosphere "the nitre or acid spirit of the air," which, in his view, further dissolves and prepares the soil to support vegetation. He had no chemistry to teach him that the indispensable mineral matters of the soil exist in it in such minute quantity, and are therefore liable to exhaustion. He had no analytical data to reveal the difference between the chemical statics of the vineyard—from the sagacious observation of which his theory originated—and the wheat field, which more largely robs the soil of alkalies and phosphates, and so he found it reasonable to use only that portion of the soil—the staple or usual tilth—to which the atmosphere has obvious access.

The system of Tull has, however, been revived, and, with the modifications suggested by modern science, has been eminently successful in the hand of its ingenious advocate, the Rev. S. Smith, of Lois Weedon, Northamptonshire, England. Mr. Smith has produced large wheat crops continuously on the same soil for a series of years by simply laying off his fields in strips five feet wide, and growing his crops in drills, with frequent and deep hoeing, on alternate strips in successive years. The tillage of the vacant strip this year prepares it to sustain a crop next year—enables the solution and absorption of food enough to feed a full crop.

By this plan of culture Mr. Smith raised the yield of his wheat

grounds from 16 bushels to an average (for ten years) of 34 bushels per acre. Although he asserts that he has never known this plan—which differs from Tull's chiefly in the *depth of tillage*—to fail where carried out according to his directions, it is easy to see that not every soil will admit of its successful application, even independently of considerations of cost. This method demands for its success that the soil be so deep and so readily decomposable that the plant may find its needful supplies in one-half the accustomed superficies, and there-fore must possess physical properties that, under the treatment, are in the highest degree favorable to vegetation.

On large holdings the maintenance of such an amount of assimilable food as constitutes the soil fertile, is often profitably accomplished by the ancient practice of summer-fallow, which is the same thing for a whole farm as the vacant strips in the Lois Weedon system are for the wheat fields. A field is left void of crops, and is repeatedly ploughed and harrowed during the whole of one summer, generally re-ceiving the seed of some winter grain in the autumn. The fallow is thus an extra period of rest for the soil—enables it to accumulate within itself a store of fertility against future harvests, and is often attended with collateral advantages that alone are sufficient to war-rant its employment, viz., the destruction of weeds, insects, and the improvement of the texture of the soil.

In many situations these processes of tillage are so laborious or in-effectual that recourse must be had to other operations to change radically the characters of the soil.

Heavy clays, especially in a moist climate, are very difficult of tillage from their peculiar physical qualities. In spring time they become so exceedingly tenacious and compacted by the rains, that they dry with extreme slowness. While wet they resist any attempt at pulverization, because if ploughed in that condition the plastic up-turned masses harden in drying to intractable clods. It hence results that heavy clays need to be tilled when they have arrived at a certain stage of dryness, and then the operation of ploughing is exceedingly laborious, while the full preparation of the seed-bed is brought late into the season. As clay soils dry, the surface is baked into a crust which impedes the circulation of water, and which, shrinking and cracking apart in innumerable places, ruptures the rootlets of plants. Is is especially difficult to induce a deep tilth in such soils, so that during protracted drought the crops suffer greatly on them.

When clays are not continuous in depth, but rest upon a gravelly and open sub-soil; or when, by art, underground channels are pro-vided for the removal of surplus water, these impediments to tillage and to profitable culture are greatly lessened or entirely removed.

Many soils of lighter character, and in wet climates, sandy soils even, are remarkably benefited by artificial provision for the removal of surplus or bottom water.

It is but a few years since the introduction into general practice of a system of drainage intended to effect this purpose took place in Great Britain. James Smith, of Deanston, Scotland, led by an in-ductive study of the evils, and the true means to be employed in the

improvement of cold soils, devised what, under the name of Thorough Drainage, has become one of the most useful appliances in cultivation.

Thorough drainage consists essentially in constructing underground channels, sufficient in number and size, for the removal of surplus water down to a certain depth. A clay field, for example, has a system of parallel ditches dug in it, three or four feet in depth, and sixteen to thirty feet apart. These have such an inclination, and so connect with cross or main ditches, as to give the water that may collect in them a ready discharge. The bottoms of the ditches are then filled with small stones to the depth of about one foot, or have carefully laid in them a pipe of baked clay, (drain tile,) one to three inches in diameter, and are thereupon filled up with earth. These channels at once discharge the water of rains and melting snows when the soil is sufficiently porous; and if at first, as happens with clays, the soil is too retentive to allow the ready removal of water, this evil mends itself in a year or two. We know that a mass of clay exposed to the air in dry weather gradually dries off superficially, and appears full of minute fissures or larger rifts. In time it becomes entirely friable; and if water be poured on it the liquid, for the most part, rapidly filters through. It is only by a prolonged immersion in water that the dried clay absorbs so much of it as to become tenacious and plastic again. The under drains are the effectual means of drying out the clay soil to such a point that excess of water flows off without hindrance, and they are no less effectual in preventing the recurrence of a too retentive state.

The fact that we are in possession of extended treatises on drainage, renders it unnecessary to do more here than to allude to some of the more striking results of this system which have been observed in practice, and to indicate their scientific explanation.

One of the most important effects of thorough drainage consists in tempering the extremes of moisture and dryness, of heat and cold, so that a drained soil is dryer in the wet seasons and moister in the dry seasons—is warmer in cold weather, and cooler in hot weather, than an undrained soil.

The result of the rapid removal of surplus water on the soil is such as enables it to be tilled from two to four weeks earlier in the spring than might otherwise happen, a gain which, in cold climates or backward spring-times, is often the saving of a crop.

The vast mass of water that is thus removed without evaporation corresponds to a large increase in the amount of heat which may accumulate in the soil, an increase that is not only perceived in the rapid growth of vegetation after the ground is prepared for seed, but also is manifest in the earlier melting of snows. The official inquiries of the Royal College of Rural Economy of Prussia show that the snow in that country thaws away on the average one week earlier on drained than on contiguous undrained land.

It is said that Smith, of Deanston, was led to his study of drainage by an observation made on ridged fields. From time immemorial it has been a custom in some countries, especially in those overrun by

Roman civilization, to ridge up the fields by the plough, thus bringing the soil into beds of a rod or thereabouts in width, which are several inches higher in the centre than at the edges. It was observed that in time of dry weather the plants stationed upon the centre of the ridges fared best, while those at the borders were liable to suffer, although it might be supposed they occupied the most favorable position, so far as access to the subterranean moisture is concerned. On a moment's reflection, it is obvious that the deeper the "staple" or penetrable friable soil is, the greater space will be occupied by the rootlets of plants, and the larger will be the supplies of capillary moisture; so that if the soil under the influence of protracted drought becomes surface-dry to the depth of one inch or two inches, less injury will accrue to the crop whose roots are diffused through a deep soil than to one stationed in a shallow tilth. The fact seen in the ridged fields is far more plainly exhibited on comparing drained and undrained lands. In fact, drainage is recognized, among practical farmers, as the best protection against drought. Not only does it regulate the use of the water which falls upon the fields as rain, but by exposing an immense amount of absorbent surface to the atmosphere, which freely permeates the drained soil, large quantities of water are collected and condensed from the vapor of the air. It has been recently observed at Hinxworth, England, that the flow of water from drains sometimes increases considerably when the barometer falls, although no rain-fall has occurred.

The various chemical advantages that have been already attributed to tillage, viz: aeration of the soil, solution and preparation of plant-food, oxydation of unwholesome matters, are evidently to be anticipated from drainage in an eminent degree. In wet climates it is found to be the best preparation for effectual tillage, and where the condition of the soil requires it, the indispensable pre-requisite to profitable husbandry.

The tenacious and intractable characters of clay soils are also effectually overcome by the operation of heat—by burning the clay. A heat of redness expels the combined water of clay, and destroys forever its tenacity. A part of the soil is converted into something like brick-dust, and the admixture of a small proportion of this is sufficient to amend the heaviest soils. The same burning likewise makes soluble the alkalies, and, in fact, nearly all the fixed mineral matters of the clay, thus rendering it more fertile by increasing its power of feeding vegetation.

It often happens that contiguous soils are greatly improved by mixing together. A few loads of clay remedy the too great porosity of a sand, and *vice versa*.

The physical characters of the soil being set to rights, the next point is to feed the plant. So soon as crops fall below a certain unremunerative rate of yield, which, in most soils, happens in a few years, other means of improvement, viz: *manures*, are called into requisition.

We have already spoken of tillage as a substitute for manure; but the word manure originally included tillage, coming from the French

manœuvrer, (*main ouvrer*,) or Latin *manus operor*, signifying to work with the hands, a sense in which it was employed by Milton. The term manure is now used in a general way to signify any *substance* added to the soil to make it more productive.

Substances added in large quantity often act chiefly by qualifying the physical properties of the soil, and are then appropriately termed *Amendments*. Matters which operate in the main by feeding vegetation are more properly *Fertilizers*. These again may nourish *directly*, by supplying at once to the growing plant one or all the nutrient ingredients it requires; or *indirectly*, by making soluble the stores of the soil, or otherwise disposing them to assume assimilable forms, or by absorbing matters from the atmosphere. Most manures combine these various offices to a greater or less degree.

While the popular name of those materials that are successfully employed as manures is legion, the chemist, by his analysis, recognizes in them all only the same dozen kinds of matter which constitute plants and soils.

The use of manures has been known from the earliest times, and there has been no lack of attempts to explain their effects; but it is only after the sciences of chemistry and vegetable physiology had entered upon the modern development that it was possible to begin understanding their mode of action. So difficult is the subject that we are as yet by no means advanced to its full comprehension, which requires a complete knowledge of the relations of each nutritive element and compound with the plant, with the soil, and with the atmosphere.

During all the centuries in which agricultural experience, with reference to the operation of manures, has accumulated, we find that the opinions of practical farmers have been almost endlessly at variance; and as these conflicting opinions have faithfully reflected the facts and phenomena which have presented themselves to agriculturists, we are prepared to find that at the present day there is a constant recurrence of endlessly differing results in the use and estimate of manures. We find in our current agricultural journals abundant examples of crops being benefited by application of nearly every one of the ash ingredients of the plant, as well as by ammonia and nitrates, or bodies yielding these; and, on the other hand, repeated instances of their failure. A scientific consideration of these results enables us to explain much that is obscure, and reconcile much that is conflicting, by taking into the account differences of soil, climate, and crop; and by a careful study of the circumstances which alter cases to such a great degree, it will be possible, in time, to unfold every mystery and elucidate every variety of effect.

The space at command here does not allow any detail with reference to the action of manures, except as may illustrate some of the general principles which alone can serve to initiate us into the method of their operation.

These general principles are the following:

1. *Plants require various kinds of fixed mineral matters, and derive the same exclusively from the soil.*

The only exceptions to this statement are, perhaps, to be found in case of chlorine and sodium, which appear to be carried inland from the sea in the direction of prevailing winds, both in the spray and dissolved in the vapor that ascends from the ocean.

2. *Some plants which, in the natural state, derive a large portion of the volatile elements of their structure—viz: carbon, hydrogen, oxygen, and nitrogen—from the air, must be supplied with much more of these matters from the soil, in agricultural production.*

As already remarked, the increased supply of these matters by the soil is requisite only to insure that rapid and abundant growth which constitutes agricultural production.

The very fact of an artificially increased supply of food to plants, in connexion with the care otherwise provided by cultivation, in a few generations enlarges their capacity for assimilating nutriment, greatly increases the mass of vegetable matter that can develop on a given surface, and, in consequence, makes a fertile soil necessary for exhibiting the capabilities of the crop.   Many of our agricultural plants are the result of high cultivation, including, as one of its most efficient factors, a fertile, and, in most cases, artificially fertilized soil.   The wretched weeds from which our numerous varieties of turnip, rutabaga, kohl rabi, cauliflower, broccoli, and cabbage have been derived, are hardly recognizable as the originals of so many useful plants, and these, as well as the wild egilops of southern Europe, from which the wheat grain appears to have come, are no less inferior to the cultivated plants, in appearance and value, than is the soil required for their natural development, to that demanded in their agricultural production.

3. *Different plants require different proportions of these substances for their luxuriant growth.*

4. *Different plants require different absolute quantities of food to mature a full crop.*

These propositions are illustrated by the accompanying table, which represents, in average figures, the weight, in pounds, of total produce, and of the chief ingredients, removed annually from an acre of good land, in case of several of the more commonly cultivated crops.

	Yield per acre.	Nitrogen.	Entire ash.	Phosphoric acid.	Potash.	Lime and magnesia.	Silica.
Wheat—							
Grain	1,840	34	32	15	10	5	$\frac{1}{2}$
Straw	4,600	14	207	8	39	11	145
Total	6,440	48	239	23	49	16	145$\frac{1}{2}$
Rye—							
Grain	1,470	28	25	12	9	4	$\frac{1}{2}$
Straw	3,500	12	140	4	27	9	93
Total	4,970	40	165	16	36	13	93$\frac{1}{2}$
Beans—							
Seeds	1,840	76	60	20	27	8	$\frac{1}{2}$
Straw	2,700	33	138	14	34	50	13$\frac{1}{2}$
Total	4,540	109	198	34	61	58	14
Beets—							
Roots	36,800	88	353	22	158	40	20
Tops	9,200	26	173	11	69	28	12
Total	46,000	114	526	33	227	68	32
Clover	6,000	130	390	25	105	121	21
Meadow grass	4,000	53	246	13	58	62	78

This table shows, that, other things being supposed equal, a supply of nitrogen sufficient for a full rye crop would answer but for one-third of a clover or beet crop; the phosphoric acid sufficient for a meadow is but little more than half enough for a wheat field, and only one-third as much as a crop of beans requires. It appears that the potash which would fully nourish a crop of wheat is nearly enough for grass or beans; while for clover twice, and for beets four-and-a-half times as much is needful. A clover crop demands almost ten times as much lime and magnesia as suffices for rye, and a wheat crop must have more than ten times as much silica as serves the growth of an equal yield of beans.

The erroneous conclusions which a hasty deduction might bring out of the foregoing instructive table are checked by the fact expressed in the next proposition, viz:

5. *Different plants, from peculiarities in their structure, draw differently on the same stores of nutriment.*

There are some plants which flourish on the poorest soils, being adapted to resist the extremes of drought, and accumulate their food

under what are, for nearly all agricultural plants, the most unfavorable conditions. Rye, for example, will grow well where wheat is utterly unprofitable. Buckwheat yields a fair crop on exceedingly poor soils; and the lupine is so extraordinary in this respect that by its help the farmer may cover the most desolate blowing sands with a luxuriant vegetation.

On the other hand, some crops are easily spoiled by overfeeding. Thus wheat, and the slender-stemmed grains generally, are unremunerative on the newly broken up prairies of our west, while maize flourishes even on the richest soils, being in practical language "a rank feeder."

It is plain that, other things being equal, a plant with long-branching numerous roots does not require so rich a soil as one with these organs short and few, because it has a greater mass of earth at its disposal out of which to collect its food.

Again, those plants which expose to the air a large leaf surface should, other things being equal, flourish better than the sparsely-leaved plants in a soil poor in atmospheric elements.

A plant which is of slow, regular, and protracted growth may, in the same manner, organize more vegetable matter on a given soil during a summer than one which quickly runs through all the stages of its life, and therefore requires more rapid supplies of food—demands more in a given time.

In general, also, those crops which produce seed require a better soil for their continuous production than such as yield only foliage.

6. *Different soils abound or are deficient, to a greater or less degree, in one or more needful ingredients in assimilable form.*

With the original differences of soils are to be likewise classed the changes in condition which tillage and cropping are perpetually inducing. By the continued removal of crops the soil suffers a diminution of its resources, and often some one or a few of the nutritive elements are soon brought to a minimum, while the others still remain in quantity sufficient for hundreds of harvests. According to the original composition of the soil, the failing ingredient may be potash in one case, sulphuric acid in another, lime in another; and application of these substances, respectively, may then form the most profitable manuring.

7. *It appears from experience that the ingredients which are rarest in the soil—which are therefore most liable to exhaustion, and most needful to be replaced—are, in general, phosphoric acid, assimilable nitrogen, (be it in the form of ammonia or nitric acid,) and potash.*

The substances just named are therefore important ingredients in all those manures by whose continued and exclusive use the soil is kept fertile, and constitute the chief part of such fertilizers as bring up exhausted lands to immediate and remarkable, though it be temporary, productiveness.

The above is intended as a very general statement, the truth of which, as such, is not invalidated by the numerous and important exceptions which occur.

In examining the question of the direct action of manures, we have first to notice the value of deductions from the composition of a sub-

stance as to its fertilizing effect. Can we, by the study of the composition of a crop, decide what manure is most likely to benefit it? or can we determine, from the composition of a manure, what crop it is best adapted for? The answer to these questions is, in many cases, No! In laying down the general principles which are to be regarded in a rational theory of manuring, we have had frequent occasion to make the truth of a proposition depend upon "other things being equal." Now it happens, unfortunately for the simplicity of our science, that "other things" are often in the highest degree unequal and unlike, so that we must busy ourselves with the slow work of induction from facts mostly yet to be extricated by toilsome experiment from their present confusion, rather than incumber theory and disgust practice by generalizing deductions that cannot fail to be premature and erroneous. There are many cases in which the effect of a fertilizer can be immediately connected with its composition. It not unfrequently happens that pasture lands from which the only matters agriculturally removed are the ingredients of cheese, after long use, deteriorate, refuse to nourish dairy animals, and become nearly worthless. The use of bones or phosphatic manures restores such fields to perfect pasturage; and the explanation afforded by chemistry—viz: that all the phosphate of lime put in the milk as a provision for the formation of the bones of a young animal is permanently alienated from the soil in the exports of cheese, so that exhaustion of this substance is caused, unless phosphates be applied—is entirely satisfactory.

The leguminous plants, though the richest in nitrogen of all our crops, do not by any means require nitrogenous manure to the extent demanded by wheat, which removes from the soil but one-half as much, or less, of this substance. The difference here is obviously due to the fact that the leguminous plants have deeper roots, more foliage, and a longer period of growth.

Leguminous plants are rich in lime and sulphur, and hence are often remarkably grateful for applications of gypsum. Fruit and shade trees yield an ash largely consisting of carbonate of lime, and their growth, especially on meager sandy soils, is often wonderfully enhanced by the accident of some oyster shells or old mortar being thrown on the ground over their roots.

The grasses and grains contain a large amount of silica in their stems and leaves; but the artificial use of soluble silicates of potash and soda has rarely been attended with more benefit than that of the corresponding chlorids, and for the reason that silica is so universally distributed.

Mr. Lawes, of England, found that on his farm wheat might be grown for a dozen years or more in succession on the same field, and give an average crop of 17 bushels per acre, without manure; while a contiguous field, planted in turnips, in three years came to yield scarcely anything. Mr. Lawes then found that, by the use of nitrogenous manures, the wheat crop was at once doubled, while the turnip crop was hardly affected; and, on the other hand, a mixture of sulphate and soluble phosphate of lime (super-phosphate of lime)

had little influence on the wheat crop, but at once raised the turnip field to a considerable degree of productiveness. These facts, borne out by the quite general result of practice, indicate the conclusion which some eminent authorities have unhesitatingly adopted, that soluble phosphate of lime exercises a specific action on the turnip, independent of the actual need of this plant for phosphates. There are, however, such grounds for doubting this doctrine that, until further investigations give us more complete data for judgment, a decision must be suspended.

Some recently described experiments of Mr. Lawes on the effect of fertilizers upon meadows are very interesting. He found that when a manure consisting of phosphates and sulphates of lime, potash, soda, and magnesia was applied to grass land, the development of clover was at once astonishingly increased; while, when nitrogenous manures were used, either alone or in addition to the above mixture, the true grasses maintained the mastery.

The attempt made not long since to manure, plants with mixtures representing what is taken off the field by a crop, turned out unsatisfactorily, as the facts we have instanced make evident such a scheme must; and we are led every day more and more to seek explanations of the anomalous effects of manures in their indirect action.

The most familiar instance of indirect action is that of gypsum or sulphate of lime. In contact with carbonate of ammonia, with so much water as to make the mixture *wet*, an interchange of ingredients takes place, so that sulphate of ammonia and carbonate of lime are formed; and Liebig accounted in part for the beneficial operation of gypsum by assuming that it thus "fixed" the volatile carbonate of ammonia of rains and dews, and held it in the soil for the use of vegetation.

On the other hand, Boussingault showed that when the mixture of sulphate of ammonia and carbonate of lime, from being wet, dries so far that it is only moist, like the soil is ordinarily, the reverse decomposition ensues, and the ammonia once fixed, is unfixed. While we can conceive of circumstances in which both these properties come into play, beneficially or otherwise, it must be remembered that the more late discovered absorbent power of the soil sets these effects of gypsum quite out of the account in nearly all cases.

Humus, which, in the form of peat or swamp muck, or as resulting from the decay of litter and the carbonacious ingredients of the excrements of cattle, is a most common and useful manure, doubtless accomplishes more by indirect than by immediate action. It is the most energetic absorbent of ammonia, as carbonate (according to Brustlein, not of other salts) is the source of carbonic acid in the soil, thus, by its presence, setting in operation the endless train of changes whose result is the solution of mineral matters, and by its hygroscopic character it assists to maintain the proper physical condition of the soil.

Lime, which is one of the greatest renovators in use in agriculture, is, in a similar manner, of more indirect than immediate effect. Its influence is especially manifest in fluxing the insoluble stores of plant-

food, and compelling the soil to yield its ingredients to the support of vegetation.

Ammonia, when acting on the soil as carbonate, (coming from the decomposition of urea, uric acid, and other nitrogenous bodies,) is not inferior to lime in its solvent effects.

Gypsum, common salt, carbonate of lime, nitrates of potash and soda, and in fact all the saline compounds which are incorporated with the soil in manures, may exert important physiological effects on the plant in addition to their mere nutritive function.

We have already intimated that the transpiration of water through the plant is very remarkably hindered when lime, potash, or the salts just named are present in the absorbed liquid. This fact, observed for the first time by Mr. Lawes, in 1850, and recently brought again more strikingly into notice by Dr. Sachs, of Tharand, Saxony, appears to be of great importance in the theory of manures. Dr. Sachs experimented on various plants, viz: beans, squashes, tobacco, and maize, and observed their transpiration in weak solutions (mostly containing one per cent.) of nitre, common salt, gypsum, (one-fifth per cent. solution) and sulphate of ammonia. He also experimented with maize in a mixed solution of phosphate and silicate of potash, sulphates of lime and magnesia, and common salt, and likewise observed the effect of free nitric acid and free potash on the squash plant. The young plants were either germinated in the soil, then removed from it and set with their rootlets in the solution, or else were kept in the soil and watered with the solution. The glass vessel containing the plant and solution was closed above around the stem of the plant by glass plates and cement, so that no loss of water could occur except through the plant itself, and this loss was ascertained by daily weighings. The result was that all the solutions mentioned, except that of free nitric acid, quite uniformly retarded transpiration to a degree varying from 10 to 90 per cent., while the free acid accelerated the transpiration in a corresponding manner.

As the processes of elaboration—the chemical and structural metamorphoses going on within the cells of the plant require time for their performance, we can easily perceive that a too rapid upward current of liquid, by diluting the juices, might measurably interfere with the assimilation of the food, and that the presence of a body may be no less useful by its regulating influence on the circulation of the water than by contributing an ingredient necessary for the formation of the substance of the plant itself.

It is also obvious that if a substance added to the soil retard the transpiration of water through vegetation, a given store of hygroscopic moisture in the soil will serve the needs of vegetation longer—will reach further into time of drought than it otherwise could. Dr. Sachs found that gypsum exerted the greatest effect in preventing loss of water, and this observation gives a scientific ground of evidence to the opinion long maintained among farmers, but rejected by men of science, (and very properly, as no cause could be discovered for such an effect, and the effect is not capable of measurement in

field culture,) that gypsum has the influence of a body that attracts moisture.

The facts brought to light by the researches of Way, Eichhorn, and Voelcker, already described, indicate another general mode by which fertilizers, especially soluble saline bodies, may operate indirectly. The investigations referred to, show that the bases (and acids?) may replace each other in insoluble or slightly soluble combinations, *i. e.*, soluble lime may displace insoluble potash, making this soluble and becoming insoluble itself. Soda may, in the same manner, displace lime or potash, or ammonia, the rule being that the body *in excess* goes into combination and expels those before combined. We observe here a tendency to bring all the bases into what we may designate as an equilibrium of solution. This principle appears adapted more than any other yet discovered to generalize the phenomena of indirect action, and enables us to forsee and explain them. Proofs are not wanting of the actual operation of this principle in the soil.

Wolff (Naturgesetzlichen Grundlagen des Ackerbaues, 3d ed., p. 148,) found in fact that the ashes of the straw of buckwheat grown with a large supply of common salt, compared with the ashes of the same part of that plant grown on the same soil *minus* this addition, contained less chlorid of sodium but much more chlorid of potassium, there having occurred *an exchange of bases in the soil.*

Closely connected in many points with these phenomena of displacement, yet in many respects different and peculiar, are the solvent effects of saline bodies, alkalies, and carbonic acid in dilute watery solution, to which allusion has been so frequently made in the foregoing pages. We refer to this subject once more in this place in order to give the results of some actual trials as to the disintegrating effect of these substances on soils and rocks. Dietrich, to whom we owe these investigations, found that from a diluvial loamy soil containing humus, the amount of matters rendered soluble by a dilute solution of carbonate of ammonia (containing one per cent. of the salt) was twice as great as that set free by water saturated with carbonic acid, and of the alkalies, potash and soda, four times as much were dissolved by the former as by the latter liquid. Solution of sulphate of ammonia dissolved six times as much as carbonated water.

The action of carbonated water and carbonate of ammonia extended chiefly to the alkalies. Sulphate of ammonia, while equally effective in their solution, likewise dissolved a large amount of lime and magnesia as sulphates. Caustic lime (one per cent.) in most cases produced a remarkable increase of volume in the earths submitted to its action; the loam just mentioned became nearly three times as bulky as it was at first, a decomposition of the silicates having taken place. Carbonate of lime, in solution in carbonated water, had the most vigorous action in eliminating the alkalies. Even gypsum, (sulphate of lime,) in moist contact with powdered basaltic rock, sets free a considerable amount of alkalies in a few days. Ammonia salts exert a strong action on insoluble silicates, the ammonia and silica being partially set free, the other acids and bases remaining in soluble combinations.

The most abundant, most generally employed, and most permanently useful manures are the excrements and waste of animals. These matters are, in fact, the residue, more or less concentrated, that remains from the oxydation of vegetables which have served as food. By the vital processes, the hydrogen and carbon of the vegetable nutrient principles are chiefly consumed to the gaseous form, while a portion of these, together with nearly all the nitrogen and all the fixed mineral matters, are separated from the animal in the liquid or solid shape, either immediately prepared, or under the agencies of warmth and moisture speedily assuming a suitable condition for nourishing a new vegetation.

The excrements of domestic animals, containing, as they do, all the ingredients of plants, and those in greatest relative amount which vegetation is obliged to seek for in the soil, constitute the most generally and durably efficient manure in countries like our own, where cattle are largely depended upon as means of supplying food. The dejections of man are a more concentrated and more powerful fertilizer, and though less adapted for maintaining the fertility of large farms tilled by a few hands, because they are not associated with matters that *amend* and modify the physical characters of the soil, are a main reliance in countries like China, where the dense population subsists almost exclusively on vegetable food, and under any circumstances are an invaluable adjunct to the resources of the farmer. Human excreta should never be suffered to waste so long as the soil is capable of stimulation to higher productiveness.

Certain animal manures, viz., those very rich in nitrogen, though usually exhibiting great energy of action, are liable to abuse, and often ultimately impoverish the farmer. Peruvian guano, the excrement of piscivorous sea-fowl, yielding sixteen per cent. of ammonia by the decomposition of its uric acid, and the flesh, blood, hair, and wool of animals are manures of this character. Nitrogen is their principal active ingredient; it passes into ammonia or nitric acid, excites a quick growth of vegetation by furnishing abundance of material for cell development, and at the same time rapidly solves the fixed minerals of the soil. The latter, being as rapidly removed by the vigorous vegetation, soon fall into a state of relative deficiency, especially on the poor soils where these applications exhibit their effects most strikingly; and unless restored by some other manure, the absence of them produces the phenomenon of exhaustion.

It is an objection, indeed, commonly raised against manures containing but one or a few nutritive ingredients, that they exhaust the soil. Obviously it is the crops, or what is taken off the soil, that exhaust it; and if a manure assists a crop to rob a field, the abetting farmer cannot rightfully complain, so long as the price of the produce goes into his pocket, although, to be sure, there are various ways of exhausting land, some of which are vastly more profitable than others.

The great practical lessons taught by experience and confirmed by science, relative to the use of manures, are, *save all refuse which contains any of the elements of vegetation ; apply abundantly the mixed ingredients of the dung and compost heaps.* As concerns commercial

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and saline manures, such as guano, salt, plaster, lime, &c., *experiment with them repeatedly and accurately on the small scale, so as to learn what the crops say about their value.* Where phosphates have been heavily applied, it is probable that ammonia or nitrogenous manures, or perhaps lime or potash, may next exert the most beneficial action, and *vice versa.* Be sure of *enough*, not only as regards the quantities, but also the kinds of matters applied.

But our subject requires treatment which only a volume can give space for. The recent progress of knowledge, thanks to the scientific farmers and agricultural philosophers of England, Germany, and France, demands a series of chapters on manures that are as yet unwritten, but, when rightly produced, will be alike novel, interesting, and useful to the true American farmer, who cultivates with equal assiduity the "soil and the mind."

LECTURES

ON THE SHELLS OF THE GULF OF CALIFORNIA.

BY PHILIP P. CARPENTER, OF WARRINGTON, ENGLAND.

The pearl fishery carried on by the Spaniards in the "Sea of Cortez" during the 17th and 18th centuries, bore testimony to its richness in molluscan life. To obtain the "pearl oysters," eight hundred divers were regularly employed, and the annual value of the exports was $60,000. So exhaustive was this fishery that it was gradually abandoned; and the very limited trade between the gulf ports and the Old World did not lead to more than the most fragmentary knowledge of its marine fauna. A few of the shells of Acapulco had been brought home by Humboldt and Bonpland as early as 1803; and collections had been made at various stations on the Central American coast by Captain Beechy and Lieutenant Belcher, R. N., in the voyage of the Blossom, 1825–1828; by MM. Du Petit Thouars, La Perouse, and Chiron, in the Venus, 1836–1839; and in the Sulphur, by Sir E. Belcher and Mr. Hinds, in 1836–1842. The shells of Panama and the coast of Ecuador, closely related to those of the Gulf of California, had been obtained in great abundance by Hugh Cuming, esq., whose vast collection of shells is not only by far the largest in the world, but, through the generous courtesy of its owner, the most accessible to students of every nation. Scarcely any shells, however, had been collected in the gulf, and indeed the records of scientific voyages, rich as they are in additions to our knowledge of fresh forms, rarely afford satisfactory data as to the fauna of any particular district. Unfortunately, it has been the custom, in the accounts of these voyages, only to describe the (supposed) new species; besides which, the locality marks, even if accurately noted at the time, are exposed to many chances of error before the information is made accessible to the scientific world.[*] Whether the shells of the gulf most resembled those of Panama or those of California, (which were described by Mr. Conrad from collections made by the late Mr. Nuttall in 1834,) was still a matter of doubt up to the period of the Mexican war in 1846–'8. When Major Rich and Captain Green visited Mazatlan, they became acquainted with a Belgian gentleman, M. Reigen, who had been employing himself in making a vast collection of the shells of that region. This collection ultimately passed into the hands of a merchant who

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[*] The works of Mr. Hinds are, however, in every respect reliable, in consequence of the great skill and accuracy of the lamented author.

divided it into two portions : the smaller was sent to Havre ; the larger, occupying no less than 560 cubic feet, to Liverpool.

A collection of such magnitude, known to have been made only at one spot, had never before been thrown open to the public ; and, knowing that its contents were likely to afford very valuable informa- tion in reference to the geographical distribution of species, I em- braced the opportunity which circumstances afforded me to pass the whole under careful review. The result of my labors will be found in a "report on the present state of our knowledge of the mollusca of the west coast of North America," prepared at the request of the British Association for the Advancement of Science, and published in the volume of transactions for 1856 ; and, in a more detailed form, in the "descriptive catalogue of the Reigen collection of Mazatlan mol- lusca" printed by order of the trustees of the British Museum, 1857.* The best duplicate series, amounting to about 6,500 shells, I have lately given to the State of New York. Having come to this country to arrange it in the natural history rooms at Albany, Professor Henry requested me to visit Washington, and arrange the shells of the United States exploring expedition. For this difficult task, the sorting out of the Mazatlan shells, amounting probably to 100,000 specimens, was perhaps no unfit preparation.

In the present lecture, it is proposed to confine our attention to a single shell from this collection. It belongs to a group nearly related to the oysters, and still retains the name of *Spondylus* given to it by Aristotle more than two thousand years ago, from the resemblance of the thorny processes outside the valves to the vertebræ of the higher animals. I have named the species *calcifer*, from the use made of it by the natives, who dive for it in order to burn for lime. Its solid, ponderous growth affords a striking contrast to the great "water-clam" of the Pacific islands, in which the shell-layers are generally separate from each other.† Unfortunately the cumbrous size of these shells led the Liverpool dealer to dispose of the whole stock before I had an opportunity of examining them ; their ignominious fate being to adorn the "museum" of a large drinking saloon, the owner of which had no idea of their scientific interest, and was unwilling to part with any of his duplicates. The very few which fell into my possession proved, however, to be a little museum in themselves; each specimen so abounding in parasites, within and without, that I have described upwards of a hundred entirely new forms of molluscan life derived from this source alone; besides about 250 others which had been pre- viously investigated, or which are not yet determined; and a variety of Annelids, Crustaceans, Zoophytes, Sponges, Protozoas, Protophytes,

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* Both of these works are in the library of the Smithsonian Institution. In order to aid in their compilation, Mr. Herbert Thomas purchased for me what remained of the Reigen collection. The first fruits of this, amounting to nearly 9,000 specimens, I presented to the British Museum.

† A very remarkable specimen of this shell was brought home by the United States ex- ploring expedition, in which the free as well as the attached valve displays the long, flat, triangular ligament area, presenting somewhat the appearance of the gigantic fossil *Pla- giostomata*.

and algæ, which are yet awaiting the attention of naturalists acquainted with those special departments. We propose first to examine the creatures which make their abode on the outside of these oyster valves.

Certain smooth, oval spaces bear testimony to the former presence of many kinds of limpets. Some of those creatures, (as e. g. the *Patella Mexicana*, or giant limpet, which is sometimes a foot in length and large enough for a basin) prefer to live on the rocks ; others are always found on dead shells ; others again always adhere to living ones. The circulation of water caused by the breathing currents of the larger animal is no doubt congenial to their tastes. Most mollusks have the power not only of forming, but also of absorbing shelly matter ; and these limpets, by the constant action of their strong muscular foot, eat into the shell of the spondylus and leave a mark by which each species can generally be recognized. Some of them make regular excursions to browse on the algæ and nullipore which they rasp off with their thousand-toothed lingual ribbon, always returning to their own hole to sleep ; but others appear to lead a sedentary life, depending, like the bivalves, on whatever nutriment the water brings within their reach. These, which go by the common names of "bonnet," "slipper," or "cup-and-saucer" limpets, are more highly organized than their more active neighbors ; the gill being a delicate little comb at the back of the neck, and the sexes being distinct. The Calyptræids ("slipper" and "cup-and-saucer" limpets) found on the Spondylus valves are the most beautiful and varied that are known in any part of the world. The shells are large and thin, delicately furrowed, and as it were engine-turned with a profusion of tubercles, which sometimes rise up into long hollow spines. The colors vary from white to a rich black-brown, or are variously mottled with sienna, while the shape may be either an elevated cone or a widely spreading disk. Sometimes the same individual will begin with one form and sculpture-pattern, and suddenly change to another ; others again seem to develop permanent and widely differing varieties. Occasionally a starved or diseased Mazatlanian will present the aspect which is normal on the colder shores of South America ; exchanging its thin texture and delicate sculpture for a coarse, solid, and nearly smooth shell. So far the views lately propounded with such ability by the celebrated author of the "Voyage of the Beagle" meet with sufficient confirmation ; and yet, amid all its changes, there is a *habit of growth*, hard to describe and yet easily recognized by the practised eye, which not only unites the most aberrant forms, but at once separates them from neighboring species found on the same coast and appearing very similar to the common observer. The ordinary plan of only preserving in collections a few picked specimens displaying marked peculiarities, is by no means favorable to the elimination of truth in reference to specific variation. These extreme forms are very naturally described as distinct species, the intermediate connecting links not passing before the view of the naturalist. On showing to a distinguished author a carefully eliminated suite of Mazatlan specimens connecting the smooth, thin, flat Crepidula squama, *Brod.* with the

coarse, arched, laminated C. Lessonii, passing through the forms C.
nivea, *C. B. Ad.* and C. striolata, *Mke.*, he complained that I had "kept
all the puzzling shells." In the very useful work of Messrs. H. and A.
Adams on the genera of recent Mollusca, these forms appear under
different subgenera.* It is not fair to blame authors for these mis-
takes, which naturally result from the imperfection of the material on
which they work. But the prevalence of such errors should lead us
to embrace every opportunity of studying large numbers of specimens,
both from the same and from different localities. Patience, accuracy,
and honesty may thus render as valuable service to science as brilliant
genius, and may supply the materials from which some master-mind
may hereafter develop the most important generalizations.

Those who describe species from minute differences founded on in-
dividual specimens, might do well to study the plates appended to
the "B. A. report on the West Coast Mollusca" before quoted. Take,
e. g., the *Crucibulum spinosum*, pl. 9. The shell is at first spiral, like
a snail. It then surrounds its entire margin with a rim, which is the
first beginning of what in the adult becomes the "saucer," or outside
shell; that is, the *hardened skin of the animal's body;* (for shells are not
to be regarded as a *house* constructed for the animal to live in, but as an
integral part of the animal itself, like the feathers of birds or our own
nails and hair.) At the same time it raises a slight lamina from the
labium, or "pillar-lip," which ultimately becomes the "cup." At
first, however, it is like the "deck" in the slipper limpets, from some
species of which it can scarcely be then distinguished. The Crepidulæ,
however, continue their deck in a horizontal direction, while the Cru-

Fig. 1.

Crucibulum imbricatum, jun.
Interior view, showing the cup lamina
beginning to double in.

cibulum turns the edges upwards at a more
or less obtuse angle. Gradually, during the
progress of adolescence, this angle becomes
right and then acute ; the outer shell mean-
while taking various forms, round, oblong, or
irregular, according to the nature of the sur-
face to which it has chosen to adhere. Often
this immature state is continued to a late
period; if permanent, it would belong to the
subgenus Dispotæa (*Say*) of Messrs. Adams.
But, normally, the sides of the cup close in,
while its body becomes greatly swollen in
front. This cup now assumes the form which
is always characteristic of the species under every modification of ex-
ternal growth ; being well rounded in C. imbricatum, angular at the
side in C. spinosum, and with the sides flattened against each other
in C. radiatum. In C. rude, the adolescent stage is very soon com-
pleted, and the cup is permanently detached from the side of the
shell, forming a veritable "cup and saucer," one, too, after the fashion
so prevalent in America, where the cup-handle has never been formed.

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* The plan adopted by D'Orbigny in his classification of Foraminifera, was to pick out
from a large mass of material the leading forms ; which he grouped into genera, families,
and orders. In my brother's papers on Orbitolites, &c , in the transactions of the Royal
Society, it is shown that individuals belonging, according to D'Orbigny, to different *orders*
are really aberrant forms of the same *species.*

It is a remarkable fact in geographical distribution, that the forms *imbricatum* and *rude*, which are typical in the west tropical fauna of Central America, reappear, but very sparingly on the Caribbean shores; while C. spinosum, which is far more common, more variable, and more widely distributed, being found (under various names) from California to Chili, has not yet been discovered on the eastern side. Again, the C. radiatum, which is the most delicately formed of the whole group, confines itself to the equatorial western seas, not having been found further north than the Panama district.

An extremely remarkable specimen of C. spinosum was dredged by Mr. Cuming in comparatively deep water. The net brought up a large stone with a small hole in it, on looking down which Mr. C. perceived a number of spines as though a sea-urchin was lodging there. A blow of the hammer discovered the existence of a large cavity within, communicating with the external world only through this narrow opening. In the hollow of this cavity lay the limpet, turned, as it were, nearly inside out. The creature had gone to live there when young, and being of sedentary habits it did not occur to him that he might be imprisoned for life by his own corpulence, else he would probably have made his escape before he had grown too large. As it was he grew larger and larger, and as the walls of his prison rose up round him on every side, he was obliged to flatten out his shell till it became a plate instead of a cone. At the same time, his body protruding into the hollow, the cup protruded along with it till it stood considerably beyond the shell, of which it was normally an inside partition. Thus our Calyptræid was fixed as immovably as any Pholas, but with this difference in their condition: that the Pholas, being designed for that kind of life, is not troubled with use-less head and eyes, and, moreover, is furnished with two long pipes to convey the water to and from the mouth and gills; while the Cruci-bulum had eyes simply to stare at the wall in the dark, feelers to push the stone, and a long ribbon tongue, armed with hundreds of teeth, to rasp the water. And while encumbered with these unneces-sary appendages, he had not the benefit of water pipes to bring what alone this lock-jawed subject had to feed upon. For this want, however, the economy of the animal provided a remedy. The C. spinocum, in its normal growth, is either spinose or not; the flatter forms being almost always smooth. The spines are developed from prolongations of the mantle, (or thin shell-bearing skin of the animal,) which appear at irregular intervals, though in a regular pattern. Sometimes the whole shell is covered with crowded prickles, (C. his-pidum, *Brod.*,) sometimes a very few long spines appear at the edge on one side of the otherwise smooth shell. Sometimes the spines are few, large, and hollow, (C. tubiferum, *Less.*,) each of the outer row communicating through a hole with the inner margin, which is after-wards filled up. Our prisoner worked for his living by constructing very large, long, and open spine pipes, which, instead of standing up at right angles to the shell, were directed back towards the narrow opening in the stone. It would appear that by this means the animal was amply supplied with nourishment, for the shell was above the ordinary size.

The most common Calyptræid on the backs of our Spondylus valves, however, was Crepidula aculeata, *Gmel.* It was first described from West Indian specimens, which are generally dead and worn, in collections, and afterwards re-described from fine West Coast shells as C. hystrix and C. echinus, *Brod.* The stunted Northern form was named C. Californica by Nuttall. The rule is laid down by some American authors of great celebrity that no species can be common to the Atlantic and Pacific waters. Accordingly, when the same form reappears on the wrong shore, it is their custom to re-describe it, there being always differences by which a few individuals can be separated from each other. But it is well known by those who have examined extensive series from different localities that each locality may present the same species under very different aspects. A large number of British shells live also in the Mediterranean, but in a mixed collection it is generally easy to pick out northern specimens from their southern congeners. So again the Panama shells (of identical species) can generally be separated from the Mazatlan; and these again from those of Acapulco and Cape St. Lucas. Now if the east and west coast shells do not differ more than those of Panama and Mazatlan; nay, do not differ so much as those of either place among themselves; it appears an *argumentum ad ignorantiam* to describe them as distinct species, merely because *we* cannot tell how they have become distributed. On comparing Dr. Gould's descriptions of Purpura pansa (Pacific) and P. patula, (West Indian,) with my own well authenticated specimens, it appeared to me that the diagnosis of patula was exactly fitted to the Mazatlan shells, while that of pansa belonged rather to the shells collected by my brother at St. Vincent's. Our knowledge of the fauna of each region is as yet too meagre to speak on doubtful matters with any dogmatism, but the researches of modern geology have already determined the fact that in the tertiary (Miocene) epoch there was a communication between the two oceans; that very remarkable Pacific shell, Malea ringens, having been found fossil on the Atlantic coast. This interesting solution of a doubtful problem is due to the research of Dr. Newberry, and is an instructive example of the light which different branches of study throw upon each other.

We may now be allowed to predicate that old species, which have survived since the Miocene epoch, may be expected to appear on both sides of the peninsula; while those of modern creation may be expected to be distinct. Furthermore, the old species may be expected to have more power of living under varied influences, and, therefore, to be more variable in shape, and more widely diffused than those more constant and local in their characters. The history of British shells, which are more thoroughly known than those of any other district in the world, furnishes many instructive instances of these facts. In Mr. Searles Wood's work on the Crag Mollusca* the newer tertiaries are divided into the Coralline crag, the Red crag, and the Mammaliferous crag, (answering perhaps to the Miocene, Pleiocene,

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* Published in three parts by the British Palæontographical Society, of which a copy is in the Smithsonian Library.

and Pleistocene of American authors,) in each of which we have
species represented still living in the same seas or in the Mediter-
ranean or Boreal districts.   If the species is in mature vigor, it may
still be found widely diffused.   If, on the other hand, it be dying out
in its general area, it may preserve a lingering existence in very
remote localities which once were connected.   Thus the Orbitolites
of the Paris basin is still living in the East Indies, although now
unknown in European seas;. while the common gulf weed of the
modern Atlantic is believed by Prof. Forbes to be a further develop-
ment of the very same plant which floated (as now) in huge masses
in the ancient ocean of the Eocene.

When the tertiary fossils on each side of the Rocky mountains
shall have been thoroughly explored, when the age of these moun-
tains in the narrow isthmus shall be better understood, when the
deep waters of the Gulf of Mexico and the Pacific coast shall have
been well dredged, we may be in a position to speak with confidence
on the points of similarity and of contrast in the two oceans.   At pres-
ent we can do little more than accumulate facts for future explanation.

In the case of Crepidula aculeata, however, the perfect specimens
brought by Mr. Dyson from Honduras correspond so exactly with
those from Mazatlan that it is hardly possible to resist the impression
that they are identical.   Specimens from South Africa, from Sydney,
(Australia,) and from the Pacific islands also present no marks of
specific distinction.   It appears to be one of the ubiquitous species,
of which several are found in various genera, and some are known to
have existed far back in time.   Of this number is Saxicava arctica,
which has been found in all the three epochs of the English crag; is
now flourishing in the boreal as well as the temperate regions of Eu-
rope and America; has been found in the China seas and in Australia,
(C. testo, *Forbes*,) and attains respectable dimensions in the cavities of
our Mazatlan Spondylus.   The Crepidula not only undergoes the
changes of form from nearly flat to deeply arched, from obese to elon-
gate, which every observer of the common slipper-limpet of the At-
lantic (C. fornicata, abundant from the icy shores of the St. Lawrence
to the tropical waters of the Gulf of Mexico) knows to prevail in that
species; but in sculpture it may either be crowded with short spines
(C. echinus, *Brod.;*) or have a few radiating lines of longer spines with
nodulous interstices (C. hystrix, *Brod.;*) or be covered with an irregu-
lar mass of spiny knobs (normal state;) or lose the spines altogether
in roughened striæ (smooth-water form;) or even become almost des-
titute of sculpture, like some northern specimens of the stunted va-
riety (C. Californica, *Nuttall.*) Through all these changes it is recog-
nized by its spiral stomatelloid growth, exemplifying a section of the
genus the extreme forms of which approach Trochita; and by its
beautifully waved deck-margin, which resembles a ⌣.   The
pointed centre, as the shell increases in size, generally leaves a char-
acteristic line on the surface of the deck, passing up to the vertex. But
often the point is rounded off, and even degenerates into a broad wave.
In one specimen, co-ordinate with this degeneracy, a sharp angle was
abnormally formed on one of the sides, so as to give the margin the

aspect of a brace turned the wrong way—thus ⌣⌢ ; a very good specific distinction, if no intermediate specimens had been found. A series of deck margins, belonging to this and the following species, will be found represented on plate 8 of the British Association report, figs. 1 *f.*, 3 *g.*

Fig. 2.
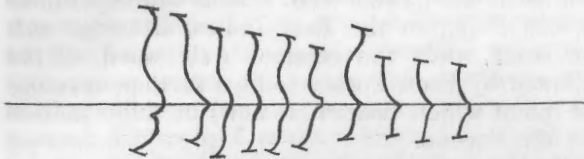
Deck margins of Crepidula aculeata. The straight line represents the situation of the medial rib.

The best means of distinguishing the species of slipper-limpets from each other was found to be the shape of the nuclear portion and the mode of growth of the very young shell. Whatever be the abnormal character of the adult, it did not appear that the offspring had a tendency to the same degeneracy, but rather to the resumption of the normal type. In the case of local varieties, the peculiarities are reproduced, because they depend on circumstances which affect all alike. But in such cases as those under consideration, where the extremes and all the intermediate forms of variation are found in the same locality, *the changes depending on the accidents of the individual,* it is not yet proved that the idiosyncracies are transmitted. In fact, the frequent instances in which the individual itself changes its form and sculpture at different periods of its life is against such a hypothesis. In the higher animals, where there is, as it were, an innate vital power shaped according to the species, with an additional power shaped according to the individual, and these powers are to no slight extent irrespective of the immediate external surroundings, there is a much stronger tendency in the offspring to imitate the parent—as in the black faces of the Southdown sheep, or the stump-tailed cats of the Isle of Man. This tendency on the part of the parent to reproduce *itself,* and even that particular phase of self which obtains at the periods of conception and gestation, culminates in man; who, of all animals, is the most independent of external circumstances. But, in the lower forms of life, the nature both of the species and the individual becomes more and more plastic, responding to the accidents of the moment; there is accordingly proportionally less of the innate power which leads to the transmission of variety. As instances of this plasticity the reader is referred to Dr. W. B. Carpenter's papers on Orbitolites and other forms of Rhizopods in the Transactions of the Royal Society of London.

It is a fact worth noticing, that while some species of shells are extremely variable, others, inhabiting the same localities, are very constant in their characters. These are seldom widely diffused, and are often rare in individuals. A few young specimens of such species were found among the slipper-limpets on the Spondyli; but the bulk of the specimens belonged either to C. aculeata, which, as we have seen, is a somewhat ubiquitous species, or to C. nivea, which, under many shapes and many names, spreads over the principal part

of the Pacific coast of America, representing there the very distinct*
C. fornicata of the Atlantic. Two extreme forms were first described
by Broderip, from Mr. Cuming's collection: the one, *C. squama*, thin,
flat, and smooth; the other, C. lessonii, solid, often arched, and cov-
ered with concentric laminæ. These sometimes appear at regular
intervals, and then seem to be the normal and unique sculpture of the
shell. It appears, however, that C. squama, (which is the calm water
form,) if exposed to rougher influences, arches its back, adds layer
after layer of porcellanous matter, hiding the color rays, and leaving
the margin like the edge of a quire of paper. Now if, co-ordinate with
this laying on of extra coats, the creature advances forward, turning
up the previous portion, the form Lessonii is produced: in general
very roughly and irregularly, which is the C. striolata of *Menke*, but
sometimes very delicately, with fine sculpture between the laminæ,
as described by *Brod*. It is common to find shells living for some
time as squama, and suddenly plunging into the Lessonii types, with
one or two strong laminæ. Every stage of intermediate form was
found among the Mazatlan shells. The degraded specimens of the
Chilian seas form a part of the C. protea of D'Orbigny—a convenient
receptacle, as the type specimens in the British Museum show, for
the dead and puzzling shells which the author did not know where
else to place. The ordinary condition, intermediate between the
extremes first described, is the C. nivea of *C. B. Adams*. As it is
the normal state, the usual rules of priority have been set aside, and
C. nivea taken for the name of the species, leaving squama and Les-
sonii for the principal varieties. The *White Slipper* is known under
all forms (when in good condition) by its shaggy, light-green skin,
and by the very peculiar character of the nuclear whirls. These are
remarkably small, though the shell is large, standing out from the
surface, of a reddish tinge, and crowded with regular transverse ribs.
The characters have
been observed in
specimens of all the
forms, although the
influences which pro-
duce Lessonii, draw-
ing the shell away
from the vertex, gen-
erally lead to its ab-
rasion. Sometimes
the White Slipper
goes to live, when
young, in the empty
burrow of a boring
mussel. In these
cases, as soon as it
has grown to the
width of its cave,

Fig. 3.
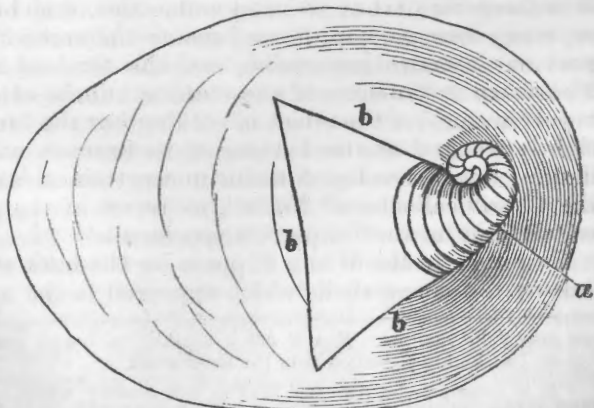
Crepidula nivea, *jun.*—Outside. *a*, nuclear spiral portion, ribbed; *b, b,*
margin of deck, seen through the transparent shell.

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* It does not follow, because certain aberrant forms from different localities resemble
a , , . , . t the species are therefore identical, if the normal state and general habit

it is obliged to develop itself longitudinally, at the same time turning up its sides in the vain attempt to get more room. The corresponding slipper limpet of the Californian coast appears to have a special fancy for this mode of life, as most of the specimens sent have assumed the form now described. It was first found by Mr. Nuttall, and distributed by him as C. exuviata. It was so published in Dr. Jay's catalogue. Dr. Gould, however, figured and described it as C. explanata. It had been previously figured by Valenciennes, in the Voyage de la Venus, as C. perforans, that author supposing that it had made the burrow in which it was found. The designation representing an untruth, it must yield to the latest name, which alone is accompanied by a description.* A very singular groove, not found in the Mazatlan specimens, appears in all the specimens of C. explanata, and gives name to the shell. It is, however, a mere accident of growth, differing in every individual, and often not appearing till the animal approaches maturity. A specimen, *in situ*, in the Smithsonian Institution fortunately reveals the cause of this unique appearance. The creature goes to live at the outer or pipe-end of the burrow of a bivalve,† which remains at the other end after the animal has perished. The growth of the shell is normal till it has attained the breadth of the pipe, be that greater or less. It then increases down the pipe, the vertex of the shell being always turned towards the outer end. There is no groove at this period of its growth; and when the vertex is rubbed off, (as it generally is in elongated specimens,) it can hardly be distinguished from similar specimens of the White Slipper. But as soon as it has reached the bottom of the pipe, where the dead bivalve (generally a Petricola, a creature with rather short siphons) still remains undecomposed, it suddenly encounters an unexpected obstacle. It wedges itself under this (to it) mighty globe, and turns its delicate mantle, exuding the shelly skin, up the sides of the new-moon-shaped cavity, but in vain. There is nothing for it but to retrace its steps, and back out. As it does so, every new portion, formed under the arched bivalve, repeats the previous concave impression, and the *Grooved Slipper* is the result. The sharp instrument of the "explanation" of one author, and the "perforation" of the other, is nothing but the little rounded "clam," tightly wedged at the bottom of its burrow; and the same slipper-limpet, freely developed under unconstrained influences, is probably the C. navicelloides of Nuttall, to ascertain the characters of which we are still in want of perfect specimens.

To return to the White Slippers on the back of our Thorn-Oyster. Among the young shells which appeared to the naked eye to be the

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are essentially distinct. Man is not a monkey, although certain unhappy idiots may appear less highly organized than the lower order.

   * It is greatly to be regretted that in this country, where type series, named by Mr. Nuttall himself, were readily accessible, his labors should have been so often disregarded. On the other side of the Atlantic they have frequently found their way into the monographs, but unfortunately too late for preservation.

   † These burrows will be found described at page 209, *et seq.*

young C. nivea were some which under the microscope displayed a much larger but smooth and *imbedded* nuclear portion. On comparing these with similarly situated specimens from the west coast of Africa and from other places, I found them exactly identical. They probably belong to the C. unguiformis of *Lamarck*. Now, it so happens that Professor C. B. Adams, who in general described every shell of Atlantic types as a new species, if found on the Pacific coast, in this one instance felt constrained to adopt the Lamarckian name for the unguiform slippers of Panama. It is not certain that in this one instance he was correct. Some of the specimens he distributed under that name are undoubtedly compressed and inverted forms of his own C. nivea; for every species may take the form of unguiformis when grown inside a dead spiral shell, especially with a hermit crab pressing against it. But there seems sufficient evidence to believe that while each coast has its special species of slipper limpets, each one of which assumes protean changes, there is this one species which has been scattered, it may be in dead shells and on ballast, round the world, and to be distinguished from all neighboring species by the peculiar character of the nuclear whirls. It is too much the custom among "collectors," and even among naturalists, to examine and preserve only well-conditioned adult specimens. More may often be learned from deformed and "ugly" shells; and especially from series in all ages of development.

We might easily fill the lecture with additional particulars concerning the slipper-limpets, but it is time to pass on to other matters. There is another family, the bonnet-limpets, (Capulidæ,) nearly related to the cup-and-saucer tribe, but without the peculiar internal appendage. Of these, two interesting species were found which appear to be peculiar to the West tropical American fauna; while others, the Hipponyx antiquatus, H. barbatus, and H. Grayanus, have a very wide distribution in one or both hemispheres.

Differing considerably in shape, but presenting remarkable points of similarity in the habits of the animals, are the Vermetidæ, (worm shells,) of which some interesting forms belonging to new types were found on our Thorn-Oysters. At first sight these shells would not be distinguished from the serpulæ, (shelly marine worms,) which are found adhering to almost every dead shell from any sea-coast. The shell-cases of both seem to crawl irregularly over the shell or stone to which they adhere; and while some of the serpulæ assume the regular spiral form of the Mollusks, some of the Vermetids assume a looseness of growth as great as that of any worm. And yet the animals which have exuded these similar habitations from their soft skins, are more widely removed, the one from the other, than lions are from snakes or fishes. The Serpulæ belong to the same subkingdom as the Insects and Crabs; and are, in fact, red-blooded worms with ring-jointed bodies, without head or eyes, and with the nervous system pretty regularly diffused. While the Vermetids claim kindred with screw-shells and Periwinkles, having their little heads, with feelers and armed tongue-ribbons, and their nervous power collected into irregular knotted centres. The true affinities with regard to

one species were long ago ascertained by Adanson, the very accurate though eccentric naturalist of the west African coast. Since that time the animals have been so far investigated that various genera have been established by Dr. T. E. Gray and others. The typical Vermetids begin life free, and so continue for some time, making a delicate spiral shell exactly like Turritella. They then begin to tire of their freedom, and long for some protecting support. They suddenly give up their beautiful spiral shape, and twist themselves anyhow in search of a secure foundation. Having moored themselves to it for life they writhe in shelly contortions, crystallized (so to speak) as soon as formed, during the remainder of their sedentary existence. Of this tribe, the Ivory Worm-shell, (V. eburneus,) furnished by our Thorn-Oysters, is perhaps the most beautiful species.

But the other Vermetids for the most part only show their connexion with spiral shells in the nuculear portion. These shells, in the Indo-Pacific ocean, generally have a deeply concave operculum,* with only a loose trace of spiral development. Of these, many specimens were brought home by the Exploring Expedition, unfortunately without the animal. But it was found that though the Gulf shells could hardly be distinguished from the Australian, their opercula most resembled those of Turritella. This was the more remarkable as the Turritelloid worm-shells have a very different operculum. The new group was named *Aletes*, the "Wanderer." It appears, however, not to wander from the west coast of America; those found from the Mediterranean to the Pacific islands (as far as known) all belonging to the concave type. Wandering among the Wanderers are some equally large and equally highly-colored shells with an operculum formed on a still more intricate pattern. From the horny plate rise up two long processes branching exactly like stag's horns. They are made, however, by a headless annelid, and are not so much worm-shells as shell-worms. The Annelids have generally an earthenware texture; while the Vermetids are porcellanous. But on breaking across some specimens of a very small species, closely though irregularly twisted, I was surprised to find a structure which had not before been described in any recent spiral shell. An extremely thin lamina, like the deck of the slipper-limpets, proceeded from one wall of the shell, doubled itself over at a right angle, and met a similar lamina proceeding from below, so as almost to divide the body of the animal into two parts one within the other.

a   *b*   *c*

Petaloconchus macrophragma.—Section across the shell at three periods of growth : a, one lamina first commencing, or ending; b, upper lamina prolonged, lower commencing; c, upper lamina doubled over to meet the lower, while a rudimentary one appears between.

° The operculum is a horny or shelly appendage to the end of the foot, drawn in last when the animal retires into its shell, and thus protecting it. It may be called in common language the trap-door or toe-nail.

This singular structure, to which there is some approach in the fossil genus Nerinæa, ran along most of the whirls, becoming evanescent in the earlier and later portions. On examining similar shells from other seas, I found species in all the principal zoological provinces, each characterized by a different growth of the internal laminæ. They had escaped observation before. I presume, because of the love entertained by collectors for "perfect" shells. Mr. Woodward, the author of the invaluable "Manual of the Mollusca," (Weale & Co., London; 3 parts,) enabled me to affiliate them to a genus established by Dr. Lea, (under the name Petaloconchus,) for a tertiary fossil of the United States, in which, however, the peculiar character is but slightly developed.

If time allowed we might dwell on a number of other interesting shells which were found either living on the backs of the Thorn-oyster shells, or accidentally lodged between the foliations of the valves. We must confine our attention to a few. Among them were eleven species of Chitonidæ or Woodlouse shells, of which eight were new. This strange family of Mollusks, while agreeing in many essential particulars with the true limpets, present some curious points of analogy with the articulated animals, having their skin-skeleton broken up into joints, and exhibiting a symmetrical and double arrangement of the organs of the body. Among them was one specimen of extraordinary beauty, though not much more than the tenth of an inch in length. Under the microscope each of the valves displayed a very elaborately ornamental sculpture, richly tinged with green, purple, pink, and brown; their shape, with the pointed beak and the transparent wing-like processes at the side, bore no very fanciful resemblance to a bird in flight; while the thick skin in which they are imbedded was covered with minute transparent prisms, and at regular intervals with what the microscope revealed as clusters of white crystals, glittering in the reflected sunlight like the finest specimens of arragonites. The same species has just been sent to the Smithsonian Institution, adhering to similar shells from Cape St. Lucas, by that indefatigable collector, Mr. Xanthus.

Sheltered from rough usage between two layers of shell was a new form of Isognomon, which may be called, in English, the "shoulder-of-mutton shell with the double face;" having one of its valves smooth, while the other has beautiful radiating lines covered with imbricated scales. These creatures, along with the *Pinna* shells, (which, like the Spondyli, have preserved the same name since the days of Aristotle,) the Hammers, and the Pearl-oysters, moor themselves to fixed objects by a *byssus* or anchor cable of their own spinning. Whenever the most minute fragment of shell belonging to any species of this family is examined under the microscope it always presents a prismatic cellular structure, like basaltic rocks in miniature, or like the "float" of a belemnite. When a large old Pinna has long been exposed to the action of the weather, its surface will crumble into these prisms in the hand. They are formed by the breaking down of a longitudinal row of ordinary cells, like the ducts of plants. It appears that all shells are originally formed by the aggregation of cells in the

same way, but it is seldom that they are so distinctly marked. This affords a safe clue to the true affinities of certain Aviculoid shells found in the palæozoic rocks, among the earliest forms of life in the Lamellibranchiate or Plate-gilled class. They are uniformly nacreous within, and it is found that the pearly lustre is due to the minute and irregular corrugation of the extremely thin film of animal matter which separates each layer of the shell ; this membrane preserving the same pearly lustre after the shelly matter has been removed by acid, but losing it when the corrugations are pressed out between two pieces of glass.*

Another very rare and remarkable bivalve found sparingly on the Thorn-Oysters, was the Placunanomia pernoides, a transition form between the true adhering oysters and those which fasten themselves by a solid plug passed loosely through a round hole in the shell. I had long known this species, (which is so different from any other that a distorted individual was described as a new genus by Dr. T. E. Gray,) having observed it on the back of some very large oysters, of which a large supply was sent to the Bristol Institution, by the captain of a ship engaged in the West African trade. There is no doubt whatever that they came from the Senegambian coast. The oyster itself, (O. iridescens, *Gray*,) possesses very distinctive charac-ters—a rare thing in that genus ; and other specimens from the same coast are preserved in the British Museum. To the disturbance of the prevailing theories on geographical distribution, I found the same gigantic oyster among the Mazatlan shells, accompanied by the same Placunanomia, and by a boring mussel, (Lithophagus aristatus,) which is abundant on the warmer western coasts of Europe and Africa. I believe that neither of these shells are found in the Caribbean sea ; and this is only confirmatory of other evidence, that just as some forms of life are peculiar to islands, not being found on contiguous continents; so others may have been created with a special adaptation to coasts facing the west, while others prefer the currents from the east. Those to whose great labors and critical acumen we owe the present state of our knowledge on the distribution of forms of life in time and space, have perhaps sometimes considered principles as established which rest on, as yet, insufficient data. It becomes us to pause before we arrive at conclusions. One little fact, like the finding of the fossil Malea ringens on the Atlantic side of Central America, may open the door to a new course of research, rich in results, important alike to geology and to recent zoology, and at the same time close it to very ingenious theories that have before been considered unassailable. Our principal duty, in the present state of our knowledge, at any rate in Malacological science, is the patient, thorough, and honest investi-gation of facts ; guided, indeed, by previously developed theories, or by those which we are ourselves eliminating, but in no sense con-trolled by them. And in doing so, it is only a false modesty or reverence for authority which would prevent us from following the advice

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* Full particulars on the microscopic structure of shells will be found in Dr. W. B. Car-penter's Report to the British Association, 1844, p. 1, *et seq.*

repeatedly given by the late Professor E. Forbes to a beginner, "follow your own judgment;" or, as expressed in the phraseology of an American politician, "be sure you are right, then go ahead."

But let us leave the surface of our Thorn Oyster, and examine what lies hidden in the ponderous substance of the valves.   If the outside swarms with life, it is only the portal to the vitality which teems within.   We will not speak yet of the worm-eaten passages, the entrances to which often riddle the external surface, making it look like solid sponge, but we will direct attention to certain ominous-looking holes, bearing the same relation to the oyster that the entrance to the great cave does to the State of Kentucky.   (We may be allowed to compare small things with great, for in nature all small things are in one sense great; in another, all great things small.) These cavern-mouths are of various shapes, but evidently not irregular. Some of them are always round, others always oval, others 8-shaped. Out of some of the windows, opened but a little way, may be seen protruding a pair of stony lips, belonging to a mouth so straight in outline that, according to physiognomical diagnosis, its possessor must be of secretive turn of mind.   The diagnosis is true.   Other mouths protrude from other holes, but with bird-like bills projecting, duck-shaped, as though to gobble the delicate morsels of the sea, or long and twisted, as in an attitude of defiance.   But some display neither lips nor beaks; which after all have no connexion with mouths, but rather with noses, as will be presently explained.   Let us take our glass, and for once imitate King George III, as described by Peter Pindar, and stare down the vacant holes "like a magpie peeping into a marrow bone."   The wise bird can see nothing but darkness.   As the awkward bulk of our bodies precludes our entering the dark chambers of the cave with lighted lamps, we must find other means of exploring the penetralia.   Let us set to work like geologists, with hammer and chisel, and not grudge an hour or so in observing how the creatures of a former generation spent their lives, like the Kings of Egypt, in making their own sepulchres.   Carefully we remove layer after layer of the oyster shell, and lay bare the underground passage.   Its floor and sides and roof are all wrinkled, presenting in miniature the appearances often caused by the running water in the great caves of this country.   It is not water, however, that has so regularly roughened these.   And why is it divided all along, like the Thames tunnel, by a stalactitic ridge along the upper surface, almost meeting a similar layer of what might be thought stalagmite rising from below?   Suddenly it makes a turn.   We have chiseled through the colored portion of the shell, which we find riddled and compara-tively soft; now we are on a bed of solid white marble.   Indeed, as we try our chisel against it, we find it much harder than marble or even than the ancient limestone.   Life, though it be that of the tender oyster, can build more solid structures than the ocean, with its mighty force of waves.   We are obliged to spend some time in chiseling shafts, levelling mounds, and preparing a field of operations to follow the new direction.   The bipartite rugose tunnel suddenly widens, and we find ourselves in a spacious cave, beautifully smooth and regular.   Its shape

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is oval; and loosely reposing within is a bivalve shell of peculiarly
graceful shape and delicate sculpture, with abundant room to open
and turn round at the pleasure of the animal.   This is a "clam," with
very long projecting siphon pipes, which fit into the two lobes of the
long passage.   They are covered with a rough skin, which produces
the wrinkles on the surface of the shelly layer.   One of these siphons
is constantly drawing water into the gill cavity, while the other carries
back the waste.   When in the cavity, the delicate frills which form
the "plate-gills," characterizing the class Lamellibranchiata,* float
loosely in the water, aerating the blood in its intricate labyrinth of
veins.   At the same time the large, flapping lips, which are so enor-
mously developed in the bivalves, (like the nose processes in the bats,)
move through the water, tasting its infusorial contents, and choosing
from among them what they shall convey to the mouth, whence a
highly organized digestive, circulatory, and generative system is at
work to transform the animalcules into molluscous life and shell.   The
animalcules thus transformed excavate these wonderful caverns; how,
we shall presently inquire.   Here is a living creature entombed from
its earliest days, or rather voluntarily entombing itself; hidden from
view and from the society of its kindred; maintaining no more con-
nexion with the outer world, except at the pipe ends, than a fossil in the
palæozoic rocks; and yet see how its wants are all provided for by this
one exception.   That little 8-shaped hole maintains within a structure
of such delicate beauty, with its tissues, nerves, blood-vessels, secre-
tions, respiratory, and reproductive apparatus, that the due description
of them would require a volume to itself, with an atlas of plates requiring
the utmost skill of the artist, as well as the most delicate manipulation
of the microscopist.   Although at the end of a long and often twisted
gallery, how fresh is everything in that inner chamber!   The best
cleaned dwelling room in regal palaces is impure by contrast.   No
flies make spots upon the ceiling, or mice leave their unfragrant odors
behind the wainscot; no closing of windows retains the impure air,
or sting of mosquitos disturbs the equanimity of Gastrochœnoid exist-
ence.   If our solitary friend has not the pleasures of eyesight, he never
dreads to see his enemy; nor has he once suffered from ache in head,
tooth, or ear.   He has an inner light of happiness, though his body
dwells in the profoundest darkness.   He has no more trouble than a
child for the supply of his temporal wants.   The all-pervading care

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* The name Acephala has precedence, but expresses nothing distinctive.   All the lower
classes of Mollusks and Articulates, as well as the whole of the Radiates, are destitute of that
to us necessary appendage.   The *proper name* of every individual (the genus and species)
should follow the modified law of priority.   But classification is a matter of opinion, and
must change (with the nomenclature founded on it) with advancing knowledge.   The name
Conchifera was applied by Lamarck to the Lamp shells as much as to the Cockles.   When
these are divided into two classes, it is well to find names which sufficiently express the
main differences between them.   The Terebratula breathes through pores in the surface of
the mantle, while the Anomia, which Linnæus (following the best light of his time) placed
in the same genus, breathes by overlapping laminæ.   Blainville's names, Lamellibranchiata
and Palliobranchiata, exactly express this difference; while, as in the case of the Dibranchiate
and Tetrabranchiate Cephalopods, these gill differences are co-ordinate with others of great
importance in the whole economy of the animals.   The name Brachiopoda is very good,
though it does not bring out the contrast.

of the mighty Father places what to him is the choicest food before his very mouth. He does not cry and suffer hunger like the young lions, but, like some mentioned in the Scriptures, he does not want any good thing. He has the pleasures of childhood, of youth, and of mature life. He emerges from the maternal egg, and finds himself swimming about in the mighty ocean. He lies in his transparent cradle, like the hollow of two fairy hands joined together to nurse the little stranger, and enjoys the opening and shutting of his tiny valves as much as any infant catching at the moon. His mother's rest is disturbed by no plaintive wail, nor is her life shortened by ministering to its diseased wants. Our little Gastrochæna sails on in the ocean of life with a literal placidity, realizing the most perfect descriptions of the novelist or the sweetest dreams of the sleeping child. The clairvoyant is said to have his eye-sense diffused over the membranes of his body; so does our infant see with the tissues of the fairy mantle in which he lies enveloped. No storms ruffle the tranquillity of his temper; no sudden frost cuts off his early bloom. He breathes, he eats, and performs all the other functions of life, as it were, unconsciously, simply happy in being alive. How the Lord has filled that mighty purifier which covers three-fourths of the surface of our globe, even in the darkness of its depths, with life and enjoyment! But the happy days of childhood are crowded into a few short hours; fulfil their term, and pass. Ah, young Gastrochæna, thou art tired so soon of freedom? Like the slave foolishly tempted away from his master under the glittering idea of liberty, thou hast tired of that bauble, and art going back, a willing captive till thy death? "Not so," answer the instincts that have been slumbering in that transparent form; "but I have a purpose to fulfil in life; I must work." And pray what art thou going to do, thou tiny living skin? "I must dig; I must riddle out those living rocks that are growing up beneath me, and threaten to choke up the very channels of the harbor. But for me and such as I, even man may hereafter be stopped in the mighty works which he carries on in the divine image. But for me and such as I, his vessels, fraught with the material uses of his fellows, and with the evangelists of eternal truth, would be wrecked on sunken rocks where his charts described old soundings of sufficient depth." Thy idea is grand, thou floating jelly; but how wilt thou accomplish these great things? "He that implanted such ideas within me, will He not work through me, and enable even my frail substance to accomplish the allotted portion of the task? I must dig. Behold my *foot!*"

As we look through the glass in our aquarium (that is to be, when the school of science is established on the shores of the sea of Cortez,) and see the tiny creature turning towards us the wide pear-shaped opening in its furrowed valves, and from a chink in the protecting mantle protrude a still more tiny, finger-like organ, calling it its "foot," the unbeliever might be tempted to scoff, and even the reverent student of nature to doubt its powers. But our infant Gastrochæna pays no heed, and steadily sets himself to fulfil his mission. Being heartily tired of a mere sportive existence—well enough,

for a time, that the species may become properly diffused—he now seeks a home of work and rest. He swims to the bottom, and alights on the oyster bed. Admiring the many caves already disintegrated by the action of other living creatures, he chooses one for his abode. If no such cave exists, he shelters himself behind a corner and sets to work to make one. Fancy a man on a desert island planting himself against a rock, with a deliberate purpose of hollowing out a cave to dwell in by rubbing with his hand or foot! And yet this is what the young Gastrochæna proposes to himself, when the "days of helpless infancy" are hardly ended. And he *does* it. Those who have witnessed the polishers of fancy marbles smoothing hard stones by rubbing them in their hands may form some idea *how* the Gastrochæna does it. A soft living tissue is always stronger than a hard dead one. The coral-polype weathering the stormy breakers proves how a feeble vital force may resist a mighty energy which is only mechanical. Long as the boring bivalves have been known and studied, their mode of operation is still matter of conjecture. But the study of our oyster burrows threw some light on the dark places. If the account here given be incorrect, let naturalists, with the marine aquaria that are to be, correct it. It used to be supposed that they dissolved away the rock by secreting acid; but not only was the said acid never detected among the secretions of the body, it was also suggested that the same solvent which destroyed the calx of the cavity would also destroy that of the shell itself. The hypothesis, however, answered sufficiently well so long as the borers were only found in shells or limestone rocks. But at last they were found burrowing in hard, primitive silex, which no acid could touch.* It was then suggested that the animal spun itself round and round, like a tetotum, or backwards and forwards, like an awl, and so filed away the matrix. The delicate imbrications on the valves of the Pholas tribe seemed to favor this view; but even here we should expect to find the fragile file worn out by rubbing, which is not the case, the points in well-conditioned specimens being as little injured as the remainder of the shell. Moreover, it is the open, gaping portion of the shell that is turned first towards the resisting medium. Besides, even if the Pholads were proved thus to bore, the Gastrochænids are generally devoid of sculpture, except lines of growth; the Lithophagi are often so twisted that they cannot move round in their holes; and the burrows of many of the Petricolidæ are heart-shaped, like the valves. If not the only agent of disintegration, I am convinced that in all cases the foot is the principal weapon of attack. This is known to be often strengthened by silicious particles, and I found its dried remains, as hard as horn, in adult burrows of (I think) every species of borer found in the Spondylus. This organ, which forms the principal part of the solid substance of the animal in all the burrowing and leaping tribes, is variously modified either to scoop out soft sand with

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* A very beautiful specimen of Pholas in its flinty home will be found in the State Museum, at Albany.

astonishing rapidity, as in the Razor shells; or to crawl slowly, as the River Mussels; or to spin an anchor cable, as the true Mussels; or to jump, as the Cockles.* In the Borers it answers the purpose of grindstone, scraper, and polisher. In Gastrochæna its comparatively small size is compensated for by the freedom with which the creature can move round and round in the capacious chamber. It is supported by a beautiful system of muscles which are moored to the fulcrum of the shell, and the nutritive material is abundantly poured into it to supply the waste. Our little tunneler sets to work with all the ardor of youth. His feeble finger, more delicate than any lady's, and as little used to toil, handles the rough surface of his cave, presses and rubs, rubs and presses, and finds the occupation as congenial to his instincts as whipping a wooden horse is to a little boy. His skin hardens with exercise. The invisible animalcules contribute their quota of silex and cartilage. The work of life has commenced in earnest. Would that we men, who have offered to us the grandest and the noblest destiny, had but one small fraction of the untiring perseverance of these headless and uncared-for hermits. How often we talk of drunkards and debauchees reducing themselves to the level of the beasts. It is a libel on the brute creation. *They* fulfil their mission. Even the little Gastrochæna is obedient to the will of the Lord, and is the instrument to accomplish his work. It is man alone that is disobedient.

The work progresses. The terrestrial changes of day and night do not reach the ocean-covered cavern. But still, in the absolute darkness of that solitude, tired nature craves and finds her stated intervals of repose. The Gastrochæna draws its finger-foot within its mantle, like the squirrel rolling itself in its protecting tail, and goes to sleep. So does the Laplander know the time of night, though the unsetting sun is high above the horizon; and the spiritual world has its Sabbaths of repose.

As our bivalve wears out his tissues so fast in his hard labor, it is necessary that the renewing functions of assimilation and respiration should be carried forward with considerable vigor. This is provided for by the long siphon pipes, already mentioned. The creature gets his air and victuals from behind, while he is at work in front. Unceasingly he finds himself surrounded, even permeated, with a nutritive and cleansing atmosphere. The active muscles at the extremity of the pipes are forever inducing currents in the watery medium, both

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*My friend Mr. S. Stutchbury, formerly curator of the Bristol Institution, England, when dredging in the Australian seas, had the good fortune to obtain the first living specimen of *Trigonia*, a beautiful and remarkable tribe of animals which, after first appearing in the secondary rocks, culminated in the higher oolites and cretaceous rocks, and suddenly disappeared in the tertiaries. He placed his solitary treasure on the middle of the rower's seat, when suddenly the blind and apparently passive little creature opened its valves, put out its leaping-pole, and in an instant, by one spring, had cleared the side of the boat and was safe in its native element. Specimens may be seen in the Smithsonian Museum, obtained by Dr. Stimpson. This is only one among the many indications that in the Australian regions we have the last remains of peculiar types of organic life, which in other portions of the world have given place to more perfect developments.

inhalent and excurrent.   Could we see in those dark regions of the
abyss, and could we make the flow of water evident by colored par-
ticles, we should wonder to see such a commotion going on around
our oyster valve; so many eddies, whirlpools, "lost channels," con-
flicting currents, produced by hidden but evidently powerful causes—
the muscular energy of the soft creatures inside the rocky holes.   It
is said by sanitary reformers that the great questions of public health
resolve themselves into two problems—how to bring pure water in,
and how to carry foul water out.   These problems our Gastrochæna
has most satisfactorily solved.   The pipe muscles are the engine,
pumping the water at high pressure; the siphon is the main which
keeps the reservoir within on full supply; from this the service pipes
of blood vessels branch to every atom of the body.   Dissolved in
the water is the ever-vivifying oxygen, and swimming in every drop
are the dainty infusoria.   The water, cleared of its nourishment by
the net-like lips, and of its renewing functions by the reticulated
films which float like fairy tissues in the living stream, (the plate gills
of our bivalve,) now receives its dose of heavy carbonic acid, as well
as the wasted tissues of the body.   Meanwhile it has been washing
round the house, and has received its particles of almost invisible
dust which the young scraper has cleared out.   The sand of the street,
as well as the drainage of the house, having been thus poured into
the sewer, it is at once flushed out by the same high pressure of the
incurrent water, and is expelled by the muscular force of the second
siphon.   Thus the house is always washed, the drains always cleansed,
the air always fresh, the table ever spread with dainties, and our
tunneler finds his refuse always carried away without the expense of
tram-road or even the labor of wheelbarrow.   Truly, nature's works
are as perfect in their minutiæ as in the guiding of the stars.

As the animal increases in size his instincts (unlike those of many
other borers) lead him to work deeper, and retire farther from the
outer world.   He adopts the sentiment of La Fontaine's hermit rat,
"Les choses d'ici bas ne me regardent plus."   As he advances, he
carefully fills up the empty space behind with layer after layer of
shelly matter, lest the spawn of obnoxious individuals should enter
and occupy the deserted mansion.   Occasionally some unlooked for
event disturbs the even tenor of his existence.   His diffused sensa-
tions, not specialized into the functions of sight and hearing, become
conscious of the slow but steady approach of some intruder on his
peace; it may be his brother, working from the opposite side of the
oyster; it may be a creature of some other race.   It is all one to
him; he displays neither blind partialities, nor special aversions; all
he asks is to be let alone; and he gives his own answer by making a
sudden turn.   His neighbor does the same, and the great event of
their two lives is over.   They never fight.

The same siphonal currents carry forth in their season the fecund-
ating influences which renew the species.   They enter the branchial
cavity of the other sex; whence, also, the matured eggs are carried
forth.

We have taken a peep into the 8-shaped holes on the back of our oyster; let us now see to what differences the round holes are an index. The creatures which leave these as their means of communication with the outer world were called Pholas by Linnæus, and were the first among the borers (if we except the ship-worm, which long ago acquired even a political importance from its ravages in the Holland dykes) to attract the attention of naturalists. They are eaten on the south coast of England, where they are called Piddocks; and are esteemed a dainty morsel on the Californian shores, where they go by the name of date fish. Our oyster borers belong to the section called "Cup-pholas," from the capacious cup-like appendage of delicate skin which rises from the end of the shell in the English species, (Pholadidea papyracea,) like a goblet resting on an alabaster pedestal, and furnishes a receptacle into which the siphon pipes may be withdrawn from in-jury.

Fig. 5.

Gastrochæna, in situ.

a a, spondylus valve, broken across; *b,* bivalve shell in its burrow, valves closed, showing large gape; *g g,* shelly lining of previous excavation; *h,* siphon pipes, with 8-shaped opening; ↓↑, inhalent and expel-lant currents.

To find this delicate fabric, with its translucent, elegantly-sculptured valves, in the middle of the hard silicious rocks of the New Red sandstone, must have been a puzzle both to the acid and the valve-file theories. Our oyster-lover (other devourers of oysters de-light themselves in the "savory fish;" this creature devours the shell) is fashioned in a somewhat coarser mould; is somewhat wedge-shaped, with the thin end of the wedge turned *away from* the seat of boring; and, instead of one delicate capacious goblet, has a series of cup-laminæ, laid one over the other, on each valve, like the shingles or tiles of a roof. Their extremely delicate structure would fare but badly were the tetotum or the awl process the principal source of oyster abrasion. During the adolescent or boring stage, the two valves only touch each other at the point of the hinge, (where the cartilage and teeth, so characteristic of ordinary bivalves, do not exist,) and at a stout projecting knob on the ventral margin. It is evident that then the powerful muscles of the foot, emerging from the large anterior gape in the shell, have "ample room and verge enough" to work from their hinge fulcrum, which is strengthened in this family alone by a long spoon-shaped clavicle, enabling the animal to direct its operations in many different points of attack. Having rubbed and scooped all round in one direction, the valves have power to twist and direct the undermining engine to another portion of the cave. But when the creature has attained maturity, (and there are dwarfs and giants among these, as among men, it being common to find adults not one-fourth the size of more highly pampered favorites of the ocean,) it not only, like the Gastrochæna, and other Pholads, lives retired from the world in its own burrow, but it lives retired
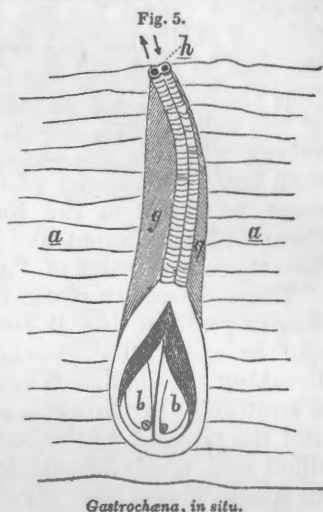
from its own burrow. First it closes in the enormous gape, that capacious portal for the egress of the foot, with an unsculptured layer of shelly matter; then it lays in a plank (so to speak) between the ventral knob and the "cups," and another on the back extending from the hinge; a plug is securely wedged between this and the beaks of the valves; and, finally, the principal part of the outside of the valves, which is not already protected by the cups, is coated over with large "accessory plates," which, even in their small develop-ment, as shown in the European species, caused Linnæus to group them with the limpet-like Chitons and the crab-like Barnacles in the heterogeneous order of "multivalves."*

These accessory plates so tightly wedge the creature into one par-ticular position that it seems impossible for it even to twist round; and in extricating specimens from the matrix it is hard to avoid breaking the shell. When a burrow is broken across longitudinally, a stratified gray lining is seen in the part between the siphon pipes and the tangent to the burrow. The same part in Gastrochæna is filled with shelly layers; but the Pholad appears to swallow, and, so to speak, digest the abraded matter, as he goes along, giving the matter out again in this altered form. In addition to this singular structure, the burrows, especially of aged individuals, frequently dis-play an irregular chamber communicating with the furthest extremity, the sides of which are fashioned in very coarse wrinkles, presenting very much the aspect of some of the water-worn chambers of the Mammoth Cave in extreme miniature. Now in these cavities is fre-quently found the hard, black, horny substance before alluded to, which I take to be the dried foot of the animal, and which is probably strengthened with silicious particles.†

The only explanation I can offer of these curious excavations, which have never before been observed, is, that the foot having lived an active life so long, but being no longer needed for the economy of the animal, does not feel easy in subsiding into a state of complete inactivity, but edges its way through the very narrow anterior chink (or more likely remains permanently outside it) and employs itself in moving backwards and forwards, thus producing the irregular wrin-kles. Sometimes these "foot-prints" make their way to the inner surface of the Spondylus valve; in which case the oyster lays a coat-ing of shell over it. The foot wears away this, and the oyster lays

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* It is very easy, from our advanced point of view, to show the strange errors of the very artificial system of classification which Linnæus devised for the Mollusks; far more untruthful, though not more artificial, than his arrangement of the plants. It was all, however, that we had a right to expect at that time ; and necessarily resulted from the " col-lector spirit" which had prevented the shells from being regarded as a part only of a living animal. His arrangement of these *animals* (apart from the shells) prefigured the classes of Cuvier, which still maintain their place, if not their rank. If we are disposed to find fault, let it not be with the old naturalists, whose works it were, perhaps, better for science some-times to disregard ; but with those who now persist in adhering to the Lamarckian ideas, which are quite as much behind our present knowledge as Linnæus was behind his stand-point. The last six years have, perhaps, contributed more to our knowledge of Malacology than the whole of previous researches put together.

† Any chemist desiring to analyze the gray lining or this black matter will find specimens preserved for that purpose in the Albany Museum.

on another coat, and the process goes on alternately till one of the party dies. The warty excrescences thus produced, often with the black foot at the top, if the oyster has died first, present a strange appearance inside the Spondylus valve, till the true cause of them is discovered. The same process from outside the pearl oysters or ear-shells produces the irregular pearls which resemble a human foot, (said to be a sovereign remedy against gout,) a horse's head, &c.

Time would fail to describe the heart-shaped burrows of Petricola robusta, and the cylindrical cavities of several species of Lithophagus, all of which have their "noses" or breathing end of the shells close to the outside. The Lithophagus or Boring-mussel tribe are remarkable for arranging the abraded matter outside the horny skin enveloping their fragile shells; each species according to a fixed pattern. One of these, before mentioned as being found abundantly on the west coast of Spain and Africa, but not (so far as known) in the Caribbean fauna, makes a long pair of twisted prongs, which look (when we examine the shell alone) as though they were formed for the express purpose of boring; unfortunately, however, like analogous "boring plates" in the ship-worm, they are situated at the *outer*, not the inner portion of the burrow. They seem connected only with the breathing apparatus. One new type of Boring-mussels was found having siphon pipes like Gastrochæna, but smooth inside (Leiosolenus.)

Fig. 6.

Parapholas, in situ.

a, a, Spondylus valve broken across; *b,* the shell in its burrow; *c,* the cup laminæ; *d, d,* accessory valves; *e,* portion of the shell which gapes in the adolescent state, filled up in the adult; *f,* irregular foot cavity; *g, g,* gray lining filling up previous excavations; *h,* siphon pipes; †↓ inhalent and expellant currents.

After these borers have accomplished their work of destruction on the hard fabric of the oyster banks, other bivalves come and live in the empty burrows, which they are often thought to have themselves excavated. They are, however, more truly *nestlers* than borers; and as several individuals often crowd themselves into a narrow cavity, they become very irregular in form, and have been divided up unnecessarily into species. Such are the Saxicavæ, Cumingiæ, Sphænia, &c., as well as the more regular Kelliadæ and Diplodontidæ. Occasionally several different kinds are found, one inside the other, each having gone to live inside the skeleton of his predecessor. Thus nature fills up death with life.

But the boring bivalves are not the only, perhaps not the principal agents of destruction on the oyster valves. The whole colored layer is generally found riddled with a labyrinth of galleries excavated by humble worms, and still more humble perforating sponges. These galleries again, after the death of their makers, form the pasture fields for many tiny species of Gasteropods, the largest of which are

only about a line in length and a few hundredths of an inch across, which crawl about browsing on the minute algæ and nullipores, or quietly inhaling their infusorial food; and also the hunting grounds for equally tiny but predacious tribes which feed upon their vegetarian neighbors. Of all these, and especially the very interesting tribe of Cæcidæ, full particulars will be found in the "Descriptive Catalogue of the Mazatlan Mollusca."

The foregoing is offered as a sketch of an investigation carried on by an ordinary student without scientific name or talents, in order to show that any one of ordinary patience and accuracy may do something to advance our knowledge of the works of God. It is by endeavoring to work out one province carefully, however humble that province be, that the interests of science are best advanced. There is no one but has the materials for some branch of inquiry within his reach; he has only to make choice, educate his eye, observe patiently, and be faithful. Those endowed with the peculiar faculty for eliminating and combining already existing details, may construct the beautiful fabric of general truths; but if this be attempted before we have obtained the facts with sufficient accuracy, the fabric must crumble away. The humblest fact duly placed in connexion with its kindred facts, is of far more value than a grand but hasty generalization. The coral polypes build more islands than the volcanic fires.

The noble science of Geology, ably defined as dealing with unlimited time as Astronomy deals with space, cannot be prosecuted with certainty without the study of recent shells, especially in connexion with their station and geographical distribution. The microscopic species of the Tertiary formations have lately been much studied in Europe, and will, doubtless, soon meet with the attention they deserve in the vast beds of this continent. These will have to be carefully compared with existing forms, both in the Caribbean and West American faunas, and it is not improbable that facts may be eliminated from them not less important than the discovery of the Atlantic Malea ringens before quoted. The study of recent and of fossil shells are but branches of the same inquiry, like the study of the fossils of two consecutive formations. No one department can be safely investigated without a knowledge of the others, any more than one existing geographical fauna can be ascertained without the relations supplied by other faunas. To the ordinary student a general knowledge of the whole subject, as it is presented in time and space, should lay the foundation for the special knowledge of his particular department. By this means his study of the details will be conducted in an enlarged spirit, and presented in that form in which it can best be collated with other ascertained facts.

The study of Natural History is commended not only to those of leisure and abundant resources, but especially to those busily occupied in the works and cares of life.

————————" Nature never did betray
The heart that loved her; 't is her privilege,
Through all the scenes of this our life, to lead
From joy to joy. For she can so inform
The mind that is within us, so impress

With quietness and beauty, and so feed
With lofty thoughts, that neither evil tongues,
Rash judgments, nor the sneers of selfish men,
Nor greetings where no kindness is, nor all
The dreary intercourse of daily life,
Shall e'er prevail against us ; nor destroy
Our cheerful faith that all which we behold
Is full of blessings."—*Wordsworth's lines on Tintern Abbey.*

Not merely is the study of the works of God a constant source of the most delightful relaxation in the regular concerns of life; but in those times, which come to almost all, of deep sorrow, of physical prostration, or of unfitness, from whatever cause, for the discharge of ordinary duties, the words of Coleridge speak the literal truth in the living experience of many:

" With other ministrations thou, O Nature,
Healest thy wandering and distempered child.
Thou pourest on him thy soft influences,
Thy sunny hues, fair forms and breathing sweets,
Thy melody of woods, and winds, and waters,
Till he relent, and can no more endure
To be a jarring and a dissonant thing,
Amid the general dance and minstrelsy,"

LATEST RESEARCHES OF M. MÆDLER

RELATING TO THE GENERAL MOVEMENT OF THE STARS AROUND A CENTRAL POINT.

The number of the *Bibliotheque Universelle,* of Geneva, for September, 1846, contains an abridged translation of a memoir by M. Mædler, from Nos. 566 and 567 of the *Astronomische Nachrichten,* having for its title "The Central Sun." The same astronomer has since published in 1847 and 1848, at Mitau and Leipsic, two volumes in folio, entitled " *Untersuchungen neber die Fixstern Systeme:* or, *Researches on the System of the Fixed Stars;*" of which the first part is devoted to the consideration of *partial systems* of stars, or the reciprocal movements and determination of the orbits of double and multiple stars; while the second part, with which we are at present concerned, has for its subject the *general system of stars,* or the study of facts, tending to prove that there is a common movement of the stars around a central point. Finally, M. Mædler, in the 14th volume in quarto of the collection of observations made at the university of Dorpat, of which he has been director since 1841, and published in 1856 in that city, has returned to the subject, greatly extending at the same time his researches, and showing that the results are confirmatory of his previous deductions. It is here proposed to follow up the former notice of this subject by presenting a summary analysis of the later labors of M. Mædler—labors which in themselves offer a subject of high interest, to which the name of their author, so long and advantageously known to astronomers, gives great weight, and which yet have not commanded, perhaps, so much attention as they merited.

It is natural to presume that the stars, which, in reference to the planets that revolve around our sun, we are generally accustomed to designate as fixed, are not in reality and altogether such, but that they, too, have movements subjected to certain laws. That in view of the immense distance of those stars, their movements must appear to us very small is readily comprehended, as well as that, to be properly verified, they must require precise observations made at long intervals of time.

In comparing observations of the same stars made at different epochs, and taking into account causes of apparent variation, already recognized, such as the precession of the equinoxes, the mutation of the earth's axis, and the aberration due to the velocity of light, astronomers had still been long aware of small differences of reciprocal position which might be attributed to a movement proper to the stars themselves. But we can scarcely go back higher than to the observations made by Bradley, about 1755, to obtain a point of departure, in

positions sufficiently exact, for what relates to these proper movements.

Sir William Herschel, in studying this subject with the sagacity which distinguished him, arrived successively at two discoveries of very high importance.

In the first place, about the year 1783, he showed that when these proper movements of the stars are considered collectively, a reason may be assigned for a great part of them by admitting that our sun has itself a movement in space in a certain direction. This movement, by a simple effect of perspective, would produce a gradual apparent, separation among the stars on the side to which the sun was directing its course, and an apparent diminution of relative distance among those from which it was receding. Though long contested, even by astronomers of great merit, the movement in question seems now placed beyond dispute, since the labors of Argelander, Lundahl, Otto Struve, Bravais, Galloway, and Mædler, directed to this subject, have given it full confirmation, and indicated its direction towards a point in the constellation Hercules, very near to that assigned by Herschel himself.*

The second great discovery of Herschel in reference to this subject dates from the first years of the present century, and relates to those stars which appear to the naked sight so near one another as to be blended into one, which, from this circumstance, we denominate *double*, *triple*, and *multiple* stars. It was shown that in examining these closely there might be detected, at the end of a certain number of years, evident changes in the relative positions of the stars composing some of these groups, from which we may conclude that such stars form systems of suns revolving around their common centre of gravity. It is well known what an extension has been given to this interesting part of science by the subsequent researches of astronomers, in which Sir John Herschel, Sir James South, Messrs. Dawes, Hind, Jacob, &c., of the British empire; the elder and younger Struve, Savary, Encke, Bessel, Kaiser, Mædler, &c., on the continent of Europe, have particularly distinguished themselves by their labors of observation and calculation.

From divers considerations set forth in the memoir above cited, and from comparing together the proper movements of some hundred stars, M. Mædler had, in 1846, already attained a firm persuasion that, besides the causes of apparent displacement just spoken of, the collective body of stars visible to us has a real and common movement of revolution around a centre, situated in the group of the Pleiades, and corresponding to the star Alcyone, or η of Taurus, of the third magnitude, the most brilliant of the numerous stars of that group.

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* We feel satisfaction in recording that among the first to confirm the results obtained by Herschel respecting the proper movement of the sun were two French philosophers, Pierre Prevost and Frederic Maurice, in a memoir inserted in the collection of the Academy of Berlin for 1801. M. Arago has shown that the idea of the possibility of such a movement had been already announced by Fontenelle, Bradley, Mayer, and Lambert, though he fully recognizes Herschel as the first who *proved* its existence.

In the first part of the great work on this subject, which was pub-
lished in 1847 and 1848, he subjected to a detailed examination all
the observations made by himself and others relative to double stars
and the orbits they describe, with a view to ascertain if there were
among these or other stars any central body competent by its mass
to exert a preponderant attractive action on all the other stars, so as
to be controller of the general system, or central sun, in the literal
sense of that word, as is our sun for the partial system which it governs.
The result was to negative any such hypothesis—a result which no
one has contested.

In the second portion of the work, the author has shown that the
idea of a distribution of the stars into simply partial systems, without
any general connexion among themselves, is not admissible, inasmuch
as it fails sufficiently to account for the proper movements already
ascertained. This idea once eliminated, there remained for M.
Mædler, while admiting the law of universal gravitation, no other
assumption but the existence of a general system without a pre-
dominant central body, or of a globular system in which the stars
all revolve about their common centre of gravity, according to
a force directly proportional to their distance from the central point.
With this assumption as a basis, the investigation turns upon the
question whether the observed movements proper to the stars satisfy
the conditions which result from it.

The conditions which the central point and the neighboring region
of the heavens ought to satisfy are the following:

1. The central point should have no *real* movement proper, and its
apparent movement, though to be taken in the opposite direction,
would represent that of the sun.

2. If around the central point there be any group of stars physi-
cally connected with it, the proper real movements of the stars of
that group should be very small and equal as regards one another.

3. If we describe around the central point C a concentric sphere,
whose radius, C S, is equal to the distance of our sun from that point,
all the stars situated within the interior of such sphere should have
a real movement proper, smaller than that of the sun, and so much the
smaller as they are nearer to C; the real movements proper should
increase in departing from C to the circumference of a great circle
described with C as a pole.

4. The point of the heavens towards which the sun is directing
itself should be some 90 degrees from the centre C.

5. If we denote by φ the angle of direction of the proper move-
ment of any observed star, and by ψ the angle of direction of the
proper movement of the sun, the difference $\varphi - \psi$ of those two angles
should be *null* at the centre, and should increase in all directions at
departing from that point, without ever exceeding 90 degrees within
the interior zone.

6. The region of the heavens when the central point is determined
should be the only one in regard to which the whole of the above con-
ditions is fulfilled.

M. Mædler afterwards gives in detail, by means of long tables, the positions and proper movements of 861 principal stars observed by Bradley, calculated for the epoch of 1840, and subdivided into different sections according to the distances of those stars from the central point or from Alcyone.* The following table presents a summary of the results:

Place of the stars.	Number of stars.	Proper movements—annual means expressed in fractions of a second of degree.	Mean value of the angle $\phi - \psi$ in degrees.
		"	°
Central point	Alcyone.	0.0673	1.6
Pleiades	11	0.0699	13.3
Zone of 1° to 5° of distance from Alcyone	12	0.0702	29.9
Zone of 5° to 10°	31	0.0699	36.1
Zone of 10° to 20°	101	0.0890	44.3
Zone of 20° to 30°	159	0.1067	48.6
Zone of 30° to 40°	224	0.1096	46.1
Zone of 82°.5 to 97°.55	302	0.1183	65.2

These values, it will be seen, satisfy in general the principal conditions announced above; but as the author has since much extended the field of his researches we shall pass at once to the exposition of his last labors, in what relates to their principal object.

M. Mædler has devoted the greater part of the 14th volume of the observations at Dorpat to a new catalogue of 3,222 stars, of from the 1st to the 7th magnitude, observed by Bradley, of which the positions in right ascension and in declination are calculated by himself for the beginning of 1850, as well after the old as the new observations. These values are accompanied by the precession and secular movement proper of each star; the latter expressed both in right ascension and declination, and in polar co-ordinates. This catalogue is subdivided into four sections ranged each in the order of right ascensions. The first is composed of stars situated to the south of the equator as far as to 30° of south declination; the second of those north of the equator up to 30° of north declination; the third of stars situated between 30° and 60° of north declination; and the fourth of those comprised between the north pole and the 60th degree of north declination. The author has also calculated, after the observations of Lacaille and of Johnson, the proper movements of 97 stars of first to fourth magnitude, whose southern declination is more than 30°.

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* M. Mædler, in his calculations of the positions of stars, has made use, among others, of the determinations of right ascensions resulting from observations made at Geneva under the direction of Professor Plantamour, and he judges them altogether comparable in point of precision to those obtained at the principal observatories of Europe.

The sum of M. Mædler's calculations gives for

	"
80 stars, 1st to 2d magnitude, a mean secular movement proper of	25. 09
200 stars, 3d magnitude, a mean secular movement proper of	17. 10
348 stars, 4th magnitude, a mean secular movement proper of	14. 18
690 stars, 5th magnitude, a mean secular movement proper of	11. 09
994 stars, 6th magnitude, a mean secular movement proper of	9. 05
921 stars, 7th magnitude, a mean secular movement proper of	8. 65

Although, according to this table, the most brilliant stars are those which in the mean have the greatest proper movement, yet the author, in comparing the strongest of those movements in each order of magnitude or apparent brilliancy, and adopting the opinion that the stars of considerable proper movement ought to be in general nearer the sun than the others, concludes that among the stars nearest us those of the least brilliancy appear, absolutely speaking, more numerous than those which are more brilliant. In effect, if alpha of the Centaur, Arcturus, Procyon and Sirius have, respectively, a proper annual movement of $3''.67$, $2''.26$, $1''.33$, and $1''.25$: on the other hand, Rigel, (the 4th star in order of brilliancy according to Sir John Herschel,) α of Cygnus and β of Perseus have scarcely any proper movement at all, while μ of Cassiopeia, the 40th of Eridanus, the 61st of Cygnus, and the two stars named after Argelander, which are of 5th to 7th magnitude only, have proper annual movements of from 4 to 7 seconds. It should be also remarked that of 52 stars of the 2d magnitude observed by Bradley the mean value of their annual proper movements, as reported by Mædler, page 192 of volume 11 of his Researches, is but $0''.138$; while of 150 stars of the 3d magnitude it is $0''.173$. Thus, contrary to the ideas adopted by W. Struve, in his Etudes Stellaires, the degree of brightness appears to M. Mædler to be a bad indicator of the relative distance of the stars.

We have already seen that, admitting that all the stars move around a common centre, without any preponderant mass, and according to the Newtonian law of attraction, the velocities will be very nearly proportional to the distances from that centre; the more uniform the distribution of masses the less will the form of the described orbits vary from the circle. From the central point C, which is in repose, the proper movements of the near as of the remote stars would be equal; but from another point, S, situated at a certain distance from C, the stars nearest to S will appear to move more quickly. M. Mædler is disposed to admit that our sun is situated at about half the interval between the central point and the exterior limits of the space comprised by the stars with which he is engaged; but those stars form not a millionth part of the whole of the visible fixed stars, without even taking account of the Milky Way.

The author commences his new researches with the determination of the movement of the sun and its direction. In this view he subdivides the stars whose proper movements he has determined into three classes, viz:

1. Those, to the number of 227, whose proper secular movement is greatest, and amounts to a mean of $55''.4$.

2. The stars, to the number of 663, for which that mean is 15″.25.

3. Those, to the number of 1,273, whose mean secular movement is but 7″.79.

He has left out of view in this inquiry, as well as in that which relates to the angles of direction $\varphi-\psi$, those stars whose proper movement is less than 4″ a century on account of the great uncertainty which results therefrom respecting their direction.

M. Mædler makes use of the formulas given by Argelander in his memoir of the proper movement of our solar system, in applying thereto the method of least squares and successive approximations. He arrives finally at the following values for the right ascension A and the north declination D of the point of the heavens towards which the Sun was directing its course in 1800 :

By the 1st class of stars · · · · · · · · · · · · · · A=262° 8′.8; D=39° 25′.2
By the 2d · · do · · · · · do · · · · · · · · · · · · · · A=261° 14′.4; D=37° 53′.6
By the 3d · · do · · · · · do · · · · · · · · · · · · · · A=261° 32′.2; D=42° 21′.9

These results, it will be seen, differ but little from one another, or from those previously obtained by other astronomers.

As to the general movement of the stars, the author presents, in the first place, new and detailed tables of the proper movements of the groups Pleiades and Hyades, of which he employs only the mean values in his ulterior calculations; not including, however, in that relative to the Hyades the two stars Aldebaran and ς of Taurus, whose movements are not in correspondence with those of the other stars of the group.

He obtains thus, respectively, for the secular movement proper and the angle $\varphi-\psi$:

Of Alcyone · 4″.70; $+$ 2°.8
Of the mean of 15 stars of the Pleiades · · · · · · · · 5″.82; $+$ 8°.7
Of the mean of 27 stars of the Hyades · · · · · · · · 11″.26; $+$ —60°.65

He afterwards subdivides the heavens, around Alcyone as pole, by means of concentric circles, described at from 10 to 10 degrees, into eighteen zones or regions, as far as the pole diametrically opposite. In the following table we give, for the stars comprised in each of these regions, the mean values of the observed secular movements proper and of their direction, resulting from his calculations. The number of stars of which he has made use, indicated in this table, refers only to the proper movements, those numbers being smaller for the angles $\varphi-\psi$, in view of the limit of 4″, alluded to above.

15

Number of the region.	Number of stars.	Mean of secular movements proper.	Mean of angles $\varphi—\psi$.
		$''$	$\circ$
1	45	7.71	39.98
2	100	8.20	46.43
3	189	9.78	55.45
4	264	9.79	56.86
5	269	10.41	61.72
6	275	11.97	62.59
7	273	10.03	61.19
8	246	10.95	67.95
9	277	10.89	62.75
10	218	9.71	68.80
11	221	9.56	58.01
12	163	11.71	67.97
13	163	12.51	63.26
14	123	12.07	61.90
15	92	10.01	58.92
16	87	13.33	61.21
17	58	9.16	54.41
18	44	7.30	47.27

We see by this table that for the first six regions the proper move-
ments and the angles of direction go on regularly increasing. In the
first there is but one case in 32 where $\varphi—\psi$ exceeds 90 degrees,
while there are 55 in 186, in the sixth comprised between 50° and
60° of distance from Alcyone. The twelve following regions, which
are less complete and more and more removed from the central point,
do not offer the same regular progression; and there is even a decrease
in the value of the two elements in the last, situated towards the
point diametrically opposed to the central point.

M. Mædler admits that his last researches have not confirmed the
opinion announced in the former, that the proper movements ought
to increase gradually up to a distance of 90° from the central point,
and perhaps even a little beyond. But he observes, 1st, that with
regard to the southern zones it is impossible yet to conclude anything
positively, so long as we have not in those regions a greater number
of proper movements exactly determined; 2d, that as to the regions
Nos. 10 to 12, they are situated in the neighborhood of the point Q,
towards which the sun is moving, a circumstance which would dimin-
ish the apparent proper movement of a part of the stars comprised in
those regions; that, besides, the number of stars observed by Bradley,
and even by modern astronomers, is smaller in those regions some-
what distant from the ecliptic than it is near the Pleiades. The
author remarks that taking the regions from 7 to 12 by pairs, we
shall still find a gradual increase in the mean proper movements. In
regard to the last regions, if the decrease in the elements with which
we are concerned should be confirmed, that, according to M. Mædler,
may be because all the stars have not a movement in the same direc-
tion with our Sun, and like the comets, their movement may be some-
times towards one, sometimes towards another point. He regards

the first as the most probable cause, but believes, in the meantime, that the conformity of movement is not so great for the stars as for the planets of our solar system.

According to the last researches of this astronomer, the central point C and that towards which the Sun is moving are distant from one another by an arc of 111° 30′.7; and the proper movement of Alcyone, taken in an inverse direction, conducts to a point situated 2°.6 south of Q. According to the preceding calculations, these numbers were, respectively, 113° 36′ and 1°.5. If the position of Q is well determined, it may be that the orbit described by our Sun is not a circle, but an ellipse analogous to that of the planet Polymnia.

The ideas promulged by M. Mædler from the time of his first researches having encountered divers opponents, it was incumbent upon him to reply in the later to those whose arguments merited a serious attention.

Sir John Herschel, in his *Outlines of Astronomy*, has objected to the central point adopted by our author, that its position was unlikely, because the group of the Pleiades does not project itself upon the Milky Way. To this objection M. Mædler replies, that it is evident that the point which is the common centre of gravity of the Milky Way, and the whole mass of stars which environ it, ought to be found on the central plane of the ring constituting that celestial belt, and be projected on the plane from some point within its circuit. But if our Sun be in that plane, it is apparent that the middle of the belt must correspond to a great circle of the celestial sphere. Now, the admirable charts published by Sir John Herschel himself show that such is not the case in reality. The Milky Way is not at the same distance from the two poles of the equator, and it does not divide into two equal parts either that circle or the ecliptic. According to the researches of G. Fuss, the small circle to which the Milky Way best corresponds, is distant $3\frac{1}{2}$ degrees from the great circle to which it is parallel. It follows from this that an interior point situated in the plane of the ring, without being in the ring itself, cannot, from the sun or the earth, be projected on the Milky Way, and would be removed from it by an angle equal to that which a straight line drawn from the sun to the central point would make with the plane of the Milky circle. M. Argelander having previously, on the occasion of his investigations of the proper movement of the Sun, thrown out the opinion that the central point of the movement of the stars was perhaps in the constellation Perseus, M. Mædler had, in his first memoir, objected to this idea, the situation of that constellation on the Milky Way. M. Argelander since then seems not to have pursued his researches on this subject; and no one, to my knowledge at least, has indicated any other point of the heavens for adoption as the central point in preference to that situated in the Pleiades.

Professor C. A. F. Peters, director of the observatory of Altona, and present editor of the *Astronomische Nachrichten*, had also, from the publication of M. Mædler's first researches, raised some objections as to their results. The latter has replied to them, (pages 254, 257, 14th vol. of the *Observations of Dorpat*.) It would appear that this reply, with

the new developments that our author has given to his researches,
have led Professor Peters to admit the validity of the conclusions
deduced from them; for the second and forthcoming number of the
new publication of Professor Peters, *Zeitschrift für populäre Mitthei-
lungen*, designed as a continuation of the *Annuaries* published by Schu-
macher from 1836 to 1844, is to contain, it seems, an article by M.
Mædler on the *Central Sun*.

Having thus cursorily exhibited the results obtained by M. Mædler
in the principal object of his labors, it is proper now to pass in
review certain consequences which he has deduced from his researches
at the end of his *Untersuchungen*. It should be remarked, however,
that he presents them only as first views, still quite uncertain, which
furnish at the most but very rough approximations towards the values
of the elements to which they relate.

As the mean proper movement of Alcyone ought, from what has
been said, to correspond to the angular movement of the Sun taken
in a contrary direction, if first we adopt, agreeably to the earlier re-
searches of the author, $0''.0673$ as the annual value of that movement,
this quantity being the 19,256,000th part of the circle, it would
thence result that the complete revolution of the sun and all the fixed
stars around the central point would be accomplished in about 19
millions of years. According to the later valuation, $0''.047$ of this
annual proper movement, the period of revolution would be still
longer, or about $27\frac{1}{2}$ millions of years for such of the stars as are not
in the vicinity of the central body, nor at the extreme exterior
limits of the circuits of the Milky Way. It is evident, more-
over, that the value of the proper movement of the central point
is still quite uncertain, and we may observe in the table of the proper
movements of each of the Pleiades given by M. Mædler, (p. 259 of the
14th vol. of *Observations of Dorpat*,) four stars of that group whose
secular proper movement is smaller by some tenths of a second than
that of Alcyone. The smallest, which is $3''.9$ only, would correspond
to the star designated by the letter l; but that star is only of the sixth
magnitude, and its angle of direction is $39°$, so that it can by no
means be regarded as constituting the central star.

The author admits that the subordinate systems should have shorter
revolutions. He shows that the non-existence of any star having an
annual parallax of several seconds of a degree proves that our Sun has
no other star associated with it, and belongs to no partial multiple
system. He has concluded from his researches on the double stars,
that there can scarcely be any system of this sort where the companion
is distant from the principal star more than six minutes of a degree.
The case of a considerable number of stars being close to one another,
is hence the only one where there is any likelihood of a partial
physical system. Thus, for the largest number of stars there exists
only a common bond, and with the exception of perturbations com-
paratively insignificant, resulting from certain conjunctions of stars
among themselves, they exert no particular action on one another,
nor on the system of planets.

We have already seen that M. Mædler is not disposed to admit, in
general, that the distances of the stars are inversely proportional to

their brightness, and he thinks that the difference in their light may often be referred either to a real difference in their diameters, or possibly in their specific luminous intensity. He cites as a striking example of this kind, the two stars of the constellation Cygnus, Alpha and 61; the first having a parallax and proper movement almost imperceptible, while the value of these elements in each is respectively 0″.348 and 5″.22; whence he concludes that the latter must be at least 30 times nearer to us than the former, and its absolute brilliancy 20,000 times less.

It is to the attentive observation of the proper movements, combined with the parallaxes directly obtained, that, according to our author, we should attach ourselves for the determination of the distances and special physical relations which exist between the systems of stars. As an essay towards absolute determinations in this way, he compares among themselves the proper movements and parallaxes for the stars to the number of 7, where this last element is approximately known, after the researches of Bessel, Maclear, and Peters. These respective values are as follows:

	Proper movement ε.	Annual parallax π.
a of the Centaur	3″.674	0°.912
61st of Cygnus	5 .221	0 .348
a of the Lyre	0 .349	0 .103
1830 of the catalogue of Groombridge	7 .020	0 .226
Polar star	0 .038	0 .067
2 of the Greater Bear	0 .535	0 .133
Arcturus	2 .258	0 .127

With the exception of the Polar star, in regard to which the smallness of the two elements renders their values very uncertain, we see that the first much surpasses the second in magnitude, their mean relation being that of 10 to 1. Macleare thinks the parallax of Sirius less than the fourth of a second, while its proper movement is one second and a fourth.

M. Mædler, after having endeavored to determine anew, with all possible care, the precise value of the annual proper movement of 61 of Cygnus, finds it to be 4″.282. Now, that of the Pleiades being, in the mean, 0″.0582, is consequently 73 times smaller. We may then admit that in the rectilinear triangle PSC, which joins the Pleiades, the Sun, and 61 of Cygnus, the side SC is much the smallest. The angle at the Sun S being 83°.4, the angle at C should be near 90°, without by possibility exceeding 96°. The author admits, then, the equality of the sides PS and PC to be very close, and consequently that also of the real proper movements of the Sun and 61 Cygnus. Thus the annual proper movement of our Sun ought to be seen very nearly under an angle of 4″.284 from a point in the celestial space where the radius of the earth's orbit would appear under a parallactic angle of 0″.3483; whence it follows that the proper movement of the Sun corresponds very nearly to 12,295 radii of the terrestrial orbit.

This result is much greater than that of 1,623 obtained by W. Struve, at the end of his *Etudes Stellaires*, the grounds of which are

contested by Mædler. He observes, however, that the recent re-
searches of Johnson and Otto Struve on the parallax of 61 Cygnus,
which raise its value to about one-half second, reduce the annual
proper movement of the Sun, if this last result be admitted, to about
8½ radii of the earth's orbit, or 276 millions of leagues of 25 to the
degree; and he remarks that this velocity is very nearly that of the
planet Mercury in the orbit it describes round the Sun.

Let us again assume for the moment, with M. Mædler, the value
of the Sun's annual movement to be 12,295, as obtained above;
this movement, as it would be seen from the Pleiades, would be
expressed, according to the estimate before given of the arc CQ, by
the formula, 12,295 × sin 111°.5; which reduces the movement to
11.44.

We have, then, for the parallax π of the group of the Pleiades,
assuming the estimate on a former page of its mean movement and
angle $\varphi - \psi$:

$$\pi = \frac{0°.0582}{11.14} = 0°.00509.$$

The distance to the Sun corresponding to this parallax is about 40½
million times the radius of the earth's orbit, a distance which it would
require about 640 years for the light to traverse.   The two last num-
bers would be still greater if we assume the values relative to
Alcyone.

The author shows that from the proper movement of our Sun, and
its distance from the central point, may be deduced, by means of
Kepler's third law, the proportion of the whole attractive mass which
revolves around Alcyone as a centre, to the mass of our Sun.
According to the preceding numbers, that whole mass would be about
one hundred and eleven million times that of the Sun.

M. Mædler considers our solar system to be situated in a region of
the heavens comparatively very destitute of stars, but the regions of
mean stellar abundance to rate much lower in point of mass than the
space occupied by our planetary worlds.   In this latter system, the
central mass exerts over the subordinate bodies, which are in limited
number, so considerable a force that the perturbations are there for
the most part of little importance.   In the general stellar system an
analogous result is obtained in a different manner, namely, by the
immense number of the members of that system, obviating in this
respect the necessity for any preponderant central mass.

The regions of the heavens near the group of the Pleiades, north
and south, are comparatively quite destitute of stars, especially from
β of Perseus to λ of Taurus.   Farther away from the constellation,
there is again, in every direction, but chiefly towards the east, a great
abundance.   Still farther on, we presently meet towards the west
another deficiency, along the zone which traverses Pisces and Pe-
gasus; while more to the east there occurs first the remarkable group
of the Hyades, and then a great diminution in the number of stars.
In other regions of the heavens we may observe that the richest in
stars have not the form of rounded groups, but rather that of
elongated zones, almost parallel to the Milky Way, and that between
these there are seen others destitute of stars.   The southern heavens

present this appearance in a still more striking manner than the northern. The Milky Way is itself composed of several concentric rings, situated one behind the other, forming circular zones rich in stars, comprised between others which are less so.

At the centre of this great system exists a rich group, forming in the whole a considerable and well-defined mass, whose diameter, nearly equal to the distance from our Sun to the 61st of Cygnus, is about 600,000 radii of the earth's orbit. It has already been said that the zone in which our Sun is now found is poor in stars; and it is to this situation that we may chiefly attribute the fact that the mean distances of the stars so little correspond for us with their apparent magnitude. It may be, also, that what we term richness in stars is referable to a greater luminous power, increasing or decreasing by alternating zones.

The double stars, and the most considerable groups of stars, occur in regions poor in stars as well as in those which are rich. Thus the two stars considered the nearest to us are double, and situated in the same zone with the Sun, which is placed not far from the middle of the interval comprised between them.

This constitution by alternative zones is not, as M. Mædler remarks, so different from that of our planetary system as might appear at the first glance, for around our Sun there is first a void space of 0.38 of a radius of the earth's orbit; then we find a zone occupied by four very dense planets of mean size, to which succeeds that of the asteroids of very slight mass, and then that of the great planets, attended by numerous satellites.

The extent of the rings of the Milky Way may be determined up to a certain point. In effect, the shortest distance from Alcyone to the middle of that belt is 21°, and the mean divergence of the Milky Way from a great circle, as has been said above, is three degrees and a half. If we draw right lines between the Sun S, the group of the Pleiades P, and the points M and M', the nearest and the most distant from the Milky Way, we shall have two triangles having a common side PS, supposed to be known from what precedes. M. Mædler deduces therefrom the following values:

The half-diameter of the Milky Way corresponds to a distance which the light would require 3,648 years to traverse. The distance from the Sun to the nearest point of that belt would be traversed by the light in 3,166 years, and its distance to the most remote in 4,140 years.

But as the belt is double, and for the points in question the two portions, by an effect of perspective, appear blended, the interior ring ought to be a little less distant, and the exterior one, on the contrary, considerably more so. It had already been supposed that the more remote regions of the Milky Way would correspond to a distance which light would occupy nearly 4,000 years in passing over.

From this it results that the orbit described by our Sun in space is very nearly to that of the circumference of the Milky Way in the proportion of the orbit of Jupiter to that of Neptune; and, pursuing the analogy, it may be remarked that the portion of stars comparatively the smallest is on the interior, and the largest on the exterior. When the stars which surround the Pleiades to a distance of 25 to

30 degrees, those situated at the point of the heavens diametrically opposite, and those of the zone of the great circle of which these two points are the poles, shall have been completely observed, (as has begun to be executed at Dorpat for a part of them,) we shall be enabled step by step to attain a more profound insight into the organism of our stellar system.

"I regard," adds M. Mædler in terminating his great work, "the complete assemblage of stars which revolve around the group of the Pléiades, their common centre of gravity, as forming a sort of *isle* in the universe; and I admit that there are in the vicinity of and beyond this stellar system, other analogous isles, of which the nebulæ offer us various examples. We cannot as yet decide, if some among them stand in a relation of neighborhood and connexion with our own; but it may be possible that there exists between them and our Milky Way a common bond, the last being always to be considered as of a more elevated order. Yet it does not appear to me probable that the particular configuration of our isle in the universe should be a model for the others, as well because this conformity would but little accord with the variety which prevails in the subordinate systems, as that the very different forms under which the nebulæ present themselves to our view could scarcely be explained by merely optical differences. Still it is true that some of these isles have an appearance which offers a striking similitude to that of our own; such being particularly the case with the beautiful annular nebula of the Lyre, whose interior, according to recent explorations, is not entirely void and obscure, and which, taken as a whole, sufficiently represents our stellar system as it would be seen at the distance of the nebulæ.

"The preceding considerations have led us to the contemplation of an extent of space and time, which, relatively speaking, we may well term infinite, and the number of bodies in the universe is far beyond our powers of computation. When, from our terrestrial dwelling-place, we strive to penetrate deeper and deeper into space, every scale of measurement, however colossal it may at first appear to us, is annihilated, so to speak, before the immensity of the heavens. It is not so much the infinity of numbers which renders this grand organism so worthy of admiration, for this manifests our own littleness still more than the greatness of the universe; it is the inexhaustible multitude of forms and figures which sets forth before our eyes in the most striking manner the infinite power and wisdom of the Creator. Nature works not after models; she knows how to combine with the strictest subordination to a single general law, the most pliant liberty of action and the richest variety of development. Hence, the meditative spirit may range through this infinite without fear of ever losing the guiding thread. Every new member, each successive gradation in the universe, is not a repetition on a wider scale [of what was already known; it presents to us formations which, whether from without or within, extend beyond all prevision our previous conceptions.

"The greatest explorers of the skies in the two last centuries, attached themselves strongly to the idea that our planetary system was a model, in itself, of the system or systems of the fixed stars; they

sought for a single Sun, which should be to the universe what our Sun is to the planets, and not having found it, they were tempted to renounce the idea of a general organization of the stars, and no longer to recognize anything but partial systems.

"If, while leaving unimpaired the validity and comprehensiveness of Newton's great law of universal gravitation, I have succeeded in proving that the organization of our stellar system has a substantial existence, wholly different from that of the systems which are subordinate to it, and in determining the most probable central point of the grand whole, my aim will have been attained, and the principal task which I had proposed to fulfil during life, will have been accomplished."

We cannot terminate this notice without rendering a just homage to the perseverance with which M. Mædler has conducted his researches, and to the important progress which, in any event, will have been achieved in the determination of the proper movement of the stars by virtue of his labors. It is now a long time that philosophers and scientists have been occupying themselves with the constitution of the universe. The first part of the interesting memoir published in French, in 1847, by the celebrated astronomer, W. Struve, director of the Russian observatory of Pulkova, under the title of . *Etudes d'Astronomie Stellaire*, comprises among others a summary and very curious exposition of the ideas, always ingenious and in part conformable to truth, which have been successively promulgated on this subject by Kepler, Huygens, Wright, Kant, Lambert, Mitchell, and Sir W. Herschel. But before this last, these ideas were most frequently rather speculative than founded on investigations and positive observations. It is Herschel, then, who has opened the most direct and surest route, by the help of his powerful telescopes; and M. Mædler is beyond doubt one of the astronomers who has followed him with most ardor and success, while profiting by the labors of his predecessors, and above all by the precise determinations of the positions of stars recently obtained.

There remain, doubtless, many points still uncertain in the results obtained by the latter, and time only can confirm in a definite manner the solution which he has given of this important problem. But as the author has in general sustained himself by observations as exact and numerous as circumstances permitted, giving them in all their details, without making an arbitrary choice among them, and without dissembling the weak points of his system, but with the sole desire of arriving at the truth, and with the conviction of having attained it, it would seem that we ought to be disposed to admit the validity of his principal deductions, corroborated as they now are. The case of the determination of the general movement of the stars by M. Mædler will, perhaps, prove analogous to that of the first determination of the movement of the Sun in space by Sir W. Herschel; that is to say, that having been a long time contested or neglected, it will be finally confirmed and generally admitted, and will constitute a fair title of honor for the skilful and bold astronomer who will have been the first to prove its reality.

REPORT ON THE TRANSACTIONS OF THE SOCIETY OF PHYSICS AND NATURAL HISTORY OF GENEVA, FROM JULY, 1858, TO JUNE, 1859.

BY PROEESSOR DE LA BIVE, PRESIDENT.

[Translated for the Smithsonian Institution from the *Memoires*, &c., *Tome* xv., *Premiere Partie*, 1859, by C. A. ALEXANDER.]

GENTLEMEN: An existing regulation of the society devolves on its president the duty of presenting to you, at the moment when he is about to lay aside his functions, a detailed report on the labors of the body during the year just elapsed. This order, which for the year previous was fulfilled by our former president, Professor Gautier, I now propose on my own part to execute.

The Society of Physics and of Natural History embraces in its field of labor, as its title indicates, alike the physical sciences and the natural sciences, that is to say, every part of the domain of human knowledge which has for a basis observation and experiment in the field of nature. Pure mathematics, therefore, do not fall within its compass, though applied mathematics are by no means excluded; for how could such be the case where astronomy, mechanics, and physics occupy so prominent a place? But as there is no one versed in pure mathematics who does not more or less make an application of them, it follows that association with us lies always open to the learned mathematician, and thus we are warranted in saying that no scientific notability of our country is excluded on system from our circle.

The division between the physical sciences and the natural sciences which the name of the Society recalls is not purely arbitrary. It is founded on a true principle, namely, that in the study of nature there are two points of view strictly different: the one having for its object more particularly the study of forces and laws, the other attaching itself essentially to the examination of bodies themselves. Not that in the former kind of study bodies do not play an important part, since it is only through their medium that we take cognizance of forces, nor in the latter that forces should not be taken into consideration, since without them we could not know the properties of bodies. But the dominant and characteristic division is in strictness that which I have indicated.

The distinction, however, is not always very definite, and if we place physiology in the division of natural sciences and not in that of the physical sciences, it is solely because physiology is inseparable from organic natural history, which furnishes its elements, and to which at the same time itself serves as a basis.

Thus, then, in the report which we are about to present, we comprehend under the same head of *Physical Sciences*, mechanics, astronomy, physics, both mathematical and experimental, terrestrial and meteorological, as well as chemistry—sciences whose points of contact are so numerous and so multiplex that it is difficult to determine the limits which separate them.   Geology, mineralogy, and organic natural history, botany, and zoology, (comprising therein physiology) form the second group, which, under the head of *Natural Sciences*, constitute likewise an assemblage sufficiently compact, to which palæontology accedes as a cement binding all the parts together. We confess that we are at a loss to know to which of these groups statistics should be referred, which, like mathematics, find a place among us only by virtue of their application, and whose labors, therefore, ought to be classed, it would seem, according to the nature of the application which is made of them.

PHYSICAL SCIENCES.

It is natural to place at the head of the physical sciences that which, lifting the regards of man towards the celestial vault, seems more suited than any other to recall to him the magnificence of the creation and the greatness of its Author.   Some years ago, and astronomy might have been thought to have uttered its last word. Certain stars to be discovered in the immensity of the heavens, improved methods to be invented for calculating the courses of the celestial bodies—these seemed to be the only lines in which progress was possible after the labors of the Herschels and La Places; but thanks to the improvement of instruments and the perseverance of observers, a new era has dawned for this part of the sciences.   New planets, announced, like Neptune, through the potency of the genius of mathematics, or discovered simply by a conscientious exploration of the skies, are constantly ranging themselves in our astronomical catalogues; a more profound study of the physical properties of the stellar bodies leads to inductions of the highest interest with regard to their physical constitution; and the aid of powerful glasses unveils to us in the fixed stars the nebulæ and the comets—appearances till now unknown.

I have mentioned comets.   Of course there was much question in the society respecting that of Donati, which was the great astronomical event of the year 1858.   Professor Thury was the first who occupied our attention with some observations he had made on that body, the tail of which he had found to be double near the nucleus. Still later, Professor Plantamour communicated a summary of the observations made upon it at the observatory of Geneva, from August 28, to October 18.   In that interval of time, the number of days of observation was 29, for which the position of the comet was determined by comparison with the neighboring stars.   From these observations, and those made by different astronomers, it results that the part of the orbit traversed by the comet before passing to its perihelion is an ellipsis which would require from 2,100 to 2,400 years as

the duration of a revolution, and the determination of which is de-
ducible to quite a close approximation from this single appearance
by a comparison of all the observed places after and before the pas-
sage to the perihelion.   M. Plantamour has entered into many details
on the physical appearance of the comet, which he has reproduced in
a series of drawings.   He has described the presence of an obscure
space situated immediately behind the nucleus in the part opposed
to the sun; this obscure space, which often appeared darker than the
ground of the sky, varied sensibly in its form and extent during the
course of the observations; and he remarks that it is equally to be
observed in the delineations of Halley's comet published by Bessel.
Lastly, he has added some remarks on the size, both apparent and real,
on the form and direction of the tail, the apparent length of which
was 41° on the 5th October, and the linear length 13.5 millions of
leagues, (25 to the degree,) while on the 13th October the apparent
length was not more than 32°, and the linear length 10.5 millions of
leagues.   M. Thury and M. Wartman have also described some pecu-
liarities relative to the light emitted by this body, and M. Wartman,
the younger, as likewise M. De la Rive, have pointed out the analogy
which the bifurcation of the tail of the comet into two parts, separated
by an obscure space, presents to the appearance which flames affect
under the action of the magnet—an analogy which might, perhaps,
confirm the idea already promulgated, and more particularly by
Bessel, of a magnetic influence of the sun.

An astronomical labor of an entirely different kind is that which
M. Ritter has been engaged in, being the calculation of observations
of the fixed stars.   This work has been undertaken with the view of
ascertaining the cause of the abnormal result presented by the re-
duction of observations of the star γ of the Dragon, as made by M.
Main in the XXIV volume of the Memoirs of the Astronomical Society
of London.   After different calculations, made with great care and
checked by numerous verifications, M. Ritter continues to find, like
M. Main, a negative parallax, but of less value, though he took
account, which M. Main did not, of the influence of the ellipticity of
the earth's orbit in the phenomena of the aberration and the parallax.
It results therefrom that the observations are infected with errors,
proceeding, no doubt, from a defect in the stability of the instrument.
In effect, as M. Plantamour has remarked, there are reasons for doubt-
ing the exact steadiness of the old zenith sector of Greenwich, and in
submitting the observations to calculation it were to be wished that
those only were employed which are made with the new sector of M.
Airy.   At all events, it has been made to appear by M. Ritter's labors
that the calculation of an elliptical parallax essentially modifies the
results found by the circular parallax, which demonstrates the abso-
lute necessity of taking account of the ellipticity of the earth's orbit
in calculations of this kind.

Independently of the original memoirs just spoken of, the society
has had several interesting communications on astronomy.   Professor
Gautier has kept it constantly advised of the researches made by
foreign astronomers, particularly those of M. Carrington on the exist-

ence of a solar atmosphere and on the eclipse of 7th September, 1858; those of M. Wolf, of Zurich, on the relation which exists between the annual mean of the magnetic declination and the abundance of the solar spots, and of the influence of certain planets on those spots; recent investigations relative to the moon, namely, those of M. Adam on the ellipticity and inclination of the moon's orbit, and the series of observations made at Greenwich under the direction of M. Airy on the movements of that planet. M. Gautier has dwelt particularly on a very important inquiry of M. Airy relative to the progressive movement of the sun in space, an undertaking in which, by the employment of a new method, the learned English astronomer has succeeded in finding a movement somewhat less in quantity than that indicated by M. Mædler, and a direction for that movement slightly different. Finally, I must not omit the exhibition by Professor Plantamour of a very beautiful relief of the crater of Copernicus, executed on a scale of $\frac{1}{200000}$ from the photographed plates of Father Secchi. This relief, when illuminated by a bright artificial light properly disposed, represents perfectly the appearance of the crater in the different phases of the moon.

Meteorology and terrestrial physics border as much on astronomy as on physics, and establish a very natural bond between them. Thus it is to our learned professor of astronomy, M. Plantamour, that we owe several communications on the meteorological peculiarities of the years 1857 and 1858. Independently of his meteorological resumé of 1858 for Geneva and the Great St. Bernard, he has brought to the notice of the society the extraordinary dryness which prevailed from the last months of 1856 up to the middle of 1858, and the anomaly of temperature exhibited at Geneva and in a great portion of Europe at the commencement of November, 1858. From the 28th of October, before which it had been higher than its normal rate, the temperature began rapidly to sink, and the depression became particularly remarkable at the Great St. Bernard. It would seem that this anomaly was caused by a northeast wind, which in the northern regions was superposed on a southwest current, but in the regions of the southwest of Europe descended to the level of the earth's surface, where it produced the extraordinary refrigeration which was generally observed. M. Plantamour noticed, moreover, the extremely mild temperature of the 25th of December, 1858, (Christmas day,) and in reference thereto took occasion to state that the extremes of temperature, as observed for the same day, were the maximum of $+ 17°.4$ in 1857, and the minimum of $-21°.7$ in 1830.

Professor De Candolle has given the society interesting details on the organization of Russian meteorological observatories, in particular that of Tefflis, whose director is M. Moritz. This latter savant furnished observations to M. Candolle, and besides gave him information with regard to an ascent of Mt. Ararat, during which efforts were made to ascertain the depth of the cap of snow which covers the mountain, a depth which was found to exceed 30 feet, and is perhaps still more considerable.

A quite remarkable fact noticed by M. Chaix is the absence of

snow in the summer of 1858 in many localities which he visited, and which, being situated above the limit of perpetual snow, are habitually covered with it. M. Marcet, from his own observation, remarked upon the great inequality in the distribution of snow during the winter of 1858, so that while so little had never within the memory of man fallen in the valley of Zermatt, there had, on the contrary, been extraordinary falls of it in the canton of Uri in May of the same year.

We are indebted to M. Chaix for several other communications on different points of terrestrial physics and of meteorology. Such are those relative: 1st, to the meteorology of Africa, according to observations given in the travels of Barth; 2d, to the change in the bed of the Yellow river during the last three years, as noticed by Captain Osborne; 3d, to the geographical labors executed by the English, and more particularly by the brothers Gregory, in Australia, from 1842 to 1858, in the course of 24 expeditions, which traversed in the whole a distance of 32,000 miles, and which led to the discovery of a great number of salt lakes, often ephemeral, of twelve large rivers subject to the same defect, and of a great number of esculent vegetables, whose existence in that continent was not before suspected.

M. Henri de Saussure, on his part, has communicated observations which he had made on the distribution of the waters in the basin of Mexico. From these he concludes that the lakes which environ the city of Mexico have heretofore occupied a much larger surface than at present, the retreat of the lakes having been due chiefly to a canal excavated by the ancient Mexicans, and he proclaims the danger of inundation which now threatens the city of Mexico in consequence of the heedlessness of the inhabitants, who have allowed the canal to become completely obstructed.

We must not forget to mention that our notice has been called by M. Chaix to the levellings made by M. Bourdalone with a view to the opening of the Isthmus of Suez, from which we learn that the difference of level between the two seas is but a few inches.

General Dufour has likewise communicated the results obtained by M. Bourdalone in his levellings of the course of the Rhone, the descent of which is 39 metres in its passage across the canton of Geneva, admitting that the mean level of the Lake of Geneva is 373 metres above the mean level of the Mediterranean. It is true that previous levellings gave an altitude of 375 metres above the ocean; but the difference between the two numbers would appear to be owing in great part to the level of the Mediterranean being higher than that of the ocean.

While speaking of the Rhone and the lake, let us recall the observation of Professor Colladon on the azure color of their waters, which he attributes to particles from the bottom of the lake held in suspension by the agitation of the water. He founds this opinion on a fact observed by himself, viz: that while the dredging machine was at work in winter on one of the shores of the lake, the corresponding arm of the Rhone assumed the deep blue tint which commonly is only seen in summer, at which season a greater quantity of water and stronger current would produce the same effect. The cause assigned by M. Colladon may possibly contribute in part to the remarkable phenom-

enon of the coloration of the waters of the Rhone, but, as was objected at the time, will not suffice for a complete explanation.

An interesting communication was received from M. Mousson, of Zurich, through M. Soret, the object of which was to show that, in the phenomenon of water-spouts, a superior degree of validity must be conceded to the theory which refers them to the meeting of two currents of air exerting a gyratory force over that which makes them depend on an attraction produced by the electric tension of a cloud. M. Mousson has succeeded in calculating what force of aspiration is to be supposed in the case of a water-spout, and does not find it out of proportion with what is possible upon his own theory.

It is to M. Mousson that we are also indebted for some curious experiments on the effect of strong pressure in hindering water from solidifying even at very low temperatures, such as 20° below 0°. These experiments, communicated first to the Helvetic Society of Natural Sciences, were imparted also to our own.

Here, then, we find ourselves on the confines of physics, properly so called, and we enter completely upon them in recalling the communications of M. Soret and M. De la Rive on the remarkable facts respecting the congelation of water observed by M. Forbes, M. Tyndall, and M. Faraday—facts which prove the error of considering only the influence of temperature to be concerned in the solidification of water, without taking cognizance of that of the molecular attraction which plays so important a part in this as well as in other crystallizations.

It is to electricity above all that the greatest number of communications this year relate in what concerns physics, properly so called. First come those of M. Volpicelli on the electrostatic induction, made one by himself in person, the other through the medium of M. Soret. A great number of experiments, conducted under varied conditions, and subjected to different modes of proof, seem to have uniformly strengthened the confidence of M. Volpicelli in his ideas on the theory of induction; but, without entering into details, we here merely remark that it has been objected that these experiments may be also interpreted in a sense favorable to the older theory, so that, although very well performed, they cannot be deemed perhaps as conclusive as he maintains, at least in that relation.

Professor Wartman favored the society with an account of some attempts he had made, on occasion of the transatlantic cable, to determine the effects of pressure on electric conductibility. In submitting a copper wire covered with gutta percha to a pressure exceeding thirty atmospheres, he observed a small diminution of conductibility, recovering, however, its primitive value when the pressure ceases. He has also observed that strong compression on any member—an arm, for instance, of a person conveniently seated for the purpose—determines a slight but sensible current to a galvanometer of 24,000 coils, in a contrary direction to that of the current which would be due to the contraction of the same member.

M. Tirtoff, a learned foreigner, who was present at one of our sessions, communicated some experiments made with a view to ascertain

the influence of the atmospheric pressure on galvanic polarization; by which he has found that this influence is inappreciable, and that the polarization depends only on the disengagement of the gas in the nascent state.   New researches on the heat disengaged by the current, when it produces an external effect, have satisfied M. Soret of the correctness of the results which he had previously obtained, but at the same time he insists on the fact that, for an equal quantity of chemical action, the external effect produced by a current is not always proportional to the diminution of intensity, and he has given a proof derived from the action of currents of induction.

It remains for us to notice two communications of M. De la Rive, the one relating to the electro-magnetic rotation of liquids, the other to the propagation of electricity in gaseous mediums highly rarefied.

On occasion of an investigation relative to the rotary action of helices and magnets on liquids traversed by electric currents, M. Bertin had contested the accuracy of an experiment made thirty-five years ago by M. De la Rive, by means of which he had demonstrated that when a magnet is hollow the rotation of the mercury placed within it takes place in a contrary direction to that of the mercury without, the two liquid conductors having the same level and being equally traversed by a current radiating from the centre, or converging towards the centre.   M. De la Rive has resumed this experiment, and has varied it by employing as well a hollow magnet of tempered steel as a tube of soft iron magnetized either by an encircling helix or by a strong electro-magnet.   He has made use of tubes of different dimensions, both of cast and wrought iron, and has verified the accuracy of his first assertion.   A single case only occurred in which the rotation took place in the same direction within and without, and that was, when employing the hollow magnet of steel, the level of the mercury both without and within was below the magnetic pole and near the middle of the magnet—an exception which is referable probably to the influence of the second magnetic pole.

In his second experiment relating to the propagation of electricity in gaseous mediums, greatly rarefied, M. De la Rive proceeded to the consideration of the subject under two distinct points of view, namely, the action of the magnet upon currents transmitted across such. mediums, and the propagation of the currents with the phenomena which accompany it—such, among others, as the stratification of the electric light.   He began with describing the effects obtained under the former point of view, and particularly those relating to the rotation of luminous currents in different planes and with different velocities, according to the conditions of the experiment—a rotation which he had already made known ten years ago.   As to the second point, he can be said as yet scarcely to have approached it; yet he has been able to conclude even from his first attempts, of which he will communicate the sequel at some future time, that the gaseous medium, when traversed by electricity in motion, undergoes, conformably to the ideas of M. Riess, mechanical and physical modifications consisting essentially in alternations of condensation and dilatation.   M. De la Rive terminates his memoir by pointing out that his new researches

have tended constantly to confirm the theory which he had given of the aurora borealis.

Besides the communications just mentioned, M. De la Rive submitted to the society the remarkable improvements introduced by M. Leon Foucault, in the construction of curved plated mirrors designed for telescopes, and the labors of M. Hoffman, of London, in regard to the vegetable parchment which for some years has been manufactured in England with great success.

The last-mentioned communication already touches rather on chemistry than physics, and in effect there remains only, to terminate this first part of our report, an account of two important memoirs on chemical subjects presented by their authors to the Society.   The first, by MM. Deville and Troost, is directed to the determination of the densities of vapors at very high temperatures, its authors having successively employed, as the source of heat, the vapor of sulphur, which boils at 450°, and that of cadmium, which boils at 850°.   They hope to be able to make use of that of zinc, which boils at about 1200°. Among the results obtained, we will distinguish that relating to sulphur, which gives 2.2 for the density of the vapor of that substance at a very high temperature, contrary to determinations generally received, which pointed to too high a number and one not in accordance, as has been now shown, with the theoretic value.   The researches of MM. Deville and Troost are in general favorable to the opinion that at a very elevated temperature, the elements of compound bodies are dissociated, ceasing thus to exist in a state of combination.

The second communication alluded to is that by M. Pyrame Morin, on the presence of iodine in the mineral waters of Saxony, in Valais. The author had, in 1853, already indicated that this principle is present in the fountain only in an intermittent way—a result which, though confirmed by other chemists, had been contested by M. Ossian Henry. of Paris.   M. Morin has resumed his investigation by employing still more sensible reactives than at first; and new experiments have been made on sixty-one bottles of the water drawn at different times and under different circumstances.   He has succeeded in establishing with certainty that the quantities of iodine are very variable, and that between 0.2257 grains and five millionths, all intermediate quantities are met with.   These variations take place at intervals of time sometimes very distant, sometimes very close; so that several oscillations may be observed in the course of a day, which proves that the presence of iodine is really intermittent.   Sulph-hydric acid, whether free or combined, was not detected in the water by M. Morin, contrary to the assertion of M. Henry.   Bromine and chlorine exist in minute quantity; the latter very constantly, the former only when there is iodine.   It would seem highly probable that this water of Saxony proceeds from two sources, having their origin, the one in a certain rock, and the other, from which is derived the iodine, in the Cargneule.

NATURAL SCIENCES.

Having mentioned rocks and localities in connexion with the water of Saxony, we pass quite naturally, in commencing the second part of this report, which regards the natural sciences, to the subject of geology. The study of our globe, moreover, in what relates to its constitution and its composition, would seem a needful preliminary to the examination of the organized bodies which cover it, although in turn the former is singularly facilitated by the study of these same bodies in a fossil state; geology and palæontology thus forming a whole whose different parts it would be difficult to separate from one another. There is no branch, indeed, in the physical and natural sciences which involves more numerous relations to every part of our knowledge. We see a striking exemplification of this in a memoir relating to geologico-archæological researches in Denmark and Switzerland, which M. Morlot has communicated to the society, and which signalizes the remarkable relations which subsist between the development of archæology and that of geology. In effect, it is only from material indications buried beneath the soil that we can ascertain the existence of men at an epoch anterior to all traditional accounts. In imitation of the Scandinavian archæologists, M. Morlot divides this ante-historic period into three ages—the age of stone, that of bronze, and that of iron. It is only with the age of iron that figures of men and plants make their appearance, as well as coins and alphabets; it is the aurora of history. Different details are presented by M. Morlot respecting these three ages, and the material traces of them which have come to light.

As regards geology proper, we have first to notice a memoir by M. Marcou on the classification of the *new red sandstone in Europe, North America, and India.* The author considers this great series of strata as intermediate between the primary and secondary periods, deciding for this middle term after having discussed the often controverted question whether the *permian* ought to be annexed to the secondary formations. He distinguishes two formations in this group: 1st, the *trias,* the composition of which is known; and 2d, the *dyas,* comprising the *zechstein* and the *rothliegende.*—(See Archives des Sciences, Ph. et Nat., 1859, t. V.)

Another geological memoir is that of M. Favre on the geology of the Môle, which forms a portion of the great work of our colleague on the liasic and keuperian formations of Savoy, (printed in our memoirs.) Among other observations of M. Favre we must make mention of that which relates to a fine deposit of fossils near the summit of the Môle, in which he has succeeded in discriminating forty species which pertain to the lias formation; but a remarkable circumstance is, that the fossils of three stages of this formation are associated in one and the same stratum. M. Favre has noticed several localities of the Alps and the Cevennes where this association has been recognized, and he has been led to the conclusion that the causes

are to be found in the physical nature of the deposit and in the submarine character of the soil.

Professor Pictet on this occasion submitted to the Society some general observations on the association, in the same locality, of the fossils pertaining to different formations, an association which he thinks may be explained by three different causes: 1st, by the fact that the relics of dead animals belonging to one epoch might be preserved for a certain time in the waters containing the living animals of the succeeding period—a case which would be very rare; 2d, by the fact that some robust species, that is to say, very abundant in one stratum, have survived the cataclysm which had occasioned the destruction of the general fauna, and reappear in small numbers in the succeeding fauna; 3d, by the fact, finally, that one portion of the sea has undergone changes less decided than others, and that in a gulf, for instance, we find the fossils of two different epochs associated, while elsewhere the two faunas remain perfectly distinct.

We have passed, almost without perceiving it, from geology to palæontology, sciences which in truth are now inseparable; and here M. Pictet must be again cited for remarks submitted to the Society on a communication by M. de Saussure respecting the discovery of fossil bones of domestic animals in the environs of Charleston. These bones pertain only to the postpliocene formation, of which the fauna is composed: 1st, of extinct animals; 2d, of animals not living at this time in South Carolina, but still existing in other parts of America; 3d, of species still living in the country. After having shown that this formation, in a palæontological point of view, presents the same character as the correspondent formations of Europe, and having discussed different hypotheses to explain the presence of the fossil bones of domestic animals, M. Pictet seems disposed to believe that there has been simply an accidental mixture of recent bones with postpliocene remains.

Besides the communications just noticed, M. Pictet read before the Society a memoir, in the preparation of which M. Campiche, of Ste. Croix, was associated, on the nautili, and more especially cretaceous nautili. After reciting that the nautili are one of the kinds, not many in number, which are met with in all geological epochs, M. Pictet has shown that their distinctive characters may be classed under four heads; and though he has essentially occupied himself, in that part of his investigation prosecuted in common with M. Campiche, with the nautili of the Jura, he and his associate have compared a very great number taken from all formations and all countries, with the view of arriving at a decision as regards the order of succession in their forms. They have ascertained that the definite species of the same epoch are exactly alike in all countries, and that this is also the case with the types imperfectly defined, so that it becomes an immediately accessory question whether these types are species or varieties. This special question recalling M. Pictet to some general considerations on the subject, he is led to regard the three following laws as being at the basis of all palæontology: 1st, every species has had in its palæontological development a limited duration; 2d, the co-

temporary species have appeared and disappeared at the same time, the causes of appearance and disappearance having been the same for all; 3d, neighboring formations present analogous forms. As to exceptions which may occur with respect to the two last laws, we have indicated a moment ago in what manner M. Pictet has sought to account for them.

Professor D. Candolle, in reference to the geological duration of species, cited the investigations of M. Gaudir respecting the fossil vegetables of the quaternary epoch in the repositories where certain actual species of Europe are found—as the beech, for instance— mingled with species which now live nowhere but in the United States; this forming an additional exception to the law of the simultaneous extinction of species.

M. De Candolle made, besides, several communications relating to vegetable physiology and botany proper, among which may be cited an analysis of the researches of M. Duchartre on the organ which produces the perfume in the vanilla, and a monographic study of the family of Begoniaceæ, of which one species (the *begonia aptera*) presents the remarkable peculiarity of being furnished with parietal and unequal placentas, contrary to what takes place in other species of that family. He directed attention, also, to the existence of a small insect which had, last year, occasioned the destruction of numbers of fir trees.

In the province of botany we have still further to cite communications by MM. Choisy and Duby. The former described to us an ivy-plant which he had observed near Peissy, growing on a horse-chestnut, and remarkable for its exceptional dimensions as well as for the singular fact that the branches hanging free bore leaves of a beautiful form and different from those of the branches which had attached themselves to the tree. He communicated, besides, a memoir on two kinds of plants little known, assigned to the family of guttiferæ, (*gynotroches* and *discotigene*,) which both belong to the island of Java. The second of these should continue to be retained in the family of guttiferæ; the first should be transferred to the family of rhizophoraceæ, as Blume and Bentham had already pronounced.

M. Duby, besides some communications on the botanic investigations made by learned foreigners, read to the society a paper on a species of *dothidea*, a cryptogam which grows on the Barbary jessamine, (*lyceum europæum*,) and which in the same pustules passes through three successive states, viz: a pulviscular state, a spermatic and a thecasporic one. M. Duby, in presenting the history of the development of this minute object, dwelt upon some questions of taxonomy which connect themselves with that development, as well as on the necessity, in the actual state of cryptogamy, of multiplying observations on the evolution of the reproductive organs of champignons.

It remains, in order to complete what we had to say on organic natural history, to speak of transactions relating to zoology and animal physiology. First in order we find the researches of M. Edouard Claparede on the organization of infusoria, presenting, after a review

of the different opinions put forth on this subject for twenty-five years past, the result of his own observations on the structure of these animals, accompanied by a series of drawings relative to that structure. He first showed that the general type of the infusoria presents an exterior cuticle covering a parenchyma of more or less thickness, which itself circumscribes the general cavity of the body; the cuticle and parenchyma pierced by two openings, which are the mouth and anus; then, after describing the mechanism of the circulation of aliments in the interior of the general cavity of the body, and of their digestion in the same, he passes to the examination of the circulatory apparatus, which he regards as a closed vascular system comparable in all respects with sanguineous systems. After some other details, M. Claparede has indicated what are the natural affinities of infusoria, and what position should be assigned them as animals related on one hand to the vermes tubellariæ, and still more on the other to the cælenteres, (polypi and acalephi,) as regards their digestion chiefly, yet differing from them in a radical symmetry of form, while the cælenteres proper are characterized by a radiate symmetry not to be mistaken.

M. Claparede has likewise communicated his microscopic observations on the organs in the antennæ of insects described as auditory by M. Lespes, and has shown that the auditory and otolithic sac imagined by that author is but an optical illusion, and that an examination of transverse sections, delicately conducted, proves those supposed organs to be only hairs fantastically modified. In regard to the use of the microscope, he has noticed a singular effect observed in looking, in the direction of its axis, into a very minute capillary tube, which plays the part of a bi-concave lens, although the liquid which fills the tube be the same with that which bathes all the parts of the body in which the tube is pierced, and although the surfaces which limit the liquid be perfectly plane.

We have yet to recall some other communications by M. Claparede: 1st, the demonstration of the electrical organs of the *malapterure* and the *mormyrus oxythyneus*, derived from a dissection of these electric fish, specimens of which had been sent him by M. De la Rive; 2d, an examination of the researches of M. Lebert on the malady of silk worms, from which it seems to result that there is no other remedy to be hoped for but the destruction of all the animals attacked; 3d, an account of the experiments of M. Heidenheim relative to the application of ligatures on different points of the hearts of frogs, the effects of which are entirely contrary, according to the place where the ligatures are placed; 4th, an analysis of the researches simultaneously but independently made by MM. Kolliker and Wedl, from which it results that the minute channels noticed by English naturalists in the shell of most of the molluscs are due to the action of a perforating vegetable parasite; channels which M. Claparede had himsef distinguished, in 1855, as not pertaining to the mother shell, but as hollowed by some parasite, which he then wrongly judged to be of an animal nature.

M. Henri de Saussure continued his account of the interesting

observations made by him on the habits of Mexican birds, illustrating
many facts regarding them which had escaped the attention of former
travellers. To M. Duby we were indebted for a report on the micro-
scopic researches of M. Amici relative to the constitution of the
muscular fibre.

The investigations of which we have been speaking, however
special they may be, are none of them deficient in point of general
interest, whether as forming necessary links in the great chain which
binds together all the phenomena of nature, or because, considered
in themselves, they reveal some of the mysteries, every day more
remarkable, of the physical world. But this is not the only advan-
tage derived from the introduction of specialty into the study of the
sciences; one still more considerable is that from this very specialty
there spring up between the different parts of those sciences new and
more intimate relations, in virtue of the greater perfection introduced
into researches. This connexion is particularly striking in animal
physiology, to such a degree, indeed, that sometimes we know not to
what branch of the sciences, physical or natural, we ought to refer
such or such an investigation. Is it to physiology or to physics that
the researches of MM. Thury and Claparede belong? the former on
the amount of mechanical force expended in walking, the latter on
the horopter. Whatever the place to be assigned them, we must
not omit them in this report, as the authors have communicated them
to the Society.

M. Thury has found 7.2 kilogrametres as the value of the labor
performed by a man for every metre of distance which he travels,
answering to 10 or 12 kilogrametres a second, according as the daily
course is 8 or 10 leagues, the mean weight of the body being fixed at
65 kilograms. He deduces from his calculation that the longest
line over which a man can pass in a day on a horizontal level, without
permanent exhaustion of his force, is 48,000 metres, (157,473.6 feet,)
and that the greatest vertical height a man can attain, under the same
conditions, in ascending along an inclination of $\frac{1}{3}$, is 4,000 metres,
(13,122.8 feet.)

M. Claparede communicated a series of experiments designed to
show that the form of the horopter* is different from that which, as
a consequence of the investigations of M. Meissner, German physiolo-
gists at present assume it to be. The horopter, as Meissner con-
ceived, is in a majority of cases a right line inclined on the plane of
vision of a quantity which varies with the distance of the point of
view. After demonstrating by conclusive experiments that this deter-
mination is erroneous, and that the line experimentally found by
Meissner is always perpendicular to the plane of vision, M. Claparede
believes himself authorized to conclude that this line belongs to a
horopteric cylindrical surface, having for its base the horopteric cir-
cle of Pierre Prevost, rejected by M. Meissner. Subsequent experi-
ments have convinced him that the horopter is really formed but of

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* The surface of *single vision* corresponding to any given binocular *parallax* is called the
horopter.—(See Nichol's Encyclopedia of Physical Science)

two lines, to wit: the circumference of the circle determined theo-retically by Prevost and wrongly attributed by authors to Vieth. a circumference situated in the plane of vision, and of the right line perpendicular to the plane of vision, to which allusion has just been made, and of which M. Alex. Prevost, and later M. Fritz Burkhardt, had already given a theoretical determination. The horopter of Meissner must therefore be entirely rejected.

It is above all with electricity that animal physiology has relations which become every day more intimate. The Society has been occu-pied on two or three several occasions with questions which connect themselves with this subject. M. Lefevre, of Dijon, has called atten-tion to his experiments on muscular and nervous excitability and irritability after death, which he has succeeded in measuring by determining the intensity of the current necessary to produce excita-tion. In operating on the frog he has found that the irritability of the sciatic nerve goes on at first augmenting for an hour after death, then that it gradually loses its excitability while the muscular con-tractibility develops itself and attains its maximum at the end of 36 hours; four or five hours before the cadaveric rigidity supervenes.

Professor De la Rive offered, on his part, some considerations on the relations between electricity and the nervous action, while dwell-ing more particularly on the experiments recently published by M. Bernard, which appear to him more favorable to the identity of the two forces than the author seems to believe. M. De la Rive points out especially the analogy which exists between the action of electri-city and the nervous action as to the peculiar state in which one and the other place the nerve, this last not acting as a simple conductor, as some physiologists have supposed, but really in virtue of its electro-molecular constitution, which may be altered by chemical means, such as the vegetable poison known by the name of *curare*. Recalling the remarkable observations of M. Dubois Reymond, which are altogether favorable to this way of thinking, he judges that it is not necessary to admit in the organic molecules other electric properties than those which belong in general to the molecules of inorganic matter, and that it is sufficient to suppose that every atom, whether it forms part of an organized body or one not organized, is endowed with two opposite electric poles. Only in the first case, namely, that in which there is life, a new force, the *vital force*, determines, by the particu-lar disposition which it impresses on the particles, an arrangement which permits the manifestation of their electric properties.

I have mentioned the *vital force*, and here would be the occasion of giving an account of the long and interesting discussion to which the simple enunciation of the existence of this force gave rise. There was here a general question connected at the same time with the most difficult points of organic natural history, and with the most deli-cate conceptions of the philosophy of the sciences, namely, those which relate to forces, their nature, their mode of manifestation, and the relations which exist among them. Hence, the physicists and the chemists, as well as the naturalists, took part in this discussion, which we must content ourselves here with merely commemorating, without

pretending to reproduce what would far exceed the necessary limits of this report.

M. Claparede, who first directly entered on the subject, which an incidental remark by M. De la Rive had thus introduced, maintained that there is an impossibility of pronouncing positively in the actual state of physiology, on the existence of vital forces, and that there is a necessity, if we admit their existence hypothetically, of considering them as general forces of nature, manifesting themselves only under certain circumstances, the result of which is organization.

Dr. D'Espine and M. Thury, in written memoirs, pronounced strongly in favor of the existence of special vital forces, proper only to organized beings—forces to which M. Thury assigns a peculiar character, distinct from that of inorganic forces, considering them as *schematic* forces, that is, producing the type and needing for their manifestation the concurrence of the organic forces from which they borrow the law of their operation.

MM. De la Rive, Pictet, Marignac, and Colladon gave in succession their ideas on this subject, and while agreeing as to the necessity of admitting that there are in organized bodies phenomena which known physical forces do not suffice to explain, they yet differed both as regards the nature of their arguments in favor of the existence of a vital force, and as to the importance of the part fulfilled by this force in physiological phenomena.

After having taken part myself in this discussion, as just stated, and having followed with care its different phases, there have remained on my mind some personal impressions which I may be allowed here to reproduce. A first impression is that whatever may be said, there is an abyss between the ordinary forces of inorganic matter and those which produce life, with the phenomena which accompany it; it appears to me, then, impossible not to admit a force, or, if one chooses, a special principle of activity in living beings, the absence of which constitutes the state of death. A second impression is that the notion of a vital force has been often abused by supposing it to intervene directly where the intervention of ordinary forces is perfectly sufficient, and that in this respect vitalism badly understood may have injured the progress of physiology. A third impression is that the principal objections urged to the existence of the vital force themselves rest on hypotheses still more improbable than the hypothesis which they are designed to combat. Thus resort is had to the hypothesis of one unique matter and a unity of force, whereas nothing rests on less proof; and as to the unity of force in particular, they found it on the principle of the transformation of forces one into another, without considering that the principle is only true of the mechanical effect produced by those forces and not of the forces themselves, and that it is besides impossible not to acknowledge that there must be forces or principles of activity not subjected to the law of the mechanical effect.

But enough of this subject. As we have seen, our Society, though it is essentially a reunion of men of specialities, does by no means disdain general questions. Doubtless it ought not to surrender itself to them,

nor does it but in moderation; yet it does not fear, when the occasion arises naturally, to broach them with freedom, for it is due to the union which subsists between its members, to the kindly familiarity which presides at its sessions, that it should indulge in discussion at once perfectly free and perfectly courteous, equally remote from too much compliance and too much insistence. Let us always preserve, Gentlemen, this custom, consecrated by our predecessors, and although I am far from proscribing men of genius, if our good fortune should introduce such into our circle, let us at least avoid the reproach which was attached to them of old, *genus irritabile vatum.*

I have thus presented a summary of the transactions which have occupied the nineteen sittings of the Society from July, 1858, to the end of June, 1859. I have not entered into administrative details, which have been few during the year, and which have consisted essentially in some elections and the publication of the second part of the 14th volume of our memoirs. In the month of January, Professor Pictet De la Rive was designated as vice-president, to become president from July, 1859, to June, 1860. M. Edouard Claparede was elected secretary for three years, succeeding M. Louis Soret, whose functions had expired, and who had declined a re-election. You have provided, lastly, by numerous nominations, for the places rendered vacant by death in the ranks of our honorary members, and at the same time limited to seventy the maximum number of those members.

Our Society has never made pretension to offer its diplomas to all the men who honor science by their labors; hence your choice has fallen, as on previous occasions, only upon those among them who have been kind enough to give us some testimony of their good will, either directly or indirectly.

I have said that death has made numerous vacancies among our honorary members, and though it has not been our custom to speak here of those whom we have thus lost, you will permit me, I am sure, to make one exception in favor of the individual whom the whole scientific world considered as its chief and honored as its presbyter.

Alexander de Humboldt was the oldest of our honorary members; the intimate relations which he had sustained with our two illustrious compatriots, Marc-Auguste Pictet and Pyrame De Candolle, had constantly predisposed him favorably towards our society and towards all the Genevese who were occupied with science. Having myself experienced his kindness, I still retain the impression of the friendly reception which he extended to me at Berlin in April, 1858. I found him then as I had known him thirty years before; his intellect had lost nothing of its extent and clearness; his conversation was always as rich and animated, his conceptions as lively and as rapid. I shall not attempt to recount that long and noble life, nor even to sketch it: it is a work of time which would be beyond my strength. I aspire but to one thing, to render a modest but profound homage to that vast intelligence which touched on almost all the points of human knowledge, and which has left monuments of its activity in every branch of the sciences, physical and natural. What essentially characterized Hum-

boldt was the necessity of embracing in his researches the whole of nature—the *Cosmos*. Thus it was, above all, the study of our globe itself which was the object of his constant predilection, and for which he went to gather materials in every quarter of the world. Did universality then impair in him to a certain extent originality, and were his discoveries less brilliant on that account than those of his illustrious cotemporaries? It is possible: one cannot be at the same time a Humboldt and a Volta. But his part has been sufficiently honorable and his influence sufficiently great in the world of science to leave nothing wanting to the lustre which attends his name. He died the 6th of May last, at the age of 90, in the plenitude of his faculties, full of years and of glory. I cannot better characterize him than by recalling here the judgment which he passed upon himself: "I am not a savant," he said to me at Berlin, eighteen months ago. "The world, then," I promptly replied, "is much deceived in regard to you." "No, I am not a savant, such as they represent me," he rejoined earnestly; "my principal discoveries have been the discovery of learned men, and my principal merit is to have caused science to be loved." Perhaps he had reason to regard this kind of glory as his first title to the admiration and the gratitude of posterity. There will always be savants who will cause science to advance, but the Humboldts and De Candolles who cause it to advance at the same time that they cause it to be loved, who encourage labor in others and themselves set the example—these are types as rare as they are precious, and when they disappear it is not science alone, but still more those who cultivate it, who ought to mourn for them.

PRESENT STATE OF ETHNOLOGY IN RELATION TO THE FORM OF THE HUMAN SKULL.

BY PROFESSOR ANDERS RETZIUS, of the Carolinska Institute, Stokholm.

Twelve years ago I presented to the assembly of Scandinavian naturalists some considerations on the form of the human skull among different nations, considerations based upon facts which I had communicated two years before. The doctrine sketched at that time was entirely new, and had been submitted to no competent proof; its destiny seemed uncertain, and many gaps remained to be filled up. But since that epoch, the classification proposed for skulls of different form has been more solidly established, and may be affirmed to be complete, as I here propose briefly to show.

A.—FORMS OF THE SKULL IN EUROPE.

At the time referred to, I had indicated that the majority of the nations of western Europe are dolichocephalæ, while the brachycephalæ predominate in eastern Europe.* This assertion has since been confirmed from different quarters.

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*A communication from Professor J. Aitken Meigs, of Philadelphia, furnishes the translator of this paper with material for a note which cannot be otherwise than acceptable, whether to the general reader as an explanation of scientific terms, or to those who propose to enter on such inquiries, especially to the observant traveller in little-explored countries, as an indication from a highly competent source of the measurements proper to be taken for determining the ethnographic character of the human head.

"The late Professor Anders Retzius, of Stockholm, divided the races of men into two great groups according to the form of the head, or rather according to the ratio existing between the length and breadth of the skull. Nations in whom the head is developed chiefly in the occipito-frontal or longitudinal diameter he called *dolichocephalæ*, or *long-heads*, (from δολιχος, long, and κεφαλη, head;) races whose skulls are developed in the bi-parietal diameter particularly, or in the direction of the breadth, to such an extent as to exhibit a more or less rounded or square form, he termed *brachycephalæ*, or *short-heads*, (from βραχυς, short, and κεφαλη.) Both the dolichocephalæ and brachycephalæ he again subdivided into the orthognathæ, or straight-jawed, (from ορθος, upright, and γναθος, jaw,) and the prognathæ, or prominent-jawed, (from προ, forwards, and γναθος)" In other words, nations with a perpendicular profile are orthognathic, as the Germans, Anglo-Saxons, &c ; those with a retiring profile are prognathic, as the negro.

"Retzius's division of the human family," continues Professor Meigs, "is liable to the objection that it forces into one or other of these two classes races whose skulls in point of conformation occupy an intermediate position. The measurements by which differences in size and form of crania are determined are variously taken by different craniographers. Some systematic writers make many of these measurements, others apply but a few. The occipito-frontal or longitudinal diameter is ordinarily measured from the glabella to the most prominent point of the occipital bone. The glabella is the prominence on the frontal bone, between the orbits and just above the root of the nose. The most projecting point of the occiput corresponds in the majority of cases with the external occipital protuberance or boss. This diameter gives the length of the head. The bi-parietal diameter coincides with the breadth of the head, and is generally measured from one parietal protuberance to the other. The depth or vertical height of the skull is measured from the

Dolichocephalæ of Europe.

Germans.
  {
    Norwegians and Normans of France and England,
    Swedes,
    Danes,
    Hollanders,
    Flemings,
    Burgundians,
    Germans of the German stock,
    Franks,
    Anglo-Saxons,
    Goths in Italy and Spain,
  }

Celts. . . .
  {
    Scottish Celts,
    Irish Celts,
    English Celts.
    Welsh,
    Gauls of France, Switzerland, Germany, &c.,
    Romans proper,
    Ancient Hellenes and their descendants,
  }

} Orthognathic.

Since the period when for the first time I announced this classification, which may be found in the transactions of the first meeting of Scandinavian naturalists at Christiana, I have examined a great number of individuals descended from Norman families in France and England. All these, without exception, have preserved the same oval form of the skull which characterizes the Normans properly so called in Norway. I have studied, besides, by hundreds, the Swedish heads found in ancient tombs or in cemeteries, or obtained from anatomical amphitheatres, and in all these skulls the same form which I had described has been found to prevail.

Some years ago, in levelling the Riddarholm, an entire cemetery was laid open, in which were found skulls and relics of skeletons. A

middle of the anterior margin of the great foramen in the base to a point in the vault of the cranium directly above. The frontal diameter or breadth of the forehead is measured between the most protuberant points of the frontal bone, behind and above the external angular processes. The breadth of the face is taken between the zygomatic arches. The height or length of the face is the distance between the point of the chin and the root of the nose. The horizontal periphery is measured by means of a graduated tape passed around the cranium below the superciliary ridges of the os-frontis, and over the external occipital protuberance. This is the horizontal circumference of the calvaria or head proper. The occipito-frontal arch is measured from the root of the nose over the top of the head in the median line, to the posterior margin of the great foramen at the base. These are the most important external measurements of the skull; many others, however, might be enumerated. In my own system, as yet unpublished, I have adopted 54 different dimensions as necessary to express fully all the ethnological peculiarities of the cranium."

Other terms, it may be added, have been proposed for the designation of particular forms of the cranium, as *platycephalic* for those distinguished by horizontal expansion of the vertical region, a feature which, when joined with somewhat low elevation of forehead and great width between the angles and condyles of the lower jaw, imparted to the countenance, says Professor J. B. Davis, of England, that quadrangular appearance so commonly observed in the statues of ancient Romans of consular and imperial times. *Acrocephalic* or elevated, whose leading characters are "great antero-posterior length; smallness of biparietal measurement, with apparent compression of the sides; roundness and projection of frontal region; absence of sagittal suture; this last being the determining cause of all the other peculiarities."—(Report of the British Association, 1857, p. 146.)

large proportion were in perfect preservation, and the skulls presented, almost without exception, the characters of the German type. A result altogether similar was furnished by the examination of skulls found in the city of Stockholm itself, at the place known by the name of Själagaordsgata, *(Street of the Abode of Souls,)* near which there was once the cemetery of a convent.

Since the epoch of my first researches I have visited Copenhagen and studied a great number of skulls belonging to the museum of that city; I have also had an opportunity of examining the skulls of a great number of Danes, and have found the German dolichocephalic form well maintained. I have verified the same fact in Holland and in the Flemish portions of Belgium and France. Moreover, Professor Vrolik, of Amsterdam, has sent me skulls of the same form found in the ancient tombs of Holland.

During an excursion in Great Britain in 1855 I was able to satisfy myself anew that the dolichocephalic form is predominant in England proper, in Wales, in Scotland, and in Ireland. Most of the dolichocephalæ of these countries have the hair black and are very similar to Celts.

Through the kindness of a distinguished archæologist, M. Frederic Troyon, whose activity is well known, I have received for the museum of Stockholm several skulls of Burgundians, derived from the ancient tombs of that race in the Canton de Vaud. All present the same Germanic form.

The first Roman skull that I had an opportunity of seeing was sent to me by the late Dr. Prichard. It had been picked up on an ancient field of battle near York with another skull of different form. Dr. Prichard desired to know my opinion on the nationality of these two skulls, but he studiously kept from me any information which might serve to guide my conclusions. I ascertained that the first of these two skulls had a dolichocephalic form altogether peculiar, which was not yet represented in the collection of the Carolinska Institute. I found, however, that this form coincided perfectly with the description and figures of Roman skulls which have been left by Blumenbach and Sandifort. The other skull was smaller, much elongated in form, straight and low, and had evidently belonged to a Celt. My conclusion then was that the former was a Roman and the latter a Celtic skull. This judgment fully satisfied Dr. Prichard, since these two skulls had been found, as he told me afterwards, in a field called in other times the field of the emperor Severus, and that the Celts *(Belgæ Brittanorum)* had been defeated in that place by the Romans. The Celtic skull bore on its posterior part the mark of a mortal blow, received, doubtless, in the act of flight, while the wound which had caused the death of the Roman had passed athwart the orbit. Since that time many authentic Roman skulls have been found and studied by Drs. Davis and Thurmam. Some of them were shown at the British Association for the Advancement of Science, which met at Glasgow in 1855, and Dr. Davis has conferred on the museum of the Carolinska Institute at Stockholm a specimen of a Roman skull in good preservation, taken from a columbarium near the Appian Way, not far from Rome. All these skulls offer a remarkable resemblance in form

and dimensions. They are of dolichocephalic form, but extraordinarily large, principally above the ears, with the parietal tuberosities largely developed, the occipital protuberance very projecting, and altogether of considerable volume.

I have introduced the Hellenes into my enumeration of the dolichocephalæ of Europe. My reasons for this were elsewhere given in 1847.* Nevertheless, according to all the facts that I have able to collect, the dolichocephalic form has never appertained to the majority of the Greek nation, which presents, on the contrary, the characters of the brachycephalæ. The brachycephalic form appertains to the Greek Sclaves, as well as to the majority of the Levantines and of the Pelasgi, the Albanians of the present day.   In the paper before cited I have already drawn attention to the fact that certain antique statues, such as those of the Apollo, the Venus, and others of the most noble character, pertain to the dolichocephalic type, while others, like those of the Jupiter and Hercules, are brachycephalic; which differences result, no doubt, from the difference of the races to which the individuals belonged whom the artist wished to represent.

Brachycephalæ of Europe.

Under this head are comprised—

Ougrians, (Muller, Latham) { Samoieds, Laplanders, Woguls, Ostiacks, Permians, Wotiacks, Tsheremisses, Mordwins, Tshuwashes, Magyars, Finns {Finlanders, Esthonians, Livonians,	Orthognathic.
Turks ·	Orthognathic.
Sclaves. { Czéckes, (Bohemians,) Wends, Slovacs, Morlacs, Croats, Servians, Poles, Russians, Modern Greeks,	Orthognathic.
{ Lettes or Lithuanians, Albanians, Etrurians, Rhetians, Basques,	Orthognathic.

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* V. Oefversigt af Kongliska Vetensk. Academ. förhandligar, 8 September, 1847.

I have not myself had the opportunity of examining several of the populations enumerated in this table; relying, however, on data derived from various sources, I venture to pronounce the opinion that they all ought to be ranged among the Brachycephalæ. It seems, indeed, a feature in the order of the world that all the dominant races of eastern Europe, which occupy the vast tract of Russia in Europe, Turkey, Greece, and a great part of the Austrian empire, are brachycephalic.

Many interesting skulls belonging to some of the tribes just enumerated have been recently received by the museum of Stockholm. Thus the celebrated anatomist of Vienna, Professor Hyrtl, has sent me the skull of a Croat of the military frontier, characterized by its height, its capacity, and its almost cubic form; also a Morlac skull from Dalmatia, large, lofty, and brachycephalic. Several Slovac skulls from Olmutz have been procured for me by Professor Bonsdorff, with two Esthonian skulls, a Turkish, and several Finnish ones. Professor Willebrand, of Helsingfors, has sent me two Carelian skulls. Moreover, I have myself examined several living Rhetians, as well as Basques; and I have received from Dr. Eugene Robert, of Paris, some superb Basque heads for our museum. On different occasions I have met with brachycephalic Scots from northern Scotland and the isles to the north of it. During my last sojourn in Scotland I encountered again divers individuals pertaining to this same type, having an expression altogether peculiar, their visage being often short and somewhat large, their hair red, the skin of their faces marked with freckles. Since then I have learned from the report of travellers that this type is common in the Highlands, where it is indigenous from a remote antiquity. I suppose that it has descended from the Finns, or perhaps the Basques.*

B.—FORMS OF THE SKULL IN ASIA.

Dolichocephalæ of Asia.

Hindoos,
Arian Persians,   } Orthognathic.
Arabs,
Jews,
Tongouses,   } Prognathic.
Chinese,

The area inhabited by these populations is restricted to the southern regions of the great Asiatic continent, viz: the following countries:

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* Since this was written, the author has been able to examine a considerable number of skulls of Tuscany, Lombardy, Piedmont, Tyrol, and Switzerland, and has arrived at the conviction that the brachycephalic form prevails in those countries in company with the black color of the hair. The same remark may be made with regard to a majority of the inhabitants of Baden, Wirtemberg, and Bavaria. In France, the Basques offer the same characters as to the form of the head and color of the hair. It is nearly the same with the population of Saxony and Austria. In these last countries the population is, without doubt, of Sclavic origin, while it is probably of Greek origin in Italy, Tyrol, and Switzerland.—*Note by the author.*

Arabia, Persia, Hindostan, and China, (not comprising Mongolia and Chinese Tartary.) Whether to the north or south of this area, we find brachycephalic populations, which are, moreover, disseminated here and there among dolichocephalæ of Asia.

I have here arranged the Chinese and Tongouses among the dolichocephalæ, though they have been generally classed by others among the Mongols. In effect, the examination of a great number of skulls has confirmed the observation which I had made long ago, and which Latham has cited,[*] that the Chinese proper have the head elongated, with the occipital protuberance very prominent; but this prominence is associated with a decided jutting out of the parietal tuberosities, which causes the contour of the skull to approximate to an elongated pentagon more than an oval. I have received several skulls of Chinese, whether real or moulded in plaster, from England, through Dr. J. B. Davis; from Holland, (Prof. Van der Hoeven;) from St. Petersburg, (M. V Baer;) and from the expedition around the world of the frigate Eugenia, (Messrs. Andersson, Kinberg, Eckströmer;) all present, as it seems to me, the same characteristic form. As regards the Tongouses, I ought to observe that I have but a single skull to serve as the basis of my decision, to wit, a mould in gypsum, which was sent me in exchange by Professor Purkinje, of Prague. I have every reason to believe that this mould is from the Tongousian skull which Blumenbach has described and figured in his second decade: "Facie plena ad arcus zygomaticos latissima, fronte depensa, &c., olfactus officina amplissima, occiput mirum in modum retro eminens ita ut protuberantiæ occipitis externæ distantia a dentibus incisoribus superioribus 9 pollices æquaret."[†] The collection of Blumenbach belongs now to the museum of the Physiological Institute of Gottingen, where it is in charge of the learned director, Professor Rudolf Wagner, who has had many of the most remarkable skulls of the collection moulded by a skilful artist, in order to place them within reach of other museums.

A striking resemblance is to be remarked between this Tongousian skull and those of the Esquimaux. The form of the face is identical: the visage is flattened, very large above the zygomatic bones, the upper jaw ample and prominent; the arch formed by the alveolar processes and the teeth is large, as among the Esquimaux and Greenlanders. The same conformity exists in the capacity of the head, the elongation and size of the occipital protuberance. These characters again are to be found in a large portion of the Chinese skulls of our collection, and it is on this account that I have thought that in this Tongousian skull might be discovered the intermediate link between the form of the skull of the Chinese and that of the Esquimaux.

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[*] Natural History of the Varieties of Man, 1850, p. 16, "Physical Conformation."
[†] Decas Collectionis suæ Craniorum diversarum Gentium, II, Table XIV.

Brachycephalæ of Asia.

Ugrians, (Samoiedes, Yakouts, &c.)
Turks.
Circassians, and probably a majority of the numerous tribes of the Caucasus.

Turcomans,
Afghans,
Lascars,
Tartars and Mantchoo-Tartars,   } all prognathic.
Mongols, as well in Asiatic Russia as in Mongolia,
Malays,

The "Indian Mongolidæ," in Dr. Latham's *Varieties of Man*, probably belong also to this division.

These populations embrace all the great Asiatic continent, with the sole exception of the countries of the dolichocephalic organization which I have given above, namely, India, Persia, Arabia, China, and a small part of Siberia. The brachycephalæ, however, as before indicated, form small communities disseminated through the midst of the dolichocephalic tribes already enumerated. In Asia, then, as in Europe, the brachycephalic form of the skull is predominant, but with this difference, that the Asiatic brachycephalæ are, the greater part of them, prognathic.

C.—FORMS OF THE SKULL IN AUSTRALIA.

Dolichocephalæ of Australia.

Australian negroes, all prognathic.

Our information relative to these people is yet so incomplete that I have not ventured to present a table of denominations, and I limit myself to saying that by the study alike of the Carolinska Institute collection and of others, and by an examination of many published works, I have acquired the conviction that dolichocephalic tribes exist on nearly all the Australian islands. All the savage nations of the Australian continent proper, New Holland, and of Van Dieman's Land, appear to be prognathic dolichocephalæ. On the other isles we find in addition brachycephalæ, (Malays, Polynesians, and Papous of Guoy and Gaimard.) Most frequently these tribes are black or blackish. Hence the name of *Australian negroes* has been given them. Moreover the form of their skulls resembles altogether that of the negroes. Many of these tribes have the hair closely crisped, but long and, so to say, felted into a bushy perruque; others have it straight. Our collection possesses the skulls of such brought from a great number of the isles of the South Sea and Pacific. They resemble one another in a striking manner, and in general are small, but thick, presenting in this point of view also an approach to the negro type. They are in size much less than the Chinese, but they have, like them, large parietal protuberances which rarely occur among the negroes. Their

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occipital protuberance is much developed and a little compressed laterally. The zygomatic arch is not greatly salient, nor is the nose flattened as in the negro; the brow is narrow and low. I have lately received from Professor Bonsdorff, of Helsingfors, skulls of this form brought from Woahu, in the archipelago of the Sandwich islands. The Danish frigate *Galatea* has furnished several others from the Nicobar islands, on which Professor Ibsen made an interesting report to the convention of Scandinavian naturalists at Stockholm, in 1851, besides having had the kindness to remit one of the specimens to our anatomical museum.

Through the kind offices of Dr. R. G. Latham, our museum is also in possession of a skull of the nation of Borneo, which is known by the name of Dayak. This is equally dolichocephalic. The half of another is preserved in the collection of the University of Christiana, and presents the same peculiarities of form. I have seen, moreover, many altogether similar at London. These Dayak skulls are small, but solid; their parietal protuberances are rather smaller than those of Australian negroes. All the skulls of Dayaks that I have seen are ornamented with figures symmetrically carved on the front, vertex, and temporal regions as far as the lambdoidal suture; some of which figures are colored a dark brown, with here and there small spots of a bright red or blue. "Before a young man can aspire to matrimony," says Dr. Latham, speaking of these Dayaks, "he must lay at the feet of his betrothed the head of a man of another tribe, slain by his own hand. Every marriage, then, supposes a murder. I suspect, however, that this observance is not so general as the rule exacts. Another characteristic trait of the Dayaks is their passion for possessing skulls; hence skulls form the chief ornament of a Dayak house, and their possession is the best proof of virility."

From all that I can draw from different data, the Dayaks are black of color, like the majority of the Australians. I believe that all the tribes called Alforous, or Haroforous, are prognathic dolichocephalæ, like the majority of those to whom we give the name of Papous or Papuans,* but who are not to be confounded with the brachycephalic Papous described by Guoy and Gaimard. A great number of tribes among these Australian negroes construct their habitations on piles reared above the water. M. Troyon has shown that the ancient inhabitants of Switzerland had dwellings of a like construction, as was the case also, according to Herodotus, with the Pæonians of Macedonia. Most of the Australian negroes occupy the interior of islands, and certain tribes inhabit the mountains.

Brachycephalæ of Australia.

Malays,
Polynesians, *(Dieffenbach,)*
Papous, *(Guoy and Gaimard,)* } all prognathic.

The above are, in my opinion, properly called Oceanic Mongols by Dr. Latham. The Malays, recognizable by their yellow skin, their black

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* Ethnological Library, conducted by E. Norris, vol. I.

and shining hair, their projecting mandibles, pertain likewise to the peninsula of Malacca. They are so well known as the most intelligent and, after their manner, the most civilized of the islanders of the South Sea, that it would be useless in this short sketch to dwell upon them. Their skulls are scarcely wanting in any ethnographic collection.

I class as Polynesians the dusky or brownish skinned inhabitants of the Tonga islands, of New Zealand, of Otahiti, of the Sandwich islands, and of a great number of groups of less considerable islands dispersed through the Micronesian archipelago of the Pacific. The skulls of Polynesians have generally the occiput more flattened than the Malays; their jaws and teeth are less prominent; the skulls themselves larger than those of the Malays proper. The Polynesians are commonly large, well proportioned, and rather muscular, and in character and temper compare favorably with the Malays. In the royal ethnographic collection of the Carolinska Institute, there are skulls from the Sandwich islands and New Zealand which might be ranked in the first class for size, and particularly for height.

PAPOUS OF GUOY AND GAIMARD.

(Mops-Papus of Dampier.)

Dampier, Forrest, and several old travellers mention a particular people of blackish brown color, inhabiting the shores of the islands near the north coast of New Guinea, who are to be distinguished from the other islanders of the Pacific by many peculiarities, especially by a profuse head of black hair, which is so finely crisped as to present the appearance of being frizzled. Guoy and Gaimard, who accompanied De Freycinet on the corvettes *Uranie and Physicienne* had made us more exactly acquainted with this people and the particular form of their skulls. The most important point which results from their observations, as appears to me, is that their skulls are entirely different from those of the Australian negroes. While these latter, as indicated above, are quite low, narrow, of elongated oval form, and provided with a greatly projecting occipital protuberance, the skulls of these Papuans are, on the contrary, according to Guoy and Gaimard, high, short, large, and flattened on the occiput. "The heads of these Papuans," they say, "present a flattening both before and behind, accompanied with a considerable development of the jaws. The skull is of remarkable height, the parietal protuberances salient, and the temples very convex; the anterior part of the temporal regions, across which the coronal suture prolongs itself below the level of the semicircular line of the temples, presents a peculiar and very marked projection.* The nasal bones are placed almost vertically, compressed backward as it were. The nasal apophysis or frontal of the upper maxilla is large and made much more prominent than usual

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* I discover that this peculiar projection exists also in general in the heads of Malays and Polynesians.—*Author.*

in consequence of the position of the nasal bones. The upper maxilla is much larger than in the European, because of the great development of the dental process which gives to the visage of these islanders an unusual amplitude. The nasal openings are very large towards the base, even more so sometimes than in the negro. The alveolar process is unusually thick at the sides where the molars are inserted; the palatal vault rather large than long."

The museum of the Carolinska Institute possesses three samples of brachycephalic Papuans, which all strikingly resemble one another, and correspond perfectly with the above description. I shall only add that they strongly resemble the skulls of Polynesians previously mentioned, only differing from the latter in a greater depression of the bridge of the nose, the largeness of the zygomatic arches, and the amplitude both of the fossæ nasales and the alveolar arch.

Guoy and Gaimard, who describe only the Papuans of the two islands Vagiou and Ravak, report that their inhabitants call themselves *Papua*, and are distinguished by positive marks from the indigenous blacks of New Guinea, who entirely resemble the negroes of Eastern Africa; that they live on the coasts, subsisting chiefly on fish and mollusks, and build their houses on piles in the waters of the country. Those who inhabit the mountains of Vagiou call themselves Alifourous, and are mentioned as Alfours, Haraforas, &c., by different travellers. The skulls in our possession from the islands of this quarter, present the dolichocephalic negro form before spoken of, being narrow, low, and oblong, with a prominent occipital protuberance.

We find an interesting paper* in the Ethnological Library, conducted by Ed. Norris, vol. I, descriptive of the Papuans of these parts, from which we learn, on the authority of Lieutenant Bruijn Kops, of the royal marine of Holland, who accompanied an expedition in 1810, and landed on the coast of New Guinea opposite the island of Dori, that the men of the latter, whom he calls *Papuans of Dori*, are five and a quarter feet, sometimes five and a half, in height, of a dark brown color, occasionally black, having black crisped hair, often very long, though with the appearance at times of having been shaved. In a plate we see one of them represented with the hair dressed after the fashion of a turban, to which these Papuans are indebted for the name of Mops-Papus. M. Bruijn Kops states that the indigenes of New Guinea divide themselves into Papuans and Alforous, the former inhabiting the coasts, the latter the interior and the mountains, though the distinction of race between them can at present only be accounted a probability, owing to the imperfect indications afforded by M. Kops. He extols the Papuans as a people intrinsically good, not addicted to theft, holding the aged in respect, kind to their children, and faithful to their wives. Chastity is held in great honor, and but one wife is permitted, the union to whom is for life. They are, however, partial to strong drinks, nor is it discreditable among them to steal children and make an article of commerce of

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* By Mr. George Windsor Earl.

them; but the children thus taken are well treated and restored for a ransom.   The trade in slaves is general, though these are well treated. Of the manner in which crimes are punished, we have the following particulars: The incendiary becomes the slave of the injured party with all his family.   The man who intentionally wounds another is fined the price of a slave.   The thief is condemned to restore what is taken and something in addition.   All waste committed in a fruit grove or plantation must be repaired.   The sin against the sixth commandment is punished with death, or in case the injury admits of reparation, still with severe punishment.   The man who does violence to a woman is bound to espouse her and pay to her parents the ordinary value of ten slaves.   When illicit association occurs, the woman is exempt from punishment, and if not married is free from all dishonor. Everything is here estimated according to the standard value of a slave.

The majority of the Papuans of Dori are idolaters; a small number are Mahometans, with priests from the islands of Ceram and Tidore. The idol of the Pagans, called *Karwar*, is rudely sculptured in wood, about eighteen inches high, deformed, having a large head with pointed nose and wide mouth, well furnished with teeth.   Its body is usually clad in a piece of calico, and its head covered with a handkerchief. Every house has its idol, which must be present on all important occasions and is consulted as an oracle.   These Papuans have also fetiches, most frequently images of serpents and lizards, suspended from the roof or carved on the posts of the doors.   They have a kind of priests, who are also their physicians and sorcerers.   Their houses are built on posts in the lakes, with plank walls.   According to the drawing given us by M. Earl in the paper we are following, these houses resemble large shallops with openings like port-holes; the interior divisions are formed by mats, and the floor by rude planks tied together.

These Papuans work in iron and other metals, and in some sort devote themselves to agriculture, or rather, to speak more exactly, to the culture of legumes; but the training of domestic animals is unknown to them.   The chase and fishing constitute the principal occupation of the men, the women being employed in the work of the household; both in the chase and in war they use bows and arrows, but do not poison the latter.   Even the fish are taken with arrows and with lances, and sometimes also with nets.

The Papuans passing much of their time on the sea, the canoe forms an important part of their riches.   They have small canoes for children, larger ones for daily use, requiring two rowers, and others still larger, for twenty rowers.   Each of these skiffs is formed from the trunk of a tree; those of larger size having a mast and mat sail. Such frail barks being unequal to long voyages, the commerce of these coasts is in the hands of strangers, especially of the Chinese.   The government of Holland founded, in 1852, a factory at Port Humboldt, on the northern coast of New Guinea, which authorizes us to hope for more exact information respecting the inhabitants of the interior.

I have allowed myself to enter into somewhat circumstantial details

regarding these Papuans of the northern coast of New Guinea, because our knowledge of them is still involved in much obscurity. We see, meantime, that M. Kops considers them as belonging to a different race from the Alfourous. Though no great ethnographic rigor seems to have been employed in applying the names Papuans and Alfors, or Alfourous, it would seem to be generally understood that by the former are meant the indigenes of the coast, and by the latter those of the interior and the mountains. The term *Papou* seems derived from the Malay expression for crisped or woolly hair, (*rambut pua pua,*) which has come to be applied to the inhabitants of the coast, whose hair is of that description. The name of Alfourou comes from the Portuguese word *alforas*, which properly signifies an enfranchised slave. The Portuguese employed this term to designate the free indigenes of the interior of the Moluccas, wishing thereby to distinguish them from the inhabitants of the cities. As applied at present to the inhabitants of the coast and the interior, the two denominations appear, as far as can be gathered, to pertain to two distinct races.

I may be permitted to cite here an important passage from Dr. Prichard on the subject of the Alfourous of these countries. "What can we make," he says, "of the Alforic race, which has been described as a people apart, with a peculiar type and a peculiar form of the skull? It continues to be one of the most remarkable varieties of the human race. We must join with it the mountaineers of Arak, in New Guinea, seen and apparently well described by Lesson, as well as the other indigenes of the great continent of Australia."

In his instructive work before cited, Dr. Latham has admitted two varieties under the head of the *Papuan branch of the Kelonesian stock, New Guinea.* He publishes two remarkable profiles of their skulls, taken from the "Voyage of the Uranie and Physicienne," one of which has the traits of a dolichocephalic negro, while the other is brachycephalic, like those of the brachycephalic Papuans cited above. May not these figures pertain respectively, the former, or dolichocephalic skull, to an Alfourou, and the latter, or brachycephalic one, to a Papou? Yet the author attributes to the former frizzled hair and to the latter straight.

In relation to the place assignable to the brachycephalic Papous, the main object of this section, I shall conclude by expressing the opinion that it should be sought in the immediate neighborhood of the brown Polynesians, of whom these Papuans are probably the stock or the progeny, modified after some special manner by peculiar modes of life, climate, &c. Mr. Earl rejects entirely the opinion that they might be hybrids, and, as far as I can judge, with very sufficient reasons.*

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*The celebrated academician of St. Petersburg, M. C. de Baer, has recently enriched ethnological bibliography with two productions of great merit, entitled *Crania selecta ex thesauris anthropologicis Academiæ Imp. Petropolit. Petrop.*, 1859, and *Ueber Papuas und Alfuren, ein commentar zu den beiden ersten Abschnitten der Abhandlung " Crania Selecta,"* &c., 1859. The learned author of these publications expresses very positive doubts as to the fact that the skulls brought from Waigion by Guoy and Gaimard really belonged to indigenous Papuans. The skulls in question were taken from a tomb, and M. de Baer considers it

D.—AFRICA.

All the people of this continent are dolichocephalæ. This fact, to which I have heretofore had occasion to draw attention at different times, and which I do not know to have been contradicted by any one, is altogether peculiar to this portion of the world. Europe, Asia, the lands of the South Sea, America, comprise populations belonging to the two forms of head. In Europe, and still more in Asia, the brachycephalæ much exceed in point of numbers; in the isles of the South Sea the two forms are nearly balanced, I think, as to numbers, but the brachycephalæ have the moral preponderance. On the other hand, the brachycephalic populations are, to all appearance, completely unrepresented in Africa. The museum of the Carolinska Institute possesses an important collection of African skulls, of North Africans, Abyssinians, Copts, Berbers, and Guanches. All present the same form of the upper half of the skull, being large, capacious, oval, resembling much those of the Arabs. The Abyssinian skulls, which we owe to the liberality of our countryman, M. Behm, and the Copts, are slightly prognathic. The Guanches, of which we have four, all belonged to individuals of advanced age, who had lost their teeth; their alveolar processes having consequently become rudimentary, their prognathism is but slightly perceptible.

In all these skulls, whether of Abyssinians or Egyptians and Guanches, the vault of the skull is depressed in an arch elongated towards the occipital protuberance, which is a little compressed at the sides; the parietal tuberosities are little prominent. We may regard this form of skull as prevailing on the coasts and the flat country of northern Africa. It is again found on the other side of the Atlantic, in the Carib islands and in certain of the eastern parts of the American continent. The museum possesses, for the south of Africa, a considerable number of skulls pertaining to divers of the Caffre tribes. They much resemble negro skulls. Some are a little larger than a majority of these last, but the greater part have jaws and teeth horribly prominent. One among them, from the interior of the country near Port Natal, is remarkable for its diminutiveness, for the complete absence of all trace of parietal protuberance, and for an occiput nearly pointed. Our museum contains, also, the entire skeleton of a Hottentot, but neither in this nor in the figures of Hottentots and Bosjessmans left us by Blumenbach and Sandifort can I discover any important difference from the heads of negroes in gene-

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extremely probable that the remains found in this tomb had pertained to some hostile race of the Malayan branch. He maintains, on the contrary, that the Papuans are dolichocephalic, although they deviate in some respects from the indigenes of the interior (Alfourous) as regards the form of the skull. In brief, he expresses himself on the subject in the following manner: " *Cranium Alfurorum aliquorum similitudinem habet cum cranio Papuarum, nam ad dolichocephala etiam pertinet; est vero amplius et potissimum altitudine et latitudine præcedit.*" Loc. cit., p. 11.

The same observations might be made with regard to other skulls obtained under analagous conditions. With a view to decide the question, it were to be wished that skilful naturalists who hereafter visit the country of the Papuans would examine the form of the skull of living individuals.—*Note of the Author.*

ral. Many ethnologists have considered the Australian negroes as
nearly related to the Hottentots, but the skulls of the former which
have come under my observation have the parietal protuberances
more marked than the latter. These protuberances are deficient,
however, in the Dayak of Borneo, in our collection.

E.—FORMS OF THE SKULL IN AMERICA.

In an ethnological point of view there can properly be no question
here but of the savage or half savage tribes which inhabited the con-
tinent before its discovery by the Spaniards. The number of these
tribes amounts, we know, to some hundreds, of which many are
already extinct, and the rest are perishing from year to year. Pro-
found and extensive researches have been made respecting them, but
chiefly on the subject of their languages. No European savant, since
Blumenbach, has produced a craniological work so instructive as the
Crania Americana of Dr. Morton; nevertheless, the results of this
work cannot entirely satisfy us. This author, who has given us such
numerous and valuable facts, as well as the linguists who have studied
these American languages with indefatigable zeal, have arrived at the
conclusion that both race and language in the New World are unique.
I am obliged to avow that the facts adduced by Morton himself, and
the study of numerous skulls with which he has enriched the museum
of Stockholm, have conducted me to a wholly different result. I can
only explain the fact by surmising that this remarkable man has al-
lowed the views of the naturalist to be warped by his linguistic
researches. For, if the form of the skull has anything to do with the
question of races, we cannot fail to see that it is scarcely possible to
find anywhere a more distinct distribution into dolichocephalæ and
brachycephalæ than in America. It would be only necessary, in
order to show this, to direct attention to certain of the delineations
in his own work, where the skull of the Peruvian infant, (pl. 2,) the
Lenni-Lennape, (pl. 32,) the Pawnee, (pl. 38,) the Blackfoot, (pl. 40,)
&c., as clearly present the dolichocephalic form as, on the other hand,
his Natchez, (pl. 30 and 31,) and the greater part of his representa-
tions of the skulls of Chili, Peru, Mexico, Oregon, &c., are distinct
types of the brachycephalic. Conclusive, however, as the plates are,
I should scarcely have ventured to advance these remarks if the rich
series of our own collection, and the numerous and excellent figures
of Blumenbach, Sandifort, Van der Hoeven, &c., did not declare in
favor of my opinion.

From all, then, that I have been able to observe, I have arrived at
the opinion that the dolichocephalic form predominates in the Carib
islands and in the whole eastern part of the American continent, from
the extreme northern limits to Paraguay and Uruguay in the south;
while the brachycephalic prevails in the Kurile islands and on the
continent, from the latitude of the straits of Behring, in Russian Amer-
ica, Oregon, Mexico, Equador, Peru, Bolivia, Chili, the Argentine
Republic, Patagonia, to Terre del Fuego.

There can be no doubt that the Carib race was the predominant

one, not only in the lesser Antilles, but the neighboring continent
where we now find Venezuela and Guiana, and all the Carib skulls
which I have observed, or of which we have any account, are dolicho-
cephalic.   With regard to the Indians of Brazil there is a general
concurrence in assigning them to the Tupi of the Portuguese, who,
more to the south, received from the Spaniards the name of Guarani,
of whom Dr. Prichard has somewhere said: "This great race, Tupi
or Guarani, is spread over the whole eastern coast of South America,
from the mouth of the river De la Plata to that of the river Amazon."
Towards Upper Paraguay it extended over almost the whole central
part of the continent, and in the province of Dhaco it reached the
eastern slope of the Andes, and even penetrated the valleys of that
great chain.   Mention is also made of the Guarani in Bolivia, New
Grenada, and other countries.   Apparently the *ancient Peruvians* of
Morton and the *Huanchas* of M. de Tschudi are also Guarani, though
their skulls were much deformed by the elongation produced by arti-
ficial compression.   The skulls of this race, as well as the Carib, are
dolichocephalic, and of much capacity, with the jaws quite large.

Towards the north we find on the Atlantic coast, both of the United
States and Canada, a predominance of the dolichocephalic form among
the tribes, that is to say, who pass under the general name of red-
skins, as the Algonquins and Iroquois.   The same result may be
definitively arrived at from a study of the delineations given by Mor-
ton of Cherokees, Chippeways, Miamies, Oneidas, Hurons, Pottawato-
mies, Cayugas, (particularly remarkable,) Cotonays or Blackfeet, &c.

To these facts it must be added that the Esquimaux, who extend
also to the eastern coast, belong equally to the dolichocephalæ, though
holding an altogether special place among them.   Many authors con-
sider the Esquimaux as related to the Tschjoudes, as well as to the
Mongols. Morton himself, in the ethnographic part of his work, classes
them in a common family with the Samoiedes and the Laplanders,
and gives it the name of the *polar family;* stating that this singular
race is found only on the northern limits of the continents of Europe,
Asia, and America. He calls them *Mongol-Americans.* Nothing could
be more inexact than this assertion, as far at least as the form of the
skull is admitted to have weight in the question of the affinities of
race.   In my first essay on this subject, laid before the Assembly of
Scandinavian Naturalists in 1842, I placed the Greenlanders among
the prognathic dolichocephalæ, and had the pleasure of finding myself
fully sustained in this view by such competent judges as Eschricht,
Van der Hoeven, Ibbsen, and Nilsson.   Messrs. Eschricht and Ibbsen
have probably seen more skulls of Greenlanders than any other phy-
siologists of our age, and the former, in a discourse before the Associ-
ation at Christiana, in 1844, delivered himself to this effect : "The
Greenlanders and Esquimaux pertain to a people among whom the
form of the head is of an altogether special type, and I rest my deci-
sion on the skulls of Greenlanders in the physiological museum of
Copenhagan."   Now these skulls, which he exhibited, have exactly
the form of those on which I based my own opinion.   I find myself
sustained also by the figures and descriptions of Esquimaux skulls

given by Blumenbach and Sandifort, and even by those of Morton himself. It is evident that this latter accomplished naturalist has allowed himself to be more guided by opinions already formed than by a scrupulous examination of facts. He saw that the form of the Esquimaux visage has something of the Mongol, and paid no attention to the salient occipital protuberance and other characters so little like the Mongol. I have already adverted to the great resemblance in form which exists between the Esquimaux skull and that of the Tongousian which we have at the Carolinska Institute, and to the description which Blumenbach has given of a Tongousian skull, which coincides entirely with the characters of the Esquimaux. In the large collection of Chinese skulls in our Institute I trace a striking resemblance in form with those of both the Tongousians and the Greenlanders. The inference would be that the Esquimaux is a *polar* race only in America; that it is thinly scattered over the islands of the polar sea, the most northern regions of America, and thence, passing from east to west, through Asia towards China, where we might identify it with the pure Chinese part of the population, but little distinguishable in appearance from the Tartaro-Chinese portion.

With regard to the other primitive dolichocephalæ of America, I entertain an hypothesis still more bold perhaps, namely, that they are nearly related to the Guanches in the Canary islands, and to the Atlantic populations of Africa, the Moors, Tuaricks, Copts, &c., which Latham comprises under the name of Egyptian-Atlantidæ.

This is not the first time that, in speaking of our collection of skulls, I have called attention to the resemblance of those of Guanches and Copts on the one side, and the Guaranis of Brazil on the other. Above I have shown that the latter are related to the race of the ancient Caribs of the Antilles. We find, then, one and the same form of skull in the Canary islands, in front of the African coast, and in the Carib islands on the opposite coast which faces Africa. The color of the skin on both sides of the Atlantic is represented in all these populations as being of a reddish brown, resembling somewhat leather tanned brown; the hair the same; the features of the face and build of the frame, as I am led to believe, presenting the same analogy.

These facts involuntarily recall the tradition which Plato tells us in his Tinæus was communicated to Solon by an Egyptian priest, respecting the ancient Atlantis, situated in the ocean in front of North Africa, and afterwards engulfed through some great change in the distribution of land and water. Though embracing many particulars of pure invention, would it be unreasonable to claim that, coming as it does from a quarter to which common consent refers the origin of our sciences and arts, this tradition deserves attention in connexion with facts which seem to point in the same direction?*   A Swedish

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* We leave to philologists the task of showing how little probable it is that the Indians, whose languages are polysynthetic, should be related to the Semites, whose linguistic system is based on dissyllabic roots. But with regard to the tradition of the Atlantis, we must remark that the submersion of a large continent situated so near the tropics, would have had the effect of a considerable refrigeration of the northern hemisphere, pro-

geometer, M. Helleberg, who resided long in Ohio, learnedly defends the opinion maintained by many others, that the Indians of North America are descended from the tribes of Israel, alleging that their features are essentially Jewish, that McKenzie saw the Chippeways practice circumcision, &c. Without meaning to adopt this opinion as my own, I yet refer to it as bearing favorably on the hypothesis advanced above respecting the primitive kindredship of the Carib and Guaranic races on one side of the Atlantic, and the Guanches on the other, the latter being in turn nearly allied to the races of North Africa, whose resemblance to the Jews, as regards the face and form of the skull, is very close, and who present a complete contrast to the Mongol type on the Asiatic side. Morton has remarked that "the primitive Egyptians, the Misraimites of Scripture and descendants of Hàm, were directly affiliated with the nations of the Lybian family, and in their physical traits were intermediate between the Indo-European and Semitic races." In view of the developments of modern geology respecting the rise and subsidence of vast tracts of land, there would seem to be nothing absurd in admitting that America was once united with Africa or Asia, and obscure traditions to this effect are said still to exist among the American Indians.

The brachycephalic tribes of America are found for the most part on that side of the continent which looks towards Asia and the islands of the Pacific, and they seem to be related to the Mongol races. A. de Humboldt, the first of modern naturalists, has expressed himself in favor of this view, which new proofs appear every day to corroborate. Some of these American brachycephalics possessed a high degree of culture at the period of the Spanish conquest, and the influence which this civilization exerted over most of the inhabitants of the continent has disposed many eminent ethnologists to infer the unity of the whole American race. Thus Dr. Latham has been led to comprise them all under the significant appellation of *American Mongolidæ*, an extension which ethnological craniology will by no means countenance. Morton, too, as has already been said, maintained a unity of race, with the exception of the Esquimaux, esteeming the brachycephalic type as predominant among all the tribes, and furnishing positive proofs of its existence at least on the western coasts. For my own part, I have long been convinced of the consanguinity between the brachycephalæ of America and those of Asia and the Pacific islands, and that this characteristic type may be traced uninterruptedly through the long chain of tribes inhabiting the west coast of the American continent from Behring's Straits to Cape Horn. In my work on the skulls of the Indians of the Pampas, (*Om Pampas Indianernas Cranier*, 1855,) I announced the opinion which I now reaffirm, respecting the distribution of the Indian tribes into dolichocephalæ and brachycephalæ, as well as the relationship of the

ducing a complete revolution in vegetation, especially in the basin of the Mediterranean, and was, therefore, much more likely to have engraved itself on the memory of the Egyptians than any other circumstance of the supposed catastrophe. Yet we hear of no coincidence of such a revolution of climate with the disappearance of the Atlantis.—*Note by M. E. Claparede, (French translator.)*

former to the Guanches and other Atlantic populations, and of the latter to the Mongols. A strong confirmation of this last position may be found in the learned researches of M. Daa, respecting the linguistic affinities of the people in question.

The observations of intelligent travellers in all the countries bordering on the Pacific leads us to the same definite conclusion with regard to the predominant type in all of them. Thus, for Russian America, we have the testimony of M. H. J. Hohnberg, who has long resided in that distant country, and whose useful researches have been given to the world in a separate series of the *Actes de la Société Findlandaise des Sciences*, Helsingfors, 1855. The skulls of Oregon were familiar to Morton, to whose liberality we ourselves owe the possession of two, as we are indebted for another to Professor Meigs, of Philadelphia. I have shown elsewhere that they belong to the brachycephalic Mongol type, and afford the better indications from not having undergone the artificial vertical compression in use among the people of those regions. The Aztecs are represented in our museum by three skulls found in an ancient cemetery near Mexico, which was uncovered in digging intrenchments to protect the Mexican capital against the armies of the United States. They are remarkable for the shortness of their axis, their large flattened occiput, obliquely truncated behind, the height of the semicircular line of the temples, the shortness and trapezoidal form of the parietal plane. They present an elevation or ridge along the sagittal suture; the base of the skull is very short, the face slightly prognathic, as among the Mongol-Kalmucs. They bear a strong analogy to the skulls of Peruvian brachycephalæ delineated by Morton.

"Every one," says this last-mentioned savant, "who has studied this subject attentively, knows that the skull of Peruvians presents a flattened and almost vertical occiput. It is, besides, characterized by an elevated sinciput, great inter-parietal breadth, considerable weight of bone, prominent nose, with the maxillary region large and prognathic. It is the type of the skull among all the tribes from Cape Horn to Canada, in a degree more or less marked." There can be no doubt that the skulls of the Araucanians of Chili are brachycephalic, and present a striking resemblance in form to those of the Peruvians and Mexicans. The same type is again recognized in the Pampas of the republic of Buenos Ayres, and throughout Patagonia, to the limits of Terra del Fuego. With the skulls of the Indians of Terra del Fuego I am only acquainted through the excellent portraits in profile taken during the voyage of Captain Fitzroy, (Narrative of the Surveying Voyage, &c., 1839.) These show that the inhabitants of this country are even more distinctly brachycephalic than the Indians of the Pampas.

To the tribes which we have thus cursorily reviewed, in proceeding from the north towards the south, we are disposed, after Dr. Latham, to apply the name of American Mongolidæ. Our review has chiefly been confined to the coasts, but they have also penetrated very far into the interior, in the direction of the east. Thus, on the authority of the great work of Morton, *Crania Americana*, we encoun-

ter them on the banks of the Lower Mississippi, (the Natchez,) in Louisiana, (the Chetimachees,) in Georgia, Alabama, and Florida, (the Muskogees or Creeks, and the Uchees or Seminoles of Florida,) in Wisconsin, (the Menomonees and Ottigamies,) in Arkansas, (the Osages.)   Morton has, besides, described and drawn skulls of a like form from tombs in the States of Virginia, Ohio, and Tennessee. Two skulls of the Mongol type of the United States were presented by him to the Carolinska Institute, the one a Sac Indian of Missouri, the other a Menomonee of Michigan.   As regards the dolichocephalic family, I have previously traced its progress as far as Peru; but since the occupation of America by Europeans, no considerable change in their place of residence has occurred in the case of any of the tribes.

It would not seem out of place, before terminating this rapid estimate of the influence which the study of the human skull has exercised on the development of ethnology, to say something respecting the *artificial deformation of the skull*.   This pagan custom, which had been mentioned by different writers, Oriental, Greek, or Roman, was long totally forgotten by the civilized world till it was discovered, as an unheard-of wonder, to be the usage among several Indian tribes in America. Blumenbach, in describing a Carib skull from St. Vincents, notices that the possibility of such artificial deformation had been denied by Sabatier, Camper, and Artaud, but himself completely refutes their opinion.   Even after this, for a long interval, the subject ceased to attract attention, till Pentland brought from Peru the singular skulls described by Tiedmann, (Zeitschrift für Physiologie, Band V, p. 107,) moulds of which in plaster are to be found in many collections, public and private.   Many other heads, artificially deformed in different ways, were subsequently procured from the same part of the world, and at last the publication of the *Crania Americana* of Morton placed before us a complete history of this custom and of the manner in which these deformations are produced by the Indians of different tribes.   The accounts thus received from America had the effect of causing this absurd and pagan custom to be generally regarded as of essentially American origin.   Still the real existence of these artificial deformations continued to be called in doubt; the celebrated anatomist, Tiedmann, himself declared for the natural origin of these strange forms, and the Swiss traveller and naturalist, Tschudi, shared his opinion.   In 1849 appeared a remarkable memoir by M. Rathke, showing that similar skulls had been found near Kertsch, in the Crimea, and calling attention to the book of Hippocrates *De Aere, Aquis et Locis, Lib. IV*, and a passage of Strabo, which speak of the practice of modifying the shape of the head by means of bandages as being in use among the macrocephalic Scythians.   Many similar skulls from the country of Kertsch have since been described by Dr. Carl Meyer.

In 1854 Dr. Fitzinger published a learned memoir on the skulls of the Avars, a branch of the Uralian race of Turks.   He pointed out that the practice of compressing the skull had been signalized by ancient authors as existing in several parts of the Empire of the East, and at the same time described an ancient skull greatly distorted by

artificial means which had been found at a more recent epoch in Lower Austria.   In 1854 I received from M. Troyon, of Switzerland, two ancient skulls of like shape derived from Switzerland and Savoy, respecting which I made a report to the Academy of Stockholm in 1854.   From the account given by Amedée Thierry, in his History of Attila, I had learned that the custom of artificially deforming the head proceeded of old from the Mongols, from whom it was borrowed by the Huns, and that it was employed as the means of conferring a certain aristocratic distinction, just as Hippocrates reports it to have been practiced for the same purpose by the Scythians, and as is still the case among the Indians of Oregon.   At the same time I was enabled to show that this custom still exists in France, where it has without doubt been perpetuated since the time when the Huns were masters of the country.   This custom, still existing in certain parts of France, has been mentioned and described by Dr. Foville in his work on the anatomy of the head, though the author seems scarcely to have perceived the historical significance of the fact.   Shortly after the date last referred to I received from Professor Geffroy, of Marseilles, a confirmation of the fact that this custom is still persisted in in the south of France, not far from Marseilles.   From a passage in the works of Vesalius we are led to infer that it exists also in several parts of Turkey.

We can no longer doubt, then, that this practice of giving an artificial form to the skull has subsisted from a remote epoch among the Oriental nations.   As Thierry, moreover, pronounces it to be a Mongol usage, I have submitted the question, in the memoir before spoken of, whether this fact does not speak in favor of an ancient communication between the Old and the New World?   Such a communication seems, indeed, to be now placed beyond doubt by the proofs which have been accumulated, from time to time, through the efforts of numerous and zealous inquirers.   It would seem likely that the usage in question has been introduced by the Mongols into America, where it has become diffused even among tribes not of the Mongol stock. Among the greater part of these the compression seems to have been effected on the occiput with the view of rendering this flat and short. The compression of the top of the head among the Indians of Oregon (Flat-heads) has no doubt sprung from their proximity to the Esquimaux, whose heads are full and large.   The frontal compression (Huanchas, Caribs) seems to have been designed to render the head more dolichocephalic, and was exclusively practiced by dolichocephalæ, for whom I propose, in analogy with the term used by Dr. Latham, as mentioned above, the name of *American Semites.*

MEMOIR OF PYRAMUS DE CANDOLLE.

By M. Flourens, Perpetual Secretary of the French Academy of Sciences, 1842.

[Translated for the Smithsonian Institution by C. A. Alexander.]

The Academy has lost, within a few years, three members whose labors have profoundly influenced the progress of the natural sciences: Georges Cuvier, to whom we owe the widest application of those sciences of which probably the genius of man is capable ; Laurent de Jussieu, who seems by his method to have given them a language for ideas, as Linnæus by his nomenclature had given them one for things; and it has but just lost M. de Candolle, who opened with a brilliant theory the long series of happy conceptions and daring aims of the nineteenth century.

Augustin-Pyramus de Candolle was born at Geneva the 4th of February, 1778, a month after the death of Linnæus, two months after the death of Haller, and three after that of Bernard de Jussieu—a circumstance which we may be permitted to recall, as he would almost appear to have imposed on himself the task of replacing those three great men in the service of botany.

He was descended through his father from one of the most ancient families of the nobility of Provence—a member of which, having embraced the reformed religion, had taken refuge in Geneva in 1590. This ancestor, as well as the father of our subject, had reached stations of much eminence in the service of their new country, the latter having attained, at a very early age, the post of first syndic, which is the highest of the republic. His mother was grand-niece of the celebrated Genevese, Le Fort, who was, at one and the same time, grand admiral, general-in-chief, and first minister of Peter the Great.

The infancy of De Candolle reminds us in some respects of that of Cuvier; in both cases there was an intellectual and tender mother; in both an infant of delicate health and the most happy disposition.

Debarred by bodily weakness from the usual sports of childhood, the young De Candolle formed a decided taste for the pleasures which attend the development of the understanding. From the age of six to seven years he exercised himself in the composition of comedies. At this period, Florian, who was a friend of the family, came to spend a winter in Geneva. "You see this gentleman," said Madame de Candolle, one day, to her son, "he is the author of many charming theatrical pieces." "Ah," replied the child, with the tone of one of the fraternity, "you write comedies; well, so do I." A serious malady placed his life for some time in jeopardy, and the studies of college were necessarily pursued with reserve, but literature, and especially poetry, lost nothing thereby. What he wrote was, for the most part, in verse, and masters and scholars stood always between the chances of an epistle or an epigram, according to the humor of the moment. Nothing as yet presaged the future savant or botanist,

but it was impossible to mistake the indications of an ingenuous char-
acter and of elegant tastes, which needed but suitable circumstances
for their development.

The course of these peaceable studies was, however, soon to be
interrupted. In 1792 a French army occupied Savoy and approached
the gates of Geneva. The women and children were sent to seek an
asylum in the interior of Switzerland. In vain did the young De
Candolle entreat to be allowed to remain with his father and partake
in the defence of his country. His years were judged too immature,
and he was obliged to withdraw with his mother and a young brother.
A village at the foot of the Jura, near Lake Neufchatel, was the place
of their refuge.

Here the charms of nature first touched and captivated him.
Flowers were at first gathered only to copy them; but he soon en-
gaged with ardor in forming a collection, and undertook long and
adventurous excursions for the discovery of new plants. Already
this future rival of the law-givers of botany, though he knew only
the vulgar names of plants, felt himself tormented by the necessity
of classifying them, and as he was without books, he classed them
according to their natural relations, as the mind is always prompted
to do when not spoiled by false systems.

Some years after this period, a French mineralogist, distinguished
by his useful labors, and since still more celebrated for his illustrious
misfortunes, traversed the mountains of Switzerland. Dolomieu saw
the young De Candolle and was struck with his ardor for study. His
patronage was offered and accepted; and our Genevese, already secure
of his own powers by the trial which he had made of them in soli-
tude, came to seek in Paris at once masters and rivals. Here, from
the first, all the higher courses of instruction received his attention;
but, irresistibly attracted by botany, he gave a decided preference
to the garden of plants.

On quitting Geneva he had promised his father to devote himself
also to the study of medicine. He tried to do so, but in vain. The
sight of the sick plunged him into profound sadness. He could not
bear the idea, which is, in truth, a formidable one, of taking upon
himself the responsibility of their sufferings. His was a daring intel-
lect, but a sensitive heart, and he longed for pursuits in which he
might err without dread. Thus when, in after times, he happened to
fall into some error with respect to the name or classification of a
plant, he would say with a sort of satisfaction, "thanks to heaven, it
is only a plant which is wrongly named."

Having renounced medicine, he thenceforward scarcely left the
garden. Day after day he might be seen engaged, from morning till
night, in observing or describing plants. All respected a youth whom
nothing as yet distinguished but industry, while the gardeners, seeing
him pass whole days on the same modest bench, came to designate
him from its situation as the young man *a l'arrosoir*.

Such perseverance did not escape M. Desfontaines, who, one day,
approaching him, said: "M. Redouté has made a collection of draw-
ings of succulent plants, and wants a botanist to describe them;

would you charge yourself with that labor?'' Startled at this unex-
pected proposition, the youth would have excused himself, as well on
account of the difficulty of the subject as his own defect of knowledge.
"You will see," said the good Desfontaines, "that it is not as difficult
as you imagine; come and work at my house; I will direct you."

The reputation of De Candolle commenced at twenty years of age
with the *Histoire des Plantes Grasses.* But shortly a labor of a higher
and more original character designated more clearly the rank which
he was to take in science. He had fortunately conceived the idea
of occupying himself with the sleep of plants.

The first step was to assure himself that air goes for nothing in
this phenomenon; for plants which sleep, when immersed in water,
pass as usual from sleep to waking and from waking to sleep. Setting
aside, then, the action of air, there remained that of light. The plants,
therefore, were placed in darkness, and alternately exposed to its in-
fluence and to that of light. By illuminating these plants during the
night, and leaving them in obscurity during the day, De Candolle suc-
ceeded in completely changing the hours of their waking and sleeping;
he saw the nocturnal plants open in the morning, and the diurnal in
the evening.

These curious experiments, being communicated to the Academy,
excited the most lively interest; and, indeed, the results obtained by
the author may be said, without exaggeration, to have possessed
something of the marvellous, even for the vulgar. By the aid of arti-
ficial light alone he had colored etiolated plants green;* had changed
the hours of sleep and waking; and had proved the remarkable fact
that plants have habits; for it is not all at once, but only at the end
of a certain time, that they discard their ordinary hours and adopt
others. Thus the life of plants is shown to be a more complicated
phenomenon, and one much more nearly approaching that of animals
than had been yet suspected. They have their activity and repose,
their sleep and waking, and likewise their habits; so that when De-
lille, celebrating these results in verse, proceeds to say,

The calyx of the credulous flower is duped ;

we feel that poetry, in view of such facts, has almost lost the privi-
lege of being metaphorical.

Fontenelle has remarked "that botany is no sedentary and slothful
science which may be acquired in the repose and shade of the closet;
it demands that its votary should traverse mountains and forests,
climb the acclivities of rocks, and expose himself on the edge of
precipices." Applied in the first instance to Tournefort, these ex-
pressions would have been doubtless thought by their author equally
applicable to De Candolle. The *Flore Française,* the flora of that
vast empire which was every day extending its frontiers by victory,
furnished him with the occasion for many fatiguing expeditions. His

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* It is necessary to remark, however, that the coloration was imperfect—the plants, as
he himself said, remaining intermediary between etiolated and green ; nor had the arti-
ficial light, which he employed, sufficient intensity to develop oxygen gas. M. Humboldt
had already observed the same phenomenon : *Comptes rendus de l'Academie,* Tome XV, p.
1194 —*Author.*

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exploration of the higher Alps would alone prove that the enthusiasm
of science has an intrepidity which yields to no other.   On one occa-
sion, wishing to reach the Great Saint-Bernard by the almost im-
practicable col Saint-Remi, he found himself, after climbing the col,
obliged to descend a frozen declivity, excessively steep, and termi-
nating in a precipice. The guides were before, marking the steps with
their iron-shod staves, while our traveller followed in silence. All at
once his footing fails him, and sliding with frightful rapidity, he hears
cries of distress from his guides, who can afford him no succor. At
last he perceives a slight fissure in the ice, and, thrusting his staff
forcibly into it, is stopped. To cries of distress succeed those of joy;
the most intrepid of his guides comes to him by a long circuit, and,
tracing a path in the snow, conducts him to a place of safety. "Ah,"
said this brave man, embracing him, "no one has ever caused me so
much anxiety."

On occasion of the inquiries, before mentioned, respecting the
habits of vegetables, De Candolle, though but twenty-two years of
age, had been inscribed by the Academy on the list of its candidates.
Adanson said of him, that "he had established himself on the high-
way of science."   Cuvier had chosen him for his substitute in the
chair of Natural History at the college of France, and Lamarck con-
fided to him the second edition of his *Flore Française*.   This edition
became in the hands of De Candolle an original work, which may well
serve for a model in extended labors of its kind. He had but just
executed it when a vacancy occurred in the Academy by the death of
Adanson.   Besides the works already mentioned, De Candolle had
published an important one on the *Astragali;* an essay full of interest
on the *medical properties of plants;* researches, equally new and in-
structive, on the *pores of leaves*, on the *vegetation of the misletoe*, &c.;
and, resting on such titles, might well aspire to the nomination. But
it was carried in favor of Palissot de Beauvais by two or three voices,
to the sensible chagrin of De Candolle.   He had been for some time
pressed by the faculty of medicine of Montpellier to accept the chair
of botany, which had been successively occupied by Gouan and Brous-
sonnet, and though hesitating till now, he hesitated no longer. He
accepted the chair, and resolved to quit Paris.

Was this well or ill done? To consider the motive only of his
resolution, the hasty counsel of a wounded susceptibility, assuredly not
well; but if we consider the important results which accrued to botany
from his sojourn at Montpellier, perhaps the answer will be altogether
different.   Would Paris have left him the same leisure for protracted
labors? The same calm for abstract meditations? The same liberty of
ideas? The same originality of views? And to say all in a single word,
would De Candolle have been as completely himself as he has been?

At the moment of his departure from Paris an embarrassment arose
which threatened wholly to disconcert his purpose. On concluding
the *Flore Française*, he had devoted himself to a not less important
work on the botanical geography of France, *Geographie Botanique de
la France*, and with so much ardor that rather than abandon it, and
thus lose the modest salary which scarcely defrayed the expense of

his excursions, he would have promptly renounced the professorship. Fortunately for Montpellier, M. Cretet, minister of the interior, when consulted on the difficulty, replied, "Let M. De Candolle choose: he shall either have both the places, or neither one nor the other." And a few days after the same dignitary gave even more distinct expression to his high estimate of De Candolle, though still couched under the brisk form of a sally. M. Laplace having called on him in company with De Candolle, and wishing to give in some way expression to the high esteem he entertained for the latter, had said to the minister, "Your excellency does us an ill turn; we had hoped to have soon had M. De Candolle at the Institute." "Ah, your Institute! your Institute!" exclaimed M. Cretet. And while Laplace looked at him with surprise, he added, "Do you know that I sometimes feel inclined to order a battery of guns to be pointed against your Institute? Yes, a battery, in order to disperse the members throughout France. Is it not deplorable to see all the luminaries congregated in Paris, and the provinces in ignorance? I send M. de Candolle to Montpellier to carry thither the spirit of activity."

In effect, the influence and efforts of De Candolle soon infused life into all the studies of Montpellier. The spirit of Linnæus reigned there almost exclusively; and unfortunately, by the spirit of Linnæus must here be understood the spirit of artificial methods.* All the labors of the last half of the eighteenth century, all the new philosophy of science, all the grand ideas elaborated by the Adansons, the Jussieus, the Cuviers, had remained unknown or disregarded. Hence the lessons of De Candolle had all the freshness of novelty for this isolated province; those admirable lessons, which afterwards reproduced in three great works, have afforded valuable instruction to all Europe.

These three works are the *Theorie Elementaire de la Botanique*, the *Organographie*, and the *Physiologie Vegetale*. Of these, the first, published in 1813, is the most important, for it was in this that the author laid the first foundations of his general theory of the organization of beings.

Every age seems to impose on itself the solution of some new problem; thus, as regards the phenomena of life, the eighteenth century was chiefly engaged with the problem of methods and the problem of the revolutions of the globe. The question of methods, so ably discussed in the seventeenth century by Tournefort and Ray, in the eighteenth by Linnæus, Adanson, and Bernard de Jussieu, finds its solution, towards the end of the latter century, through the labors of Laurent de Jussieu and Georges Cuvier. The question of the revolutions of the globe commences in 1575, with some speculations of the potter, Bernard Palissy; two centuries afterwards Buffon

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* They had been introduced there by Sauvages, and directly applied by him to the regular classification of maladies by classes, genera and species   See his *Nosologia Methodica*, a remarkable work for the time at which it appeared   In justice to Linnæus, it should be observed that no one better understood than he the different parts assigned to the natural and the artificial methods, nor better marked the characters which distinguished them.— *Author.*

conceives the grand idea of the ages of the world, and produces his
Epoques de la Nature; at length Georges Cuvier gives to the world his
Recherches sur les Ossemens Fossiles, and the question of the revolutions
of the globe cannot long remain without its solution.

The problem which the nineteenth century seems to have proposed
to itself in the same province is the determination of the intimate
laws of the organization of beings; and on this occasion the light has
proceeded from a source from which we could scarcely have expected
it.   In 1790 a small work was published in Germany, entitled the
Metamorphosis of Plants.   The author, who seemed to unite the
genius of the two neighboring nations in the flexibility of his powers
and the extent of his inquiries, was the first to see in the transformation
of one part into another all the secret mechanism of the development
of the plant.   Thus a first transformation changes the leaf into the
calyx; a second, the calyx into the corolla; a third, the corolla into
organs of a still more delicate texture.   All these organs are there‧
fore but the modifications of one organ, all the parts of the flower are
but modifications of the leaf; transformation is the predominating
principle, and the generalized expression of this striking fact con‧
stitutes the celebrated theory of Göethe.

For the theory of De Candolle even a greater degree of elevation
may be claimed.   According to this, each class of beings is submitted
to a general plan, and this general plan is always symmetrical.   All
organized beings, regarded in their intimate nature, are symmetrical.
But this primitive symmetry, on which all reposes, and from which all
emanates, what is it? how define it? how even determine it?[*] for
symmetry, the primitive fact, is rarely the fact which subsists.   The
abortion, the adhesion, the degenerescence of the parts almost always
alters or masks it; and to rediscover this symmetry, which is the
primitive fact, we must ascend through all the subsequent irregulari-
ties, which are but secondary facts.   And yet these views of De Can-
dolle, bold and striking as they are, may be already announced as, in
more than one instance, a demonstrated truth.   To show this, an

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[*] Doubtless the idea of a primitive symmetry subsequently altered is still in many cases
but a supposition, yet in many others it is the fact itself; in the *Marronier d'Inde* (horse‧
chesnut) the primitive symmetry is changed under the eyes of the observer; in a multitude
of species the primitive symmetry, masked by the ordinary irregularities, disengages itself
momentarily from those irregularities, and all at once reappears.   De Candolle was the
first who made what are termed *monstrosities* in the vegetable kingdom enter into a general
theory; he defines them as "returns to symmetry."

"It is by the observation of certain monstrosities," he says, "that we have been enabled
to detect the true nature of certain abortive organs, and consequently the true symmetry
of the plants.   Thus the observation of the *Peloria* has proved that a certain filament which
is found on the inner base of the corolla of the *antirrhinum linaria* and some others, is an
abortive stamen, since we have seen it change into a stamen "—*Theor. Elem. de la Bot.,* p. 98.

The causes which produce anomalies, *constant and predisposed,* are subject to laws so fixed
and regular that De Candolle finds in those anomalies the very source of genera and species.
"The arrangement of plants in natural orders supposes," he says, "that we may one day
establish the characters of those orders on that which constitutes the ground of their sym-
metry, and refer the varied forms of species and genera to the action of causes which tend
to alter the primitive symmetry,   Thus each family of plants may be represented by a
regular condition, sometimes visible to the eye, sometimes conceivable by the intellect; this
is what I call its *type*: adhesions, abortions, degenerescences or multiplications, separate or
combined together, modify this primitive type so as to give rise to the habitual characters
of the objects which compose them."—*Organogr. Vegetale,* II, p. 240.—*Author.*

example or two will suffice. If we take the fruit of the common horse-chesnut, we shall find but three seeds at most, sometimes but a single one; but on recurring to the flower we shall see three cells, and two seeds in each cell, that is to say, six seeds. The fruit of the oak, the acorn, exhibits in all cases but one seed; and here we see the primitive type *altered*. But in the flower of the same tree the ovary has always six ovules, and here we have the primitive type *re-discovered*.

The theory of De Candolle reveals a new world to the observer. In a group of plants whose corolla is *polypetalous*, should an ordinary naturalist find one whose corolla is *monopetalous*, he would probably rest satisfied with having verified the fact; but with the naturalist inspired by theory, inquiry commences where it terminates with the other. Such an one sees, in the species which he is comparing, the consolidated corolla occupy the place of the corolla with several petals; he finds the ribs (*nervures*) of the former correspond with the divisions of the latter; he reverts to an earlier stage of the flower, and seeking this consolidated corolla in the bud, he there finds it composed of several pieces; and thus the profound analogy of the group, masked by the soldering of the petals in one species, reappears in all its entirety.

What De Candolle calls *degenerescence* constitutes, when taken in an inverse order, the *metamorphosis* of Göethe. The latter, following an ascending scale, sees the leaf *metamorphosed* into the calyx, the calyx into the corolla, the petals into stamens, the stamens into pistils, ovary, and fruit. De Candolle, pursuing the opposite course, sees fruit, ovary, and pistil *degenerate* into the stamen, the stamen into the petal, the corolla into the calyx, the divers parts of the calyx into leaves. Thus our double flowers are for the most part but the result of the transformation of the stamens into petals, as in that most beautiful of all transformations which changes the simple flower of the eglantine into the many-leaved rose of our gardens. While metamorphosis, taken in the sense of Göethe, evolves, so to say, from the leaf all the parts of the flower, degenerescence, in the sense of De Candolle, brings all the parts of the flower back to the leaf. One of these facts proves the other; and the theory of Göethe, under a proper point of view, is but a part, though an admirable part, of the theory of De Candolle.

It was long ago said, and with reason, that books also have their destiny. When Göethe, towards the close of the last century, gave his doctrine to the public, the poet damaged the botanist; the fame of the author of Werther and Faust caused the more modest merit of the author of the Metamorphosis of Plants to be overlooked. When De Candolle published his theory in 1813, he was far from Paris, in an obscure province, and his book succeeded but slowly, almost imperceptibly, in attracting general attention. It was, in fact, nearly twenty years later, and only when a dispute between two eminent rivals had carried the discussion into the halls of this Academy, that public opinion learned at last to comprehend the force and weight of the new ideas.

Yet why not confess it? Without doubt, the new spirit of the

sciences, whatever praise may be due to its boldness, has not always known how to restrain its flight or master its audacity. Even in De Candolle, whose judgment is so firm and logic so sound, there is more than one generalization which surprises, more than one consequence which it appears difficult to admit. We cannot well explain to ourselves how it is that the *primitive symmetry*, that mysterious key to the whole system, is so rarely the dominant fact, while the habitual fact is, on the contrary, almost always the anomaly.* But, on the other hand, who can fail to recognize the grandeur of so many daring and profound conceptions? Who but must wonder at so many results obtained by new methods, so many truths which it was necessary to surprise, as it were, by yet unattempted methods of approach? Who but must be struck at the number of ancient difficulties resolved, and, what is more remarkable, the number of new difficulties which as yet had no existence for science—which science had as yet not sufficient insight to suspect?

The *Theorie Elementaire de Botanique* had appeared in 1813, quickly followed by the disastrous events of 1814, when France, after unparalleled successes, began to experience reverses equally without a parallel. During the Hundred Days, De Candolle was appointed rector of the Academy of Montpellier. During the administrative anarchy which followed the second Restoration, the local authorities of Montpellier, without consulting the higher authorities, or rather, as regarded De Candolle, in contrariety to the express orders of those

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*As an elucidation of some portions of the text relating to vegetable morphology, the following passage from an able article in the Foreign Quarterly Review, April, 1833, may not be unacceptable, at least to the general reader :

"A marked law of symmetry regulates the conditions under which the vegetable structure is presented to us, in such plants as are closely allied in natural affinity, however much they may differ in certain individual peculiarities, those peculiarities always depending upon some modification in the mode of development in certain organs, or upon the partial or entire suppression of them in one and not in the other species. Repeated examples have shown us that certain organs may sometimes be accidentally developed in plants in which they are generally absent, or else may disappear in some individuals of a species where they are usually present. It is by the study of these peculiar ' monstrosities' that we are enabled to ascertain the actual existence of particular organs in a latent or undeveloped state; and it has been by connecting the results of such inquiries that the whole theory of the natural classification of plants has of late years undergone a complete revolution. The chief phenomena which regulate the conditions essential to the extension of this kind of knowledge are the abortion, degeneration, metamorphosis, and adhesion of certain parts. The account of these belongs more especially to the organographical department of botany, and very little is known to the physiologist of the causes which produce them. The non-development or abortion of any latent organ in a plant seems to arise very frequently from its compression by some contiguous part, or else from an abstraction of its nutriment by another part which exerts a greater vital activity. As these effects depend upon the relative position of such parts, the influencing cause begins to operate even from their nascent state, and long before their form is discernible by us. We have consequently no control over these causes, and their influence could never have been noticed by us if nature herself had not assisted in the discovery by producing those occasional aberrations from the ordinary state of plants which are known by the name of ' monstrosities.' That all the various parts of the fructification are modifications only of the leaf, is demonstrable by an appeal to numerous examples of monstrosities in which these parts may be seen to possess an intermediate character. But we are still utterly ignorant of the nature of those predisposing causes which are capable of effecting such wonderful modifications in the form, color, consistency, and nervation of this single organ, and, above all, such a complete dissimilarity between its various functions."—*Note by Translator.*

authorities, decreed that all the functionaries of the Hundred Days should be deprived, and De Candolle was deposed from the rectorate.

What was the rectorate to De Candolle? He was still professor of the faculty of medicine and dean of the faculty of sciences; he was more endeared than ever to his pupils, to his colleagues, to the entire population; but the susceptibility of his character, always so lively, again prevailing, he threw up all his appointments, and left Mont-pellier for Geneva.

It is easy to imagine how he was received. The enlightened country of the Trembleys, the Bonnets, the Saussures, felt a pride in regain-ing him. There was no chair of natural history—one was created for him. There was no botanic garden—one was founded, and he was soon pursuing the scarcely interrupted course of his lessons and his labors. Nor were these long in reflecting a new lustre on Geneva.

In 1827 appeared the *Organographie Vegetale*, a work which in substance, is but a reproduction of the *Theorie Elementaire*, but with an extraordinary extension and development of its doctrines. In 1832 the *Physiologie Vegetale* made its appearance, and was imme-diately crowned by the Royal Society of London with the high prize which it had just instituted—a distinction due alike to the wide and elevated views, the superior method, and the lucid expositions of this admirable work. The year 1817 had seen the first volume of the *Systeme Naturel des Vegetaux;* the second appeared in 1820.

Let us pass now to another order of facts and ideas, in order to exhibit the merits of De Candolle under a new aspect.

The ancients were acquainted with but a small number of plants; Theophrastus, the most learned of them in this matter, having reck-oned but five hundred. Many centuries after Theophrastus, Tourne-fort counted ten thousand, but without separating varieties from species. Linnæus, in rendering one of the most important services to botany by separating species from varieties, reduced the number of species, properly so called, to seven thousand.

When, about the year 1815, De Candolle conceived the design of drawing up a complete catalogue of the vegetable kingdom, the num-ber of known species scarcely amounted to more than twenty-five thousand. But no sooner had the general peace of 1815 opened the entire world to the researches of travellers, than every year witnessed the arrival of vast numbers of unknown plants from all quarters. De Candolle, in a paper published in 1817, already counted fifty-seven thousand species. "An immense host," he added, "where order the most methodical and natural can alone avoid confusion! Marvell-ous fecundity, which might abate the courage of the botanist, if the first sentiment were not that of admiration for the cause of this count-less variety! Would that botanists might draw from these calcula-tions the conclusion that much remains to be accomplished; that there is fame to be acquired by all; and that consequently it is fitting neither to sleep, as if all were done, nor to be jealous of one another, as if nothing remained to do."

Thus in two years, from 1815 to 1817, the number of known vege-tables had more than doubled. In 1840 this number, according to

the computation of De Candolle, had reached eighty thousand. A single family, that of the *Composita*, as described by him, embraces more than eight thousand species; thus presenting more species in that family alone than was contained in the entire vegetable kingdom of the times of Linnæus. The work in which these eighty thousand known plants are brought together and definitively classed bore at first the title of *Systema Naturale Regni Vegetabilis*, but was recommenced in 1824, under a more abridged form, with the title of *Prodromus Systematis Naturalis Regni Vegetabilis*. Yet in this abridged form it is not the less an immense work. Eighty thousand plants are there ranged in an admirable order—the order, that is to say, of nature itself; each with its characters, its relations, its entire description, and that description of a precision of detail till then without example. This work was left unfinished by its author, though comprising seven massive volumes of from seven to eight hundred pages each. The energy evinced by such vast labors reflects honor not on the individual alone, but on the race; it seems to enhance our idea of the forces of human nature.

The *Prodromus*, as has been just said, remained incomplete. In the memoir which the author has left of his own life, he alludes to the impression produced upon him when he felt that strength would fail him for the completion of his undertaking. "That," he says, "is a great and solemn epoch in life when one acquires the conviction that he has wrongly calculated his plans, and that it is necessary to renounce that to which he attaches the highest price. It should be observed, however, that my own error of calculation has resulted not from idleness on my part, but from the sudden augmentation in the number of known plants." Who might not be dismayed when such a man as De Candolle is found defending himself from the imputation of idleness? He has himself computed that he had established more than seven thousand new species, and nearly five hundred new genera; that is to say, nearly the fourteenth part of known species and the sixteenth part of admitted genera.

As only the more important labors of De Candolle can be here noticed, a multitude of memoirs on pure botany must be omitted; nor can more than an allusion be made to his important studies on the fertilization of downs, on the theory of the distribution of crops, on botanical geography, &c. It is by such of his works as had a direct influence on his age that we must be content to remember him—works which led to his adoption into all the learned societies of the world, and to the inscription of his name, in 1814, among those of the eight foreign associates of this Academy.

Allusion has been made above to the memoir which he left of his own life; and if, in the study of his scientific works, we are struck with the pre-eminence of his intellect and the extent of his acquirements, here we are taught to appreciate the gracefulness, the kindness, and the simplicity of his character. "I have always loved," he says, "those persons who speak of themselves; they are generally persons of good disposition, and who have little to reproach themselves with. And my pleasure," he continues, "in the perusal

of personal memoirs has always been in proportion to the equality of position between the writers and myself. It is not only on account of the style that the Confessions of Rousseau have met with such success, but because he was neither king nor prince, and most readers could trace certain analogies between his position and their own. The memoirs of Marmontel, of Morellet, and, above all, of Gibbon, enable us to see how a mediocrity of condition causes us to overlook. if I may say so, the mediocrity of incidents and the slenderness of the narrative."

De Candolle had a decided fondness for society, and, as Fontenelle said of Leibnitz, "often amused himself with the ladies, and did not count for lost the time which he gave to their conversation." Qualified to please by the characteristic brilliancy and freshness of his imagination, he was excited in turn by the interest and attention of the sex to clothe the driest and most abstract subjects in those terms of animation and imagery which render the lesson which he improvised at Coppet on the "actual state of botany," for the benefit of such a society, one of the most remarkable *resumés* of his theory. Of the interest which he inspired the following may serve as an instance:

Soon after his return to Geneva he was obliged to send back to Spain the beautiful designs of the *Flore du Mexique.* The author of this Flora, the learned Mocino, exiled from his country by the violence of politics, had saved himself from the storm, bearing, like Camoens, his work in his hands. During his sojourn in France, as he despaired of publishing it, he had confided it to De Candolle, with these words : "It is through you that I shall become celebrated." Recalled to his country, now become more calm and just, he was not willing to return without this Flora of Mexico, one of the most valuable services which the Spanish government has rendered to science. De Candolle was about to lose these precious and indispensable materials for his great work. At this news Geneva bestirred itself. De Candolle had hardly thought of having more than a few of the rarest of the specimens copied; it was decided to copy for him the entire Flora; more than a hundred ladies took part in the task, which was completely executed in ten days.

Montesquieu has said "that he never knew a chagrin which an hour's reading had not dissipated." De Candolle might have said as much of society; he not only relaxed himself therein, but his genius acquired new animation and vigor. From the first of his residence at Paris he had had the good fortune to reunite himself with several friends, natives, like himself, of French Switzerland. The family for which J. J. Rousseau had written his Letters on Botany, was naturally the first to appreciate De Candolle. The head of that family, Benjamin Delassert, joined to the care of vast commercial enterprises an enthusiastic love for botany. This taste was the occasion of the closest friendship between him and De Candolle, and it might be here that the latter caught that ardor for the public good which led him to devote himself to active public services. A member of the Philanthropic Society, of a special commission for the hospitals, one of the founders of the Society for the Encouragement of National In-

dustry, &c., he carried into these functions the same fervor as into his studies; and this he called joining a *practical* life to his *theorizing* life. "I have no doubt," he says, in the memoirs before cited, "of the utility of the sciences in general for society, taken in the mass; but it has always seemed to me that I owed, as an individual, some more direct service to my cotemporaries." And this way of thinking seems to have formed the rule of his life. Elected, at Geneva, on three successive occasions, a member of the sovereign council, and, as one might say, an obligatory member of every commission of public utility, he found time and activity for all.

The friends of his youth were those of his entire life. It would be difficult to say in which of the three cities where he had lived, Montpellier, Paris, and Geneva, he counted the most and truest friends. Two of them, Desfontaines and Cuvier, preceded him to the tomb; and the names of these two may stand as a eulogy for all the rest. For his tastes he manifested the same constancy as for his friendships. He began with making verses, and he continued to make them to the last. But having discovered, in good time, that poetry, and especially French poetry, demands great labor, and all the forces of his mind being otherwise employed, he made verses only for his friends, and never published any of them.

His childhood had been delicate, but at the age of fifteen or sixteen his constitution underwent a happy revolution. From that time his body seemed formed, like his mind, for great labors. During more than forty years he preserved a sound health in spite of excessive fatigues. In 1835 he was seized with a violent malady, from which he recovered only to return to his occupations. Since then he has published, perhaps, the most difficult and incomparably the most extensive part of his great work. His fine genius seemed restored to us entire, but his health was never re-established. He died September 9, 1841.

To the happiness due to success, and still more to assiduous employment, he joined the yet more precious happiness of an honorable alliance, contracted in 1808, which constituted the charm of his life. He has left a son worthy of bearing his name and of continuing his renown. His last words were: "I die without disquietude; my son will finish my work."

Such was the life of De Candolle, and such the nature of those great works which mark a new epoch in the progress of botany. Tournefort had constituted the science; Linnæus had given it a language; the two Jussieus had founded its method; it remained to open to botany the study of the intimate laws of life; and this has been done by De Candolle. He is the only one, since Linnæus, who has embraced all the parts of this science with an equal genius.

Considered as a professor he stands without a rival; botany had never before appeared with so much eclat. The perspicuity of his ideas, the soundness of his method, the grace of his elocution, all conspired to captivate and reward attention. When explaining facts, he seemed to communicate the art of judging them; when detailing observations, he appeared to lay open the art of observing; and, as

Fontenelle has said, "the art of observing or discovering is more precious than the greater part of what we discover."

In his writings, it is true, De Candolle exhibits neither the charming style of Tournefort nor the singularly original expression of Linnæus, but he has all the attributes of a writer which result from vigorous thought. He is both elevated and clear—qualities which are often erroneously supposed to be incompatible, as if clearness were not inherent in elevation. *Transcendentally clear*, is an expression of Descartes, the most luminous intellect of France.

As an innovator, the quality which distinguishes him beyond all others is a perfect logic. Logic is the secret guide of genius when it dares successfully.

Considered, finally, as a man, De Candolle must always be regarded as one who, to usefulness as a citizen, added those personal graces of character and gentleness of temperament which make us forget the man of science, and dispose us to pardon his superiority.

ON THE MEANS WHICH WILL BE AVAILABLE FOR COR-RECTING THE MEASURE OF THE SUN'S DISTANCE IN THE NEXT TWENTY-FIVE YEARS.

By the Astronomer Royal.   From the Monthly Notices of the Royal Astronomical Society.

At the meeting of the Society, on the 8th of April, the Astronomer Royal gave an oral statement "on the means which will be available for correcting the measure of the Sun's distance in the next twenty-five years;" the substance of which is contained in the following abstract:

The members of the Society will not be surprised at our looking so far in advance as twenty-five years. The special opportunity, which will then present itself, is the last which will occur for nearly a century and a half from the present time. Some years of preparation will be required to enable us to secure the full advantages which may then be within our reach.   But, with all possible care, it will be found that the risk of total failure is not inconsiderable. The recognition of this danger naturally leads us to examine whether there will not be some earlier opportunity, of a different kind, for arriving at the same determination.   And it will appear (in the judgment of the Astronomer Royal) that circumstances will be favorable, in the course of a few years, for obtaining a very good measure by the use of a different principle; less accurate, undoubtedly, in each of its individual applications than the method upon which reliance has usually been placed, but admitting of almost indefinite repetition, demanding no co-operation of distant observers, and requiring only that, in each instance, the observations which are to be compared be made with the same instrument and by the same observer (or with observers only so far changed that any personal equation would correct itself.)   But even this method requires appliances, which cannot be constructed at the moment of observation; and it is necessary to study well, some time before the operations shall actually commence, what equipment, instrumental and literary, is desirable for giving the best chance of success.   It will appear that we are not beginning too soon to direct our attention to these matters in the present year.

The measure of the Sun's distance has always been considered the noblest problem in astronomy.   One reason for this estimation is, that it must be commenced as a new step in measures.   It is easy to measure a base-line a few miles long upon this Earth, and easy to make a few geodetic surveys, and easy to infer from them the dimensions of the Earth with great accuracy; and, taking these dimensions as a base common to every subsequent measure, it is easy to measure the distance of the Moon with trifling uncertainty.   But the measure of the Moon's distance in no degree aids in the measure of the Sun's distance, which must be undertaken as a totally independent operation.   A second reason is that, in whatever way we attack the problem, it will require all our care and all our ingenuity, as well as the application of almost all our knowledge of the antecedent facts of as-

tronomy, to give the smallest chance of an accurate result. A third reason is, that upon this measure depends every measure in astronomy beyond the Moon; the distance and dimensions of the Sun and every planet and satellite and the distances of those stars whose parallaxes are approximately known.

The received measure of the Sun's distance depends on the transits of *Venus* of 1761 and 1769, but mainly on the latter. Very careful discussions of these will be found in the two books published by Encke, and in a memoir of great value by Don Joachim Ferrers, printed in our own memoirs. On examining these it will be found that, though there is very close accordance in the results obtained by the different investigators and from the different transits, yet all investigators have expressed their doubts upon those results. In the transit of 1761 the result depended almost entirely upon an accurate knowledge of the differences of longitude of very distant stations, which are undoubtedly subject to great uncertainty. In the transit of 1769 it happened that the result depended almost entirely upon the observations made by Father Hell at Wardhoe; and to these great suspicion has attached, many astronomers having, without hesitation, designated them as forgeries. It is evidently desirable to repeat the practical investigation when opportunity shall present itself.

It is desirable, for clearness, to begin with a reference to the simplest operation for measuring distance by parallax; as applied, for instance, to the Moon. In figure 1, let A and B be two observatories on the same meridian, and at A let the star C be observed to touch the moon's limb, and at B let the star D be observed to touch the limb. (It will readily be understood that it is not essential that the observatories should be on the same meridian, if, as is in fact true, the Moon's apparent change of place can be exactly computed; nor is it necessary that the star touch the limb, if its angular distance can be very exactly measured.) After communication of the observations, the observer at A can measure the angle C A D. This angle differs from A M B by the angle A D B; but such is the distance of the stars that the angle A D B is in every case unmeasurably small; and A M B, therefore, is to be taken as equal to C A D. Now, the dimensions of the Earth being known, the length and direction of the line A B will be known, and the directions of A M, B M, are known; and therefore the length of A M, B M, or of any other line drawn from M to any other part of the Earth is easily found. A small error in the angle at M—that is, in the angle C A D—will produce a great error in the result for A M or B M. With this caution the problem is completely solved.

Fig 1.
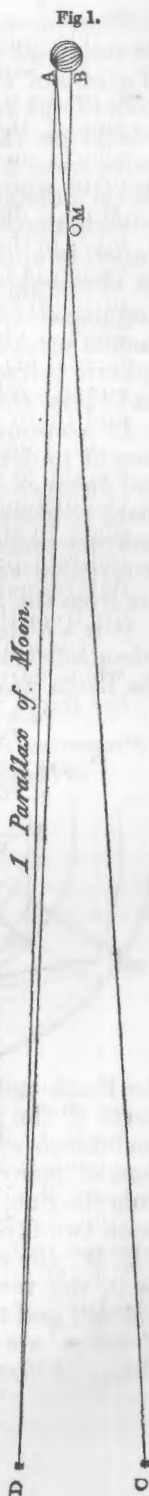
The question naturally rises, cannot the same method be applied to the Sun? Practically it cannot, for the following reasons: First, if errors of equal amount were committed in determining the inclination of the two lines A M, B M, for the Sun and for the Moon, their effects on the results would be enormously unequal. Thus, if the error were 2″, it would produce an error of one hundred miles in the Moon's distance; but it would produce an error of sixteen millions of miles in the Sun's distance. Secondly, no stars can be seen for observation in apparent contact with the Sun's limb. Thirdly, if for want of observable stars we rely upon the instrumental measure of the angular elevation of the Sun's limb, we introduce the risk of instrumental errors, and (far worse) of errors in the computation of atmospheric refraction at the most unfavorable of all times of observation; and these are sufficient completely to vitiate the method.

In consequence of these difficulties, astronomers have always sought to determine the distance of the Sun indirectly by determining the distance of a planet, either by referring the planet's apparent place to stars or by referring it to the Sun. In order to make this indirect process available, it is necessary to rely upon the *antecedent* determination of the *proportion* of the distances of the different planets from the Sun.

It is a historical fact that, in the time of Copernicus and Keppler, when astronomers did not know whether the Sun's distance from the Earth was nearer to ten millions or to a hundred millions of miles,

Fig. 2.

Proportions of Orbits of Planets
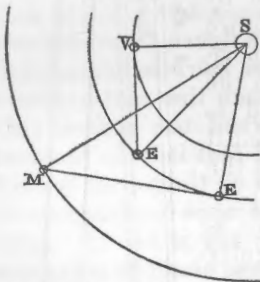
the *proportion* of the distances of the different planets was known almost as exactly as at present. The first and rudest means of obtaining these proportions may be understood from figure 2. Commence with the assumption that the planets move in circular orbits. At the Earth E the apparent angle S E V, between the Sun and *Venus*, reaches, but does not overpass, a certain value. At this time, then, the angle E V S is a right angle. Therefore, in the triangle E V S, two angles are known, (namely, at E and at V,) and therefore the proportions of the three sides can be found, and two of these sides are the distances of the Earth and *Venus* from the Sun. Again, conceive that from the Earth E′ the planet *Mars* is seen in the direction E′ M′. By an acquaintance with the movements of *Mars*, derived from the observations of many preceding years, it is known that his position, as seen from the Sun, is in the direction S M′. The angular difference between these two directions is the angle S M′ E′. Also we know the angle S E′ M′, the apparent angular distance of *Mars* from the Sun. Hence (as in the instance of *Venus*) we know two angles of the triangle S E′ M′, and therefore we know the proportion of its three sides, two of which are the distances of the Earth and *Mars* from the Sun. These, at first, are very rude determinations; but they aid materially

in introducing more exact ones. It is found by degrees that some alteration must be made in the inferred mean distances of the planets from the Sun; it is found by degrees that this will not suffice, and that the supposition of different degrees of ellipticity and in different directions must be introduced; and at length. by infinite repetitions of the process of trial and error, of which scarcely a trace remains, except in the results, proportions of very considerable accuracy are obtained. In all this there is not the smallest reference to any of the absolute distances.

In figure 3 is shown the first practical inference from this knowledge of proportion of distances, as applied to a transit of *Venus*. Let *Venus* V be so exactly between the Sun and the Earth that she can be seen upon the face of the Sun. An observer at A sees her upon the point S, and an observer at B sees her upon the point S′. Suppose the relation between the points S and S′ to be such as to admit of record (the mode of making this record will be considered shortly,) and suppose, by means of that record, the angle S A S′ is measured. The angle which we desire to obtain, in order to measure the Sun's distance is A S′ B. Now, the proportion of our measured angle S A S′ to the desired angle A S′ B, is sensibly the same as the proportion of S′ V to A V, or as 72 : 28, very nearly. Thus it appears that we measure a large angle in order to infer from it a small one: and this is the circumstance which is the most favorable of all for obtaining an exact result. (If we tried to use a transit of *Mercury* in the same way, it would be found that the measured angle at A is to the required angle at S′ in the proportion of 4 to 6 nearly, that is, that we measure a small angle in order to infer from it a larger; hence the transits of *Mercury* are inapplicable to the measure of the Sun's distance.) It is further to be considered that, in this reference of the apparent place of *Venus* to the disk of the Sun, no use is made of stars, and nothing depends on the difficulty of computing refraction, inasmuch as *Venus* and the Sun are, at the time of the observation, subject to the same refraction.

This method then appears likely to be excellent, provided that we possess a practical process for measuring the angle S A S′. The mode of finding this will be our next consideration.

Fig. 3.
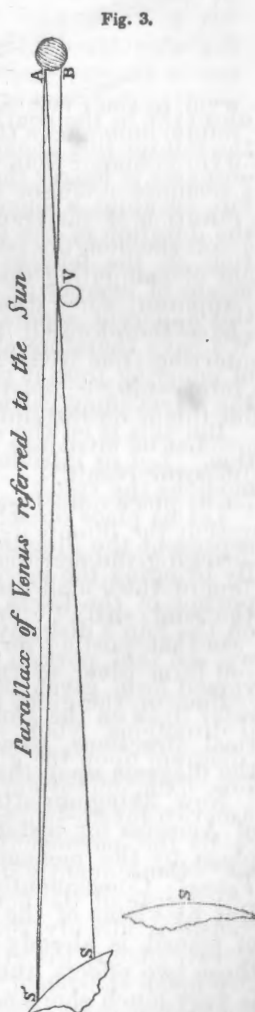
Fig. 4.

Sun's Disk reversed

Egress

Ingress
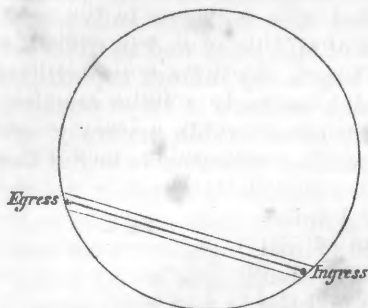
In figure 4 is represented by a black line the path which *Venus* will appear to describe across the Sun's disk in the transit of 1882, (reversed in regard of right and left, for the convenience of subsequent investigations) as seen from the centre of the Earth. For the present let us lay aside the consideration of the Earth's rotation. An observer in the northern portion of the Earth will see *Venus* describe, not the black line, but the fainter line below the black line and parallel to it. An observer in the southern portion of the Earth will see *Venus* describe the fainter line above the black line. The path seen by the southern observer is longer than that seen by the northern observer, and therefore occupies a longer time. Consequently the mere observation of the duration of the transit at these two stations would give information on the lengths of the two chords, and therefore would give means of computing the amount of separation of the two chords: and this apparent separation corresponds to the angle S A S' in figure 3. We have therefore all the means of computing the angle A S' B, and of inferring from it the Sun's distance; although, as may be imagined, the intervening calculations are sufficiently complicated.

But this is on the supposition that the Earth has no motion of rotation. Let us introduce the consideration of rotation, and see how it modifies the result.

Let us place ourselves over a globe with its south pole elevated to represent the illuminated portion of the Earth on the day of transit. By bringing the meridian of 135° E. to the vertical, we shall see the portion of the Earth turned toward the Sun at the ingress of Venus on the Sun's disk; by bringing the meridian of 75° E. to the vertical, we see that portion turned toward the Sun at the egress. The reversed form given to the solar disk in the cut (fig. 4) enables us to refer lines on the globe and on the diagram to corresponding geometrical directions, when we imagine ourselves to be looking through the diagram upon the globe.*

Now, fixing our attention on a northern station, in the United States of America for instance, it will be seen that the translation of this place by the movement of rotation carries it to meet the motion of *Venus*. Consequently it tends to shorten the duration of the transit. But by virtue of the northerly position of that station, the duration of transit is already shortened. Consequently, by combination of these two effects, the duration of the transit at the northern station is very much shortened.

Now, can we select a southern station such that the same rotation

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* On account of the unavoidable omission of the diagrams representing the illuminated portions of the Earth at the times of ingress and of egress of the two transits, a few passages have been omitted, and equivalent ones introduced, using the globe as a means of illustration.—J. H.

of the Earth shall tend to lengthen the duration of transit as seen there? The transit at southern stations is already the longer by virtue of their southern position; and if to this we could superadd a further lengthening by virtue of the Earth's rotation, we should have a very long duration of transit there which we might hope to compare with the very short duration at the north station, with the prospect of obtaining a combination which would be most advantageous for obtaining the measure sought.

We can select such a station.

It is essential to remark, that the transit will take place in the month of December, and that at that time the Earth's south pole will be turned towards the Sun, and therefore, that those regions of the earth which are included between the south pole and the southern limit of illumination will be carried by rotation in a direction opposite to the direction of movement of all the northern parts of the earth. If we fix our attention on a part of the Antarctic continent, between Sabrina Land and Repulse bay, it will be seen that it is carried in the same direction as *Venus*, that the apparent movement of *Venus* is therefore made slower, and that the duration of transit is thereby lengthened. And as it is lengthened already by the southern position of the station, it will by the combination of these causes be very much lengthened. Comparing this with the observed duration in the United States, where it is very much shortened, we shall have a large difference, depending entirely upon the proportion which the Earth's radius bears to the distance of the Sun, and most favorably available for the determination of that proportion. The difference of the times of duration would probably be not less than twenty-five minutes.

Thus the circumstances of the transit of 1882 are peculiarly favorable (subject only to certain practical considerations, to be noticed hereafter,) for the determination of the proportion of the Earth's radius to the Sun's distance, (usually called the Sun's horizontal parallax, or more strictly the sine of the Sun's horizontal parallax.) A discussion of the transit of 1874 will show what are the conditions on which this favorable state depends.

In figure 5, where *Venus* is seen crossing the northern part of the Sun's disk, it will be perceived that the northern station has (independently of the Earth's rotation) the longer duration of transit and the southern has the shorter. Now when we introduce the consideration of rotation — for which purpose we regard the globe with the meridians of 30° W. and 120° W., brought to the vertical to represent the illuminated portions at the ingress and egress, respectively—

Fig. 5.
Sun's Disk, reversed.
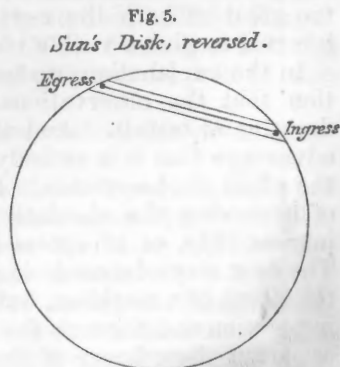
no selection of stations on the principle adopted for 1882 will tend to exaggerate this difference. If both stations are on the north side of

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the south pole, the movement of rotation shortens both durations in no very unequal degrees. If we take a station between the south pole and the southern limit of illumination, the motion of rotation tends to lengthen the duration, which by virtue of southernly position is shorter, and thereby the inequality is diminished. Thus it appears that the transit of 1874 cannot be used with the same advantage as that of 1882 for determining the Sun's horizontal parallax.

An examination of the characteristics of these transits, and also of those which occur in the month of June, (as 1761 and 1769,) when the north pole of the Earth is turned towards the Sun, suggests the following remarks: The transits favorable for the determination of the Sun's horizontal parallax are those in which the part of the Sun's disk crossed by *Venus* has the same name (north or south) as the pole of the Earth which is turned towards the Sun. Now, in general (but not always) the transits of *Venus* will occur in pairs, (as 1761–1769, 1874–1882,) with an interval in each case of eight years. This interval depends on the circumstances that the transits can only be visible when the conjunction of the Earth and *Venus* takes place very near to one of the nodes of the orbit of *Venus* on that of the Earth; and that in eight years *Venus* has revolved almost exactly thirteen times, so that a conjunction at any one degree of heliocentric longitude is followed by a conjunction very near to the same degree after an interval of eight years. But in consequence of the proportion of 8 : 13 being not quite exact, and because in eight years *Venus* revolves a little more than thirteen times, the successive conjunctions take place in $2\frac{1}{2}$ days *less* than 8 Julian years. Therefore, at the second conjunction *Venus* is less advanced in respect of the node than at the first. At the December conjunctions *Venus* is near the ascending node; at the June conjunctions she is near the descending node. In the former, therefore, she will be at the second transit more southerly, and in the latter more northerly, than at the first transit. These indications correspond with those of favorable transits. Therefore, in all cases the second transit of each pair is the more favorable for determining the Sun's horizontal parallax. The exceptional case is when *Venus* crosses the middle of the Sun's disk, as then the latitude of *Venus* is too great at both the next preceding and the next following 8-year interval to give a visible transit.

In the explanation, up to this point, we have gone on the supposition that the observations of transit to be employed are those of duration of transit. And this method possesses the very important advantage that it is entirely independent of the assumed longitude of the place of observation. But there is another method, namely, that of observing the absolute time (as referred to Greenwich time) of ingress only, or of egress only, at different stations on the Earth. The best way of considering this is to conceive that figure 3 is not in the plane of a meridian, but in the plane passing through the observing station and through the Earth's centre. Then it is plain that the apparent disturbance of the point S, from the point at which *Venus* would be seen from the Earth's centre, is in the plane which passes through the observing station and through the Earth's centre. Now,

if this plane is parallel to the Sun's limb at the point of ingress, the disturbance of the apparent place of *Venus* will merely cause its place to slide along the Sun's limb, and will not affect the time of ingress. If the plane is perpendicular to the Sun's limb at the point of ingress the disturbance will tend to throw *Venus* upon or off the Sun's disk in the greatest possible degree, and therefore to accelerate or retard the ingress in the greatest possible degree. But the observed time of ingress must necessarily be expressed, in the first instance, in local time; this can be converted into Greenwich time only by application of the assumed longitude of the place, and, therefore, when we compare the Greenwich times of ingress as observed at two stations, the result is necessarily affected by the possible errors of two longitudes. The same remarks apply to the egress.

We are now in a state to consider the applicability of the two methods to the transits of *Venus* in 1874 and 1882. The calculations of the places of the Earth and *Venus*, upon which the diagrams of figure 4 and figure 5 are founded, have been made by Mr. Breen, assistant to the Royal Observatory, and may be accepted as accurate. At the commencement of this evening's meeting an independent set of calculations was handed to the Astronomer Royal by Mr. Hind, superintendent of the Nautical Almanac, which do not sensibly differ from Mr. Breen's. In the exhibitions of the illuminated side of the Earth, the nearest integral hour of Greenwich mean time is taken, because (as will be mentioned) there is yet a little uncertainty on the exact time.

First. On the application of the method of difference of duration of transit to the transit of 1874.

It has already been remarked that in this transit there is no possibility of combining the effect of Earth's rotation with the effect of difference of latitude of stations, so as to exaggerate the difference of durations of transit depending on difference of latitude alone. And if we consider the effect of difference of latitude only, we find that circumstances are not very favorable. The most northerly stations are to be found in Siberia, Tartary, and Thibet, (which will scarcely be visited by astronomers in December,) on the coasts of China, and in North British India. The most southerly stations will be Kerguelen's island, Van Dieman's Land, and New Zealand. But the observable difference of duration will probably not be half of that in 1882.

Second. On the application of the method of the difference of absolute times to the transit of 1874.

For the ingress, favorable positions will be found at Owhyhee (where the displacement tends to throw *Venus* upon the Sun's disk, or to accelerate the ingress) and at Bourbon, Mauritius, and Kerguelen's island, (where the displacement tends to throw *Venus* from the Sun's limb, or to retard the ingress.) For the egress, Sicily, Italy, and portions of Europe west of the Black Sea, are so situate as to throw *Venus* upon the Sun's disk, or to retard the egress; and New Zealand, New Caledonia, Van Dieman's Land, and Eastern Australia, are well situated for accelerating the egress. But it is doubtful

whether the longitudes of any of the stations named, except those in Europe, are yet known with sufficient accuracy.

Third. On the application of the method of difference of duration of transits to the transit of 1882.

It has been already pointed out that there are two tracts, each sufficient to contain a number of observing stations, which are particularly well adapted to these observations. And it is specially to be remarked that the command of a number of stations, sufficiently near together to see the astronomical phenomenon in nearly the same way, but sufficiently separated to take the chances of different states of the sky, is very important. On occasion of the eclipse of 1842 the astronomers at Turin saw nothing, in consequence of the cloudy state of their sky, while the Astronomer Royal on the Superga, not five miles distant in a straight line, saw all the phenomena of the eclipse. Bearing this caution in mind, we will consider the circumstances of the two tracts in question.

The northern tract includes the whole of the United States of North America. The observatories are numerous, and they possess an advantage which even yet is little known in Europe, namely, that from the extent of galvanic telegraph, the habit of using it in the United States, the public spirit of the nation and of the telegraph companies, which would assuredly induce them to devote that wonderful auxiliary to the exclusive use of astronomy on an occasion so important, and the absence of political suspicions, all the observing-stations would for an observation like this be connected by the galvanic telegraph. (The Astronomer Royal adverts to the political suspicions, not without some bitterness, for he has been prevented by them from using a European telegraph for a single hour to determine the longitude of an important continental point.) The peculiar advantage of connecting, at least of comparing, all the observers' clocks, would be of this kind. Suppose that there were ten observing-stations, and that, in consequence of the changeable weather, the ingress only was observed at five of these stations, and the egress only at the other five: If the clocks of the observatories were not connected or compared, these observations would be totally lost. But if they are connected, then every observation is referred to the absolute time of one clock, say the Washington clock; and from a knowledge of the geographical position, a correction of the absolute time may be computed, so as to deduce, from every observation of absolute time of ingress at any station, what would have been the absolute time of ingress had it been observed at Washington, and from every observation of the absolute time of egress at any station, what would have been the absolute time of egress had it been observed at Washington; and thus we shall have five observations of ingress and five observations of egress, all as if they had been observed at Washington and noted by the Washington clock. Humanly speaking, therefore, we may say that the probabilities for the accurate and efficient observation of these phenomena in the United States are vastly superior to any that could have been reckoned on in any former time, or to any that could now be reckoned on in any other region.

The southern tract is a part of the Antarctic land discovered by

Lieutenant Wilkes, of the United States navy, included between Sabrina Land and Repulse bay, and occupying an extent of about 400 miles. The Astronomer Royal is informed by General Sabine that the 6th of December is rather early in the season for a visit to this land, but probably not too early, more especially as firm ice will be quite as good for these observations as dry land. It must, however, be borne in mind that it is indispensable to secure observations both of ingress and of egress in this tract, without which all the advantages of the North American observations will be useless. For this purpose it appears absolutely necessary to establish a chain of observing posts, and to furnish some means of comparing the clocks. We are in possession now of two powers, unknown in former times, applicable to this purpose. One is the galvanic telegraph, which possibly (but not very probably) might be laid down in a temporary way. The other is the use of steamers, by which the observers would be distributed to their several posts, and which would be constantly employed for some days before and some days after the transit in running up and down the line of coast with a number of chronometers, and comparing them with the stationary chronometers at each observing post. It would be extremely desirable that the country should be reconnoitered some years before the transit, in order to ascertain at a sufficiently early time the practicability of these or some equivalent plans, without which the risk of entire failure would be great.

Fourth. On the application of the method of the differences of absolute times to the transit of 1882.

For the ingress, the islands of Bourbon, Mauritius, and Kerguelen's island, are very favorably situated for accelerating the ingress; and the United States of North America for retarding it. For the egress; Van Dieman's Land, Eastern Australia, New Zealand, and New Caledonia, will have the egress much retarded; while the United States, the West India islands, and the coast of South America as far as the Rio Plata, will have it accelerated.

In the transits of 1761 and 1769 great difficulty was found to attach to the observations of the internal contact of the limb of *Venus* with the Sun's limb, from the phenomenon which in late years has attracted attention under the name of Baily's Beads. The Astronomer Royal expressed his opinion as entirely coinciding with that of Professor Powell, that this phenomenon is simply due to irradiation, as arising partly from diffraction, partly from fault of the telescope, and partly from the nervous excitement of the eye. From his own experience in two total eclipses of the Sun, in which he had taken great pains to see the phenomenon, and had (as he believes simply because he took care to see the Sun very distinctly) been unable to see the slightest trace of it, he had not the smallest doubt that when proper care is taken for distinct vision, the phenomenon will not be seen at all. He referred specially to his delightful view of the very beautiful phenomenon of the disappearance of the last portion of the Sun in the valleys between the lunar mountains, in the eclipse of 1851, which with less distinct vision would probably have created strings and beads. This

distinctness of vision he ascribed principally to the use of a graduated dark glass, constructed under his direction by Mr. Simms. It consists of a long wedge of red glass and a long wedge of green glass, their edges turned the same way, combined with an equivalent wedge of colorless glass, its edge turned the opposite way. Nobody would suppose without trial how fastidious the eye is as to the proper intensity of shade, and how distinctly, when intent on clear vision, it rejects a shade in the most trifling degree lighter or darker. He thought it highly important that such shades should be used for observing the transits of *Venus*. It is desirable also that the color left by the shade-glass should be agreeable to the observer's eye.

Still there is one caution which must not be put out of sight. The selection of places depends entirely upon the portion of the Earth which is illuminated at the times of ingress and egress ; and if the tables of the movements of *Venus* are erroneous in 1882 to the amount of an hour's motion, the illuminated face of the Earth will be altered to the amount of two or three hours' rotation of the Earth, and the selections of stations may be totally changed. It is therefore most important that the tables of *Venus* should be thoroughly examined, and where necessary rectified. A great mass of observations of *Venus* exist, already reduced so far as to require only the very last step of substitution of errors of planetary elements. The Astronomer Royal referred particularly to the Greenwich Planetary Reductions from 1750 to 1830, to the reduction of certain Cambridge observations, to the reduction (in the annual Greenwich volume) of the Greenwich observations down to the present time, and to the discussion of some of the Greenwich observations by Mr. Main and Mr. Glaisher. And he took the opportunity of expressing his opinion that fifty pounds spent on calculations with an object like this would confer much greater benefit on astronomy than a thousand pounds employed in the foundation and equipment of an observatory.

On viewing the expense and the risk of the determinations of the Sun's distance by transits of *Venus*, as well as the distance of time, which must necessarily place them beyond the knowledge of many observers of the present day, it appears natural to consider whether other methods cannot be used, less stringent as to the moment of observation, requiring less co-operation of observations, and occurring at an earlier time. Such are the direct determinations of the parallaxes of *Venus* and of *Mars*, when near to the Earth, by simultaneous observations at northern and southern stations, as in figure 1, or by successive observations at the same observatory when it is brought to different positions by the Earth's rotation, as in figure 6.

Venus cannot be compared with stars on the meridian. She may be compared with stars in extra-meridional observations before sunrise or after sunset, but she is then uncomfortably bright, and rarely well defined; and she has only one illuminated limb admitting of observation, and therefore in the comparison of observations made at different stations there is great risk of error from difference in the estimation of her semi-diameter. Moreover, she does not remain long in the position nearest the Earth, and the nearer she is the more con-

tracted are the daily hours of observation. It seems unlikely that trustworthy results will be deduced from the observations of *Venus*.

Figure 6.
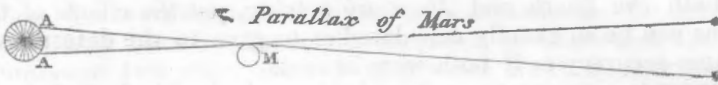
The circumstances of *Mars* in opposition to the Sun (figure 6) are much more favorable. *Mars* may then be compared with stars through the whole night; he has two observable limbs, both admitting of good observation; he remains much longer in proximity to the Earth, and the nearer he is the more extended are the hours of observation.

Here, however, a circumstance is to be considered which has not previously called for attention. The orbit of *Mars* is much more eccentric than those of *Venus* and the Earth. At some oppositions, therefore, he will be so far from the Earth that little advantage will be derived from attempting to observe his parallax. (It is understood that such observations were made in the United States expedition of a few years past, which, from the great distance of *Mars*, must have been nearly useless.) At other oppositions he is almost as near as *Venus* is about conjunction. The following table expresses roughly the distance of *Mars* from the Earth, at some of the nearest and some of the most distant oppositions. The unit of measure is the Earth's mean distance from the Sun:

1860 about July 21 ·························	0.38
1862 about October 1 ······················	0.39
1869 about February 13················· ·····	0.68
1871 about March 22····················· ···	0.64
1877 about September 3····················	0.37

The years 1860, 1862, and 1877 are, therefore, favorable for the determination of parallax. But they require the following special considerations:

When, as in figure 1, the method of comparison of observations made at a northern observatory and a southern observatory is employed, the most favorable position of the planet is that of verticality to the point midway between the two observatories. The north latitude of the northern observatories (Greenwich, Berlin, Pulkowa) is greater than the south latitude of the southern observatories, (Cape of Good Hope, St. Jago.) Hence, *cœteris paribus*, a north declination of *Mars* will be preferable to a south declination. In this respect the opposition of 1862 is preferable to that of 1860.

But there is another method of making observations for parallax not applicable to *Venus*, but applicable to *Mars*, namely, by observing the displacement of *Mars* in right ascension, when he is far east of the meridian and far west of the meridian, as seen at a single observatory. Thus, in figure 6, conceive the pole of the Earth to be turned towards the eye, and conceive the Earth and *Mars* to be stationary in space, the Earth, however, rotating round its axis. By the diurnal rotation, an observatory is carried from the position A to A'; and, at

one of these times, *Mars* is seen in contact with one star, and at the other time with another star. These observations give the means, as in figure 1, of determining the distance of *Mars*. And though, in fact, both the Earth and *Mars* are moving, yet the effects of those motions can be so exactly calculated as to give to the determination the same accuracy as if both were at rest.

In order to compare the value of this method with that of observations on the meridian at two observatories, we must estimate the length of the base line A B in figure 1, or A A' in figure 6. The greatest meridional base line, from Pulkowa to the Cape of Good Hope, is = Earth's radius × 2 sine 47° nearly. The measure of the greatest base line A A' depends on the latitude of the observatory. At Greenwich it is = Earth's radius × 2 sine 38° 30'; at the Cape of Good Hope and at St. Jago (Chili) it is about = Earth's radius × 2 sine 57°; at Madras it is nearly = Earth's radius × 2 sine 77°. Thus it appears that at each of the three last-mentioned observatories the base line which can be obtained is considerably greater than the best which can be obtained by meridional combination of two observatories. At Madras the angle to be measured would be about 44″. To this is to be added that the method is attended with no expense whatever; that the observations which are compared are made with the same telescope and by the same observer, or the same series of observers; that there is none of the tediousness, the wearying correspondence, or the doubt, which are inseparable from observations requiring distant co-operation; and that the observer is supported by the feeling that his own unassisted observations will give a perfect system of means for deciding one of the most important questions in astronomy. The Astronomer Royal expressed his opinion that this method is the best of all.

In order to use the process to the greatest advantage, *Mars* ought to be visible at six hours' distance from the meridian on each side, and therefore his declination ought to have the same name as the latitude of the observatory. Thus 1860 will be a favorable year for the Cape of Good Hope and St. Jago; 1862 will be favorable for North American and European observatories. It is scarcely necessary to discriminate between them for Madras, where both years are good; 1862, however, is preferable to 1860.

The first equipment for this observation, on the necessity for which special stress must be laid, is an equatorial, firm in right ascension. Many modern equatorials are deficient in this important quality. It would be well in using them to apply a temporary mechanism for fixing the instrument in right ascension, such as its construction may permit. The next, which will be found advantageous, though not strictly necessary, is the apparatus for the American or chronographic method of transits, by which the number of observations may be greatly increased, and something will be gained in the accuracy of each. These, with the ordinary clocks and chronometers, &c., of an observatory, are all that are required.

The principal rules for the observer would be: To make the observations, as near as practicable, to the six-hour intervals from the

meridian on both sides, and to repeat the observations in continued sequence morning and evening, morning and evening. If different observers are employed, to take care that each observer is charged as often with morning as with evening observations. To determine the difference between the right ascension of *Mars* and the right ascensions of two stars, one having greater N. P. D. and the other smaller N. P. D. than *Mars*. To use the same stars in at least two observations of different names. morning and evening, and in as many more consecutive observations as can be conveniently arranged. When it becomes necessary to change the selection of stars, to observe both the old pair and the new pair in one morning or evening observation. In all cases to observe, by such alternation as is most agreeable to the observer, both limbs of *Mars*, (the preceding and the following.) The observations might with advantage commence a fortnight before opposition and terminate a fortnight after it.

In the nature of external preparation, applying generally to all observatories, the principal requisite is a chart of the apparent path of *Mars* in considerable detail, giving the place of the planet for every hour or every few hours, and giving the places of all the stars, little and great, in its neighborhood. The observer in possession of this will be able to select stars of such a magnitude as he judges most agreeable to his eye, and at such intervals as will be convenient for his system of wires; and to attend rigorously to the condition of always comparing the planet with two stars, one of greater and one of less N. P. D. It might be proper that the color of the stars should be noted, in order that, to avoid possible inequalities of refraction, stars of the same color as *Mars* (if there are such) may be selected. It would not, perhaps, be too much to expect such charts for 1860 and 1862 from the superintendents of our national ephemerides.

On reviewing the whole subject, the Astronomer Royal presses on the attention of astronomers the importance of observing *Mars* in 1860 and 1862; and for this purpose the necessity of speedily making the preparations, instrumental and literary, which he has described, especially that of the charts of stars with the path of *Mars*. At the same time he urges that the future astronomical public will not be satisfied unless all practical use is made of the transits of *Venus* of 1874 and 1882; and that for these a thorough discussion of the elements of the orbit of *Venus*, the determination of some distant longitudes, and a reconnoissance of Wilkes's land must be effected within a few years.

REPORTS ON THE STATE OF KNOWLEDGE OF RADIANT HEAT, MADE TO THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, AT THE MEETINGS IN 1832, 1840, AND 1854.

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REPORT FOR 1832.

In attempting to give a condensed account of the present state of our knowledge of the science of *Radiant Heat*, it appears to me that I shall be best consulting the design of such a report by offering, in as brief a form as possible, a sketch of what has been formerly done in this department ; and thence proceeding to a more detailed survey of what is now doing. And we shall proceed with greater clearness if we distinguish the several different departments into which the subject divides itself, agreeably to certain known distinctions in the properties and species of heat acting under peculiar circumstances. All these have been too commonly confounded together under the general and vague name of Radiant Heat, whence not unfrequently the most erroneous views have resulted. By distributing our subject, however, under the few well-marked divisions which the scanty results of observation as yet supply, we shall at once secure perspicuity in our views, and be treating the subject in a way most accordant with the inductive process : which, it must be distinctly avowed, has not yet enabled us to advance to any such comprehensive knowledge of the facts as can warrant us in generalizing them, or in ascribing to a common principle the radiation of heat from a mass of hot water, from a flame, and from the sun.

We shall take each of these principal divisions separately, and under each shall consider what is known in reference to those properties to which experiment has been directed.

DIVISION I.

Radiation of heat from hot bodies below the temperature of luminosity.

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a.) *Radiation (or communication of heat to sensible distances) is distinct from its conveyance by* conduction *through the air; since,*

1.) It takes place perpendicularly *downwards :*

2.) Only in *elastic* media.

The relative cooling in different media is seen in the following experiments.—(Rumford's *Essays*, ii, 425 ; Torricelli ; Murray's *Chem.*, i, 328.)

Thermometer cooled from 212° to 32° Fahrenheit :

	in 10 min.	5 sec.
In Vacuo...	in 10 min.	5 sec.
Air	7	3
Water..	1	5
Mercury..	0	36

Dulong and Petit, in their elaborate researches on the cooling of bodies, have investigated the law of cooling in the most perfect vacuum they could form : but they admit that there was always a minute portion of air present. The radiation, therefore, of heat in an absolute vacuum is by no means conclusively established.—(See *Annals of Phil.*, vol. xiii, p. 241.)

3.) Professor Leslie ascertained that the effect from a mass of given size is nearly proportional to the angle which it subtends at the thermometer ; and that the heat suffers little or no diminution in its passage through the air.

The radiation is most copious in the direction perpendicular to a plane surface of the hot mass, and is proportional to the sine of its inclination to the direction of the thermometer.—(*Inquiry into the Nature and Propagation of Heat*, p. 51, &c.)

For the same position the effect is proportional to the *excess* of temperature of the hot body above that of the air.

4.) Pictet made an attempt to estimate the velocity with which heat radiates, by means of concave reflectors at sixty-nine feet distance. The effect on the focal thermometer was absolutely instantaneous.—(*Essais de Phys.*)

b.) *Reflection of simple heat from non-luminous hot bodies.*

1.) The general principles are established by Professor Leslie.—(*Inquiry*, pp. 14, 51.)

2.) He shows that the quantity of heat reflected is proportional to the sine of incidence on a plane surface.

3.) It is affected by the polish of the surface.—(Leslie, *Inquiry*, pp. 81, 20, 98, 106.)

4.) The most exact experiments are those made with conjugate concave reflectors ; a ball of iron below luminosity in one focus, a thermometer in the other : a glass of boiling water may be substituted for the iron ball. In either case a great effect is produced in the opposite focus, though little out of it.—(Saussure, *Voyages*, t. iv, p. 120; Sir W. Herschel, *Phil. Trans.*, 1803, p. 305.)

Professor Leslie made extensive use of reflectors, but observed that there was a *very considerable degree of aberration in the focus* from an exact position ; considerably nearer to the reflector than the true focus, the effect continued undiminished.'—(*Inquiry*, p. 64.)

5.) Alleged reflection of cold.

An account of the earliest experiments will be found in the *Memoirs of the Florentine Academy*, (Waller's *Transl.*, p. 103; also Gærtner, 1781.)

Pictet, with conjugate reflectors, found the thermometer sink when ice was in the opposite focus.—(*Essais de Phys.*, p. 82.)

Count Rumford employed a tube, a frustrum of a cone, open at both ends ; placing ice at the small end, the thermometer at the large end sunk *very little*. The ice being at the small end, the thermometer at the large end fell considerably. Rays reflected by the inside of the tube from the body at the large end would be concentrated on that at the other.

6.) M. Prevost (*Essai sur la Calorique Rayonnant*, Geneva, 1809, and *Recherches sur la Chaleur*, p. 15) proposes a theory of radiation, that heat is a discrete fluid, every particle of which moves in a straight line, and such motions are constantly taking place in all directions, whether there be more or less heat present. Hence all bodies, whether of a higher or lower temperature, are supposed to be continually radiating heat ; and this going on mutually tends to bring them all to an equilibrium of temperature.

On this theory explanations are given of the apparent radiation of cold.

The thermometer in the conjugate focus, when nothing is in the other, remains stationary, because the rays reflected from all the surrounding space so as to cross at the focus of the opposite mirror, and be reflected in a parallel state to the other, and thence on to the thermometer in the focus, are exactly equivalent to those which the thermometer radiates. But when a mass of ice is placed in the opposite focus, it intercepts and absorbs a portion of the rays which would otherwise have fallen on the first mirror, and so have reached the thermometer, which in consequence radiates more than it receives, and therefore sinks.

A similar explanation applies to Count Rumford's experiment.— (See Thomson *On Heat, &c.*, p. 163.)

In the *Quarterly Journal of Science* (June, 1830, p. 378) some observations are given on this subject, and an explanation offered, which, though very ingenious, appears somewhat complicated.

It may not be improper to observe, that if the above be a correct view of Prevost's theory, it can hardly be conceived as otherwise than partially hypothetical. The idea, viz: that bodies even of a lower temperature than those about them actually give out a small degree of heat, is extremely difficult to conceive ; and it does not appear absolutely essential to the explanation of the facts.

Without reference to any theory, I venture to propose the following as the simple experimental law :

All bodies of *unequal* temperature tend to become of equal tempe-

rature ; if in contact—by conduction ; if at sensible distances—by radiation, of the *excess* of heat ; and (in the latter case) whether the radiation reach the cooler body *directly* or by an intervening *reflection*.

This appears sufficient to include the facts of Pictet's and Rumford's experiments.

7.) Alleged polarization of simple heat by reflection.

Mons. J. E. Berard (Mémoire sur les Propriétés des différentes Espèces de Rayons qu'on peut séparer au moyen du Prisme de la Lumière solaire, *Mém. de la Société d'Arcueil*, Paris, 1817, tome iii; see also *Annals of Phil.*, O. S., ii, 164; Biot, *Traité de Phys.*, iv,) tried experiments for the polarization of heat. His apparatus was the same as Malus's, having the axis of revolution vertical ; but no precautions of screening, &c., are mentioned. He used an air thermometer containing a bubble of alcohol in the tube, in the focus of a reflector, moving round along with the second glass: a ball of copper about two inches in diameter was in the focus of a reflector placed in the position for polarization of light. (His experiments on heat with light will be referred to in another place.) He tried the effect with the metal heated below luminosity, and assured himself that there was a difference in the degree of heat reflected in the two rectangular azimuths of the second glass.

I have attempted to repeat these experiments with the same kind of apparatus, carefully screened and arranged with the tube horizontal ; but could produce no diminution in the proper position.—(*Edinb. Journal of Science*, N. S., vol. x, p. 207.)

I also tried the experiment with a delicate mercurial thermometer, comparing this case with others, (referred to in their proper place,) in which light accompanied the heat ; but in the former could detect no difference in a long series of repetitions.

The total effect is in all cases extremely small, and the disturbing causes considerable, especially the heating of the glasses, &c. The whole experiment was very unsatisfactory.—(*Edinb. Journal of Science*, N. S., vol. vi, p. 297.)

c.) *Effect of the nature of* surfaces *on the* emission *of simple heat.*

1.) Count Rumford (*Nicholson's Journal*, ix, 60) employed two similar vessels of hot water of the same temperature—one naked, the other coated with linen, glue, black or white paint, or smoked with a candle ; the results were,

> Naked vessel cooled 10 degrees in 55 minutes.
> Coated   "   10   " $36\frac{1}{2}$ "

Mr. Murray supposes a relation between radiating and conducting powers.—(*System of Chem.*, i, 326, 334. See *Phil. Trans.*, 1804, p. 90, &c.)

2.) The most complete investigation of this and other parts of the subject has been made by Professor Leslie in his *Inquiry into the Nature and Propagation of Heat*, 1804.

He first used hot water in a globe of tin, in which the inserted thermometer fell a given quantity, with the tin bright, in 156 minutes; with the tin coated with lampblack, in 81 minutes.

The difference was greatest in still air, and diminished with the violence of its motion :

Time of cooling.

Wind.	Bright.	Blackened.
Gentle ·············	44 minutes ······	35 minutes.
Strong ·············	23 " ······	20¼ "
Violent.. ··········	9½ " ······	9 "

Hence the effect is different from conduction by air.

3.) The most exact series of experiments was that in which he used conjugate reflectors, a differential thermometer having one bulb in the focus, and a cubical tin canister of hot water, (the temperature of which was seen by the projecting stem of a thermometer,) and each side of which could be coated with a different substance, and presented successively towards the reflector.

The following results collected together afford the best view of the general nature of the conclusions relative to the influence of the *state of the surface* on the radiation of heat.—(*Inquiry*, pp. 81, 90, 110.)

Lampblack ····························	100*
Water, (estimated)..·····················	100
Writing paper ·························	98*
Rosin ·······························	96
Sealingwax. ··························	95
Crown glass ··························	90
China ink ···························	88*
Ice ·······························	85
Minium ···· ························	80*
Isinglass ····························	80
Plumbago ···························	75
Thick film of oil ·····················	59
Film of jelly ························	54†
Thinner film of oil ···················	51†
Tarnished lead. ·····················	45
Film of jelly, (¼ of former quantity) ··········	38
Tin scratched with sandpaper ·············	22
Mercury ····························	20
Clean lead ··························	19
Polished iron ·······················	15
Polished tin, gold, silver, copper ···········	12
Thin lamina of gold, silver, or copper leaf on glass	12‡

* From comparing the results marked, it appears that the effect follows *no relation to color*. *Softness* probably tends to increase radiation.

† Thickness of film increased beyond a certain limit does not increase the radiation.

‡ The tenuity is not sufficient to produce any diminution of effect which probably would take place if thinner films could be applied.

4.) The effect of the surface on radiation is beautifully exemplified in the laws which regulate the formation of dew as developed by Dr. Wells.—(*Essay on Dew*, 1814. See also Dufay, *Mem.*, Paris, 1736, p. 352; and Harvey on Dew, *Quarterly Journ. of Science*, No. 33; *Edinb. Journ. of Science*, i, 161.)

5.) Dr. Ritchie (*Edinb. Phil. Journ.*, xxiii, 15) explains his theory of the mode in which the radiating power of surfaces is increased by making them rough, or furrowing, &c. He contends that it is not owing to the increase of surface, but to the quantity of heat reflected by the sides of the furrows.

He adopts the hypothesis of material caloric, and that its molecules are mutually repulsive.

The effect of surface is an essential distinction between radiation and conduction by air ; the latter being shown by Dulong and Petit to be absolutely independent of the nature of the surface.—(*Annals of Phil.*, xiii, 322.)

d.) *Effect of* surface *on the* absorption *of heat from non-luminous hot bodies.*

1.) De Saussure and Pictet, with the apparatus before described, found that the thermometer rose in two minutes:

Plain .... $4\frac{1}{8}^\circ$ Fahrenheit.
Blackened .... $3\frac{1}{8}$

2.) By the same apparatus before described, Professor Leslie found that on coating the bulb of the thermometer with the different substances, the *absorptive* power was very nearly in the same proportion as the *radiative;* and by making the same modifications in the surface of the reflector, he found that *reflective* power is inversely as the *radiative* or *absorptive.*—(*Inquiry*, pp. 19, 81, 98.) He also gives a very precise set of experiments on the effect of coatings of jelly of increasing thicknesses.—(p. 106.)

3.) Dr. Ritchie has devised a very elegant mode of showing that the absorptive power of surfaces is precisely proportional to their radiating power.—(*Royal Inst. Journ.*, vol. v., p. 305.)

The instrument consists of a large differential thermometer, whose bulbs are chambers of considerable size, presenting large and equal plane surfaces on the sides which are towards each other; of these one is plain or polished, the other coated. Midway between them is placed a canister having equal plane surfaces facing each of the former respectively, and one polished, the other coated with the same pigment as before; this canister is filled with hot water, and is capable of turning on a vertical axis; thus the coated surface of the canister can be turned to the coated bulb or to the polished; in the former case a great effect is produced on the coated bulb, and a very small effect on the plain; in the second case the better radiating surface is directed to the worse absorptive one, and the worse radiating to the more absorptive, and the liquid in the tube remains *perfectly stationary;* the exact equality, therefore, of the absorptive and radiating powers is established. The whole is on a large scale, and can be exhibited to a class.

4.) The most recent and curious researches on this part of the sub-
ject (and extending, as we shall see, to other parts also) are those of
MM. Nobili and Melloni.—(*Annales de Chimie*, Oct., 1831; Recherches
sur plusieurs Phénomènes Calorifiques, &c.)

The authors commence by describing their *thermo-multiplier*, by the
aid of which their researches were carried on. This consists in a
thermo-electric combination, susceptible of excitation from the feeblest
conceivable application of heat, and connected with a delicate *gal-
vanometer*, which gives a *measure of the effect produced, and conse-
quently of the heat.*

The pile is in a case coated with the smoke of a flame when used
for radiant heat, but left naked when for heat of temperature, on
account (as they observe) *of the bad conducting quality of this coating.*

They applied this instrument to the examination of the different
reflecting, absorbing, and radiating powers of surfaces.

They confirmed in general the results of Leslie and others already
mentioned. They found that *polish* augments the reflecting power
much less than usually supposed. Non-metallic substances possess
scarcely any reflecting power, whatever be the state of their surfaces.

They examined the absorptive power of different substances, taking
laminæ of equal thickness and similarly fixed, &c.; these having been
heated for a few minutes *in the rays of the sun*, were placed in pairs
on apertures at the opposite sides of the thermo-multiplier, and in
this way the *order* of their absorptive powers was considered to be
obtained by the degree of heat they respectively radiated; and the
results were, that the effect increased by blackness of color and with
roughness of surfaces. Also the following surfaces were in this order:
silk, wool, cotton, flax, hemp, (all *white*,) which is the inverse of their
conducting powers. In like manner, with metals of nearly the same
color and polish, the order was—copper, silver, gold, steel, iron, tin,
lead, exactly in the inverse order of the conducting powers; the same
with several woods and minerals.

On these experiments I must remark, that the heat *acquired from
the sun's rays* is so obviously dependent on *color* that it is astonishing
that any experimenter should adopt this as affording any ground for
making conclusions respecting the comparative absorbing or radiating
powers for *heat in general.* The later results, when the surfaces were
all of the *same color*, are extremely important. Supposing they all
acquired the same degree of solar heat which was thus converted into
heat of temperature, and then radiated from the surfaces as simple
heat, the real conclusion established is, that the RADIATING *powers of
surfaces for simple heat are in the inverse order of their conducting
powers.*

e.) *Effect of screens on heat from non-luminous hot bodies.*

1. Pictet found a difference in the interceptive effect, according
as the plain or the silvered side of a glass screen was towards the
source of heat.

Towards hot body.	Ratio of effects on thermometer.
Glass····································	5
Amalgam······························	35
Amalgam, blackened ·················	92
Amalgam removed,—glass blackened ···········	180

<div align="right">(<i>Essai, &c.</i>, p. 72.)</div>

2.) He tried to *refract* simple heat without effect.

Sir W. Herschel tried with a lens, and supposed it effected: this has been refuted by Sir D. Brewster.—(*Vide infra; Phil. Trans.* 1800, Part II, No. 15, Exp. 19, 20.)

3.) Professor Leslie's experiments on screens are perhaps the most valuable portion of his inquiry.

He found the effect of a screen increase rapidly with its distance from the source, (p. 28,) and less so with its thickness, (p. 38.)

Different substances appear to have a different interceptive power; but this upon examination appears always to be dependent on their *conducting* power, and the *absorptive* nature of their *surface jointly*.

The most decisive experiment on this point was that made with two panes of glass, each having one side coated with tin foil; according as the plain or coated sides were placed in the contact, the compound screen had a greater or less *apparent* interceptive power; that is, a greater or a less power of *absorbing* and subsequently *radiating* the heat. Again, either might be used separately, or the two at an interval.—(p. 35.)

4.) Prevost concluded that a certain portion of heat is *directly* transmitted through transparent screens, by employing *moveable* screens which continually presented a fresh surface, so that it was supposed all communication of heat and conveyance by way of secondary radiation would be prevented.

But it must be considered that it is impossible to prevent entirely any portion of a screen in the most rapid motion from acquiring heat: no such experiments therefore can be strictly conclusive.

Dr. Ritchie tried experiments with the same view, by means of a film of liquid adhering to threads stretched across a frame continually renewed.—(*Phil. Trans.* 1827, Part II, p. 141.) But to this a similar objection must apply.

5.) The results of Professor Leslie do not apply to temperatures above those of boiling water.

This extension of the inquiry formed the subject of the researches of De la Roche. The complete account of these is given in its proper place; at present we have to consider them only as far as relates to bodies below luminosity. He tried the effect of a screen of glass, first transparent, and then with one surface blackened, on the heat radiating from mercury at 180° centig. and at 346 when it was boiling.—(Biot, *Traité de Phys.*, iv, 640.)

The results were as follows:

20

Rise of focal thermometer (centig.) in 1^m.

	No screen.	Transparent screen.	Blackened screen.
Mercury at 180°	3°.94	0°.22	0°.07
Mercury at 346	16 .33	1 .36	0 .17

He hence infers a partial *transmission* of heat at these high temperatures; and the more so, viewing these results in connexion with the rest of the subsequent series (considered in another place.)

These are the only ones of his experiments referring really to *simple* radiant heat; and the inference of an actual transmission in the way of direct radiation is open to several objections.

6.) The blackened screen causes a greater diminution of heat than the transparent, and it was therefore inferred that a portion of heat radiates freely through the transparent screen, and is stopped by the opaque one; but there are several circumstances which show that this is not a *necessary* conclusion.

The coating was *towards* the source of heat, and rendered this screen more absorptive of heat where exposed to it, that is, at its central part, and a better radiator towards the edges without the area of the incident rays; so that it radiated its heat most copious on the side away from the thermometer. With the plain screen there was no such tendency to radiate more on one side than on the other, and hence the greater effect on the thermometer.

This explanation I suggested in the *Annals of Philosophy*, xlv, 181.

Some observations bearing upon this subject occur in Sir David Brewster's elaborate paper on "New Properties of Heat," &c., in the *Phil. Trans.* 1816, Part I. His 40th proposition is directed to prove that radiant heat is not susceptible of refraction, and is incapable of permeating glass like the luminous rays. The truth of this is demonstratively shown from the curious properties examined in the previous parts of the paper, and shown to be communicated by heat to glass; and by the progress of which, the passage of the heat through the glass may be as clearly traced as if the heat itself were visible.

He applies this conclusion to the experiment of Sir Wm. Herschel in which the concentration of simple heat by a lens appears to be proved. The thermometer must have received the heat radiated by the lens itself ; and from the circumstance that the edges will cool first, the most copious radiation of heat will be in the direction of the axis.

In connexion with the same point, he also examines the conclusions of MM. De la Roche and Prevost, and observes: "The ingenious experiments of M. Prevost, of Geneva, and the more recent ones of M. De la Roche, have been considered as establishing the permeability of glass to radiant heat. M. Prevost employed moveable screens glass, and renewed them continually, in order that the result he obtained might not be ascribed to the heating of the screen: but such is the rapidity with which heat is propagated through a thin plate of glass, that it is extremely difficult, if not impossible, to observe the

state of the thermometer before it has been affected by the secondary radiation from the screen.

"The method employed by M. De la Roche, of observing the difference of effect when a blackened glass screen and a transparent one were made successively to intercept the radiant heat, is liable to an obvious error. The radiant heat would find a quicker passage through the transparent screen, and therefore the difference of effect was not due to the transmitted heat, but to the heat radiating from the anterior surface. The truth contained in M. De la Roche's fifth proposition is almost a demonstration of the fallacy of all those that precede it. He found that a thick plate of glass, though as much or more permeable to light than a thin glass of worse quality, allowed a much smaller quantity of radiant heat to pass. If he had employed very thick plates of the purest flint glass, or thick masses of fluid that have the power of transmitting light copiously, he would have found that not a single particle of heat was capable of passing directly through transparent media."

7.) I have further attempted a direct experimental examination of the question in a paper inserted in the *Phil. Trans.*, 1826, Part III, p. 372.

The substance of my observation is as follows :

De la Roche found that if radiant heat be intercepted by two transparent screens, the *additional* diminution of effect occasioned by the *second is proportionally much less* than that produced by the *first ;* and the same conclusion is extended to any number of screens. This was explained by the supposition that the heat in its passage through the first glass undergoes a certain modification, in some respects analogous to polarization, by which it is enabled to pass with very little diminution through the second and subsequent glasses.

In those cases where the source of heat is luminous, such phenomena would receive an obvious explanation on the principle investigated in my other paper.—*Vide infra.*

But if the same effect is still observable below the point of luminosity, we must have recourse to some other principle of explanation. That deduced by De la Roche appears at least plausible; and though it should be considered proved that, in general, heat is incapable of being radiated directly through glass, it perhaps would not necessarily follow that it might not, under peculiar circumstances, have a power of doing so communicated to it. Though, on the other hand, it must be confessed that in the present case some difficulty would attend such a supposition.

It certainly would not be easy to conceive such a property to be communicated to the heat by the mere act of being conducted through the first glass. Again, a new property of heat is thus introduced, which, it must be conceded, is not absolutely and exclusively established.

It appeared to me, therefore, a point of some interest to examine, in the case of non-luminous heat, in the first place, the accuracy of the fact, and secondly, if verified, whether there might not be circumstances observable in the conditions of the experiment by which it

might be accounted for, without the necessity of supposing any peculiar property of heat, or a direct transmission even through the second glass.

My apparatus, in following up this inquiry, was similar to that described by M. De la Roche, and consisted of two tin reflectors; in one focus the bulb of a thermometer coated with Indian ink, and in the other an iron ball two inches diameter, which was heated to redness, and then cooled till it ceased to be visibly red in the dark, at which point it was placed on its stand, and a thick screen withdrawn. The indications were observed, first, for the direct effect ; secondly, with one glass screen interposed; and thirdly, with two. The temperature of the screens was observed by means of a small thermometer attached to the face of each away from the ball, towards its central part; the bulb being kept in contact by the spring of a wire with which the thermometer was fastened.

The results are: First. That the additional diminution occasioned by the second screen is proportionally much smaller than that occasioned by the first. Thus De la Roche's conclusion is shown to hold good, not only in the case of luminous, but also of non-luminous hot bodies, which is perhaps of consequence, as I believe doubt has been entertrined respecting it; and it may be remarked that here the greater thickness of the second screen would be against such a result. Secondly. If the progress of the indications of the direct effect be followed, it appears that the rise in the first 30 seconds is the greatest, and that those in the subsequent periods gradually diminish. Thirdly. With one screen the effect in the first period is equal to or even less than those in the subsequent ones; and if we follow the temperature of the first screen, it appears to sustain a rapid increase at first, and afterwards continues gradually to rise till some time after the focal thermometer has become stationary. The progress of the focal thermometer exactly accords with what must be the heating effect of the screen as a source, viz: rising slowly at first as the screen acquires heat sufficient to supply it, and remaining stationary so long as the still increasing temperature of the screen could balance its loss of heat. Fourthly. With two screens there is no rise till the second half minute, when it is not greater than in the next half, after which the thermometer becomes stationary, and this trifling effect exactly accords with what the temperature of the second screen should produce. It does not begin till the second screen has acquired a higher temperature, and it is stationary while the temperature of the screen continues to increase, and the temperature of the second screen is such as is clearly accounted for from the heating effect of the first. It does not begin to rise till after that of the first has risen; it continues stationary some time after the first has begun to cool, as the first screen did when the iron was cooling. But as in this case the source of heat was cooling during the whole time of the experiment, whilst in the other it was heating during the first part of the time, it follows that a greater proportional temperature should be communicated to the second screen by the first, than to the first by the iron ball.

Other circumstances will partially co-operate in producing this

effect, as the greater proximity of the second screen to the thermometer; also more heat might be lost in communicating an equable temperature to the first screen from its central and more heated part; whilst the heat would be thus more equally radiated to all parts of the second without such loss.

Thus it appears that the fact stated by M. De la Roche is fully substantiated, while, on the other hand, it is satisfactorily accounted for without supposing any new property of heat or any direct radiation through glass.

In some unpublished experiments of my own, I found, upon observing the *temperature* acquired by a screen exposed to iron below luminosity, first plain, and then coated with Indian ink towards the source of heat, the thermometer being in contact at the central part on the outside, that it rose rather *more* on the *plain* than on the *coated screen.*

8.) MM. Nobili and Melloni, in the memoir before quoted, applied their instrument to estimate the effects of transparent screens. Over the thermo-multiplier were placed successively transparent screens of glass, sulphate of lime, mica, and of water, oil, alcohol, and nitric acid (enclosed between plates of glass?) and also of ice.

The source of heat was a ball of iron, heated to a point below luminosity, suspended, or rather passed rapidly, at a certain distance above the screen.

The index indicated an *instantaneous effect*, greater or less in all cases except those of water and ice, in which none was produced, even when the iron was kept a longer time over the instrument, or even heated to redness, and the screen reduced in thickness.

9.) A set of experiments presenting some important results with respect to the absorbing and radiating properties of surfaces, as well as the action of screens in air and in vacuo, are given by Mr. W. R. Fox, in the *Phil. Mag. and Annals*, New Series, No. 65, p. 245. A brief statement of the results is as follows :

A cylindrical tin vessel of hot oil with its surface polished, and another similar, painted black, had their times of cooling a certain number of degrees observed under a receiver first highly exhausted, and then full of air; the cylinders being respectively first exposed, and secondly enclosed in one and sometimes more tin cases with intervals; the outer and inner surfaces being one or both polished or blackened. From all the different combinations of these results, of which he states in detail, I collect the following general inferences:

I. In *vacuo:* (1) the *polished* vessel had its cooling always *accelerated* by the cases, and in this order—

	Case.	
	Inside.	Outside.
Most accelerated ·························	bright ···········	black.
	black ···········	black.
	bright (3 cases)	black.
	bright (3 cases)	bright.
	bright (1 case)	bright.
Least accelerated ····················	black ·········	bright.

(2.) The *coated* vessel had its cooling in all cases *retarded ;* and in this order—

	Inside.	Outside.
Least retarded ·	black · · · · · · · · · · black	
	bright · · · · · · · · · black	
	black · · · · · · · · · bright	
Most retarded ·	bright · · · · · · · · bright	

II. In *air : both* vessels in all instances had their cooling *retarded* by the cases.

Mr. Fox also found the boiling of water in a bright vessel *before* a fire *accelerated* nearly doubly by a *case blackened externally.*

He considers the results inexplicable, except on the hypothesis of an attraction between matter and heat.

Mr. Fox has also communicated to me in manuscript an account of some further experiments of the same kind on iron raised to a red heat, but which, nevertheless, are of such a nature as properly to come under this division of the subject.

The precise temperature to which the iron was raised in each experiment was estimated by the remarkable cessation of its action on a magnetic needle at a certain stage of incandescence.

The iron was enclosed in tin cases of two different sizes, within which the air could be exhausted, the inside being either plain or coated with lamp-black.

The whole was immersed in water, and the temperature communicated to the water in a given time noted. After observation the iron was plunged in water, and the residual heat thus communicated to the water noted.

The general results were, that in the *smaller* case the cooling was *more rapid* than in the larger; and in either the internal *coating accelerated* the cooling; in no case was any material difference produced by exhausting the air.

10.) Dr. Ritchie (*Edin. Phil. Journal,* xxii, p. 281,) has shown that when a hot non-luminous body is placed between the two bulbs of a differential thermometer, blown out very large and thin, and both remaining plain, the liquid is stationary; the *outside* half of one being *coated with black,* the liquid sinks from that side.

Hence he infers that the coating has here stopped the heat, which otherwise radiates freely through the *very thin* glass.

He varied the experiment by using portions of glass blown thin as screens over an aperture: when blackened in a flame or coated with silver leaf they intercepted heat; when transparent, not. That this was not from increase of thickness was shown by using three thicknesses transparent, then removing the middle one, and blackening the inner surface of the others.

He explains the subject by the theory of material caloric and mutual repulsion of its particles.

The same author in another paper (*Ann. of Phil.,* 2d series, xii, 123,) gives a variation of the experiment: the hot body is placed between two large and very thin bulbs; one of the hemispheres of

one bulb, formed by a plane passing through the centres of both, is coated with China ink, as are also two of the alternate quarters of the other, formed by a plane cutting the former at right angles.

A greater effect is produced on this second bulb.

This is an argument against the effect being due to greater radiation from the outer surface of the bulb.

Dr. Ritchie has also maintained the same conclusions in his paper before referred to, (*Phil. Trans.*, 1827, Part II, p. 142,) by varying the *distance* of the screen, which he found to produce no sensible difference in the effect, though with screens of moderate thickness it diminishes rapidly with the distance, according to Leslie's experiments.

Division II.

TERRESTRIAL LUMINOUS HOT BODIES.

a.) *Nature of radiation.*

The earliest observers noticed differences between this case and that of heat from non-luminous bodies.

The heat from flame, &c., *at least in part*, passes through air, &c., without heating it.

Scheele observed this with a fire, and that currents of air did not change the direction of the rays.—(*Treatise on Air and Fire, &c.*)

Cavallo (*Phil. Trans.*, 1780,) found a blackened thermometer affected by the light of a lamp.

Leslie (*Inquiry*, p. 448,) found a fire affect his photometer; also candles, &c., (p. 447,)—a distinction pointed out between this and the solar rays, (p. 83, 54.)

The light from putrescent substances does not appear to be accompanied with any appreciable degree of heat, according to Dr. Hulme. (Thomson's *Chem*, i, 414, 4th edit.) But the effect, if any, must be so small that we cannot positively assert there is none.

The same remark may apply to many other very faint lights.

b.) *Reflection of heat.*

1.) Mariotte collected the heat of a fire in the focus of a reflector. —(*Mem. Acad. of Sciences*, 1682.)

Lambert, with burning charcoal in the focus of conjugate reflectors, found a combustible body kindled in the other focus.—(Lambert, *Pyrometrie;* Saussure, *Voyage*, iv, 119.)

Scheele (*On Air and Fire*, p. 67, 71,) observes that a glass mirror, though it reflects the light of a fire, does not reflect the heat, (it is not stated by what means the heat was estimated,) but the mirror becomes heated. A polished metallic mirror reflected both the light and heat, and did not become much heated itself; if blackened it was soon hot.

Pictet extended the experiments with conjugate reflectors to this case, by placing a candle in one focus. The thermometer rose nearly 10° in six minutes.—(*Essais de Phys.*, p. 63.)

Sir W. Herschel (*Phil. Trans.*, 1800, p. 297,) placed a candle at twenty-nine inches from a concave metallic reflector; the focal thermometer in five minutes rose $3\frac{1}{4}°$; another out of the focus was not affected.

The same took place with a fire, and with red hot steel.

2.) Polarization by reflection.

Berard (Memoir before cited) tried the polarization of heat from luminous sources, and found a considerable diminution in the position when *the light* ceases to be reflected.

There was of course here no distinction drawn between the heat accompanying the light and the simple heat. Of the latter nothing is proved; the former may be merely an effect of the *absorption of light*, and if so, the term polarization is applied to the *heat* without any proof.

I repeated these experiments, and, after all precautions, thought there was a small perceptible effect, (when the simple heat was cut off by a glass screen,) which was diminished in the position of non-reflection for the light; when the whole heat was admitted no *proportional* diminution took place.—(*Edinb. Journ. of Science*, vi, 303.)

c.) *Effect of surfaces on emission of heat.*

Nothing ascertained under this head, unless we except some remarks in the *Edinb. Journ. of Science*, No. ii, p. 302.

d.) *Effect of surfaces on absorption of heat.*

All experimenters have usually blackened their thermometer.— (Cavallo, *Phil. Trans.*, 1780.)

Prof. Robison exposed a thermometer on *charred oak* under a glass cover to the rays of a fire, when it rose to 212° Fahr.—(Black's *Lect.*, i, 547; Thompson, i, 127.)

e.) *Effect of screens.*

1.) Mariotte interposed a glass screen between the fire and concave mirror, and found the heat no longer sensible at the focus.—(Biot, iv., 606; *Mém. Paris*, i, 344.)

Scheele interposed a glass screen in the experiment before mentioned, and found the heat of a fire so much intercepted as to be no longer sensible to the hand—not even sensible in the focus of a reflector.

Pictet with the conjugate reflectors interposed a glass screen. The focal thermometer, which had risen 10°, fell 7° in nine minutes; on removing the screen it rose again.—(*Essais de Phys.*, p. 63.)

2.) Sir W. Herschel tried experiments on this point.—(*Phil. Trans.*, 1800.) Two moveable objects illuminated by a lamp were viewed by the eye, one through an open hole, the other through a hole covered successively by different transparent media. One object was moved to greater or less distance, till they appeared equally bright; the

interceptive power was estimated directly as the illumination required to produce the equalization, that is, inversely as the square of the distance.

Two equal thermometers enclosed in a box, with apertures over the bulbs, (which were plain,) one open, the other covered successively by the different transparent media, were exposed to different sources of heat, and the interceptive effects compared together and with those of the same media for light. Thus among the results were the following:

	Common Fire.		Candle.	
	Light.	Heat.	Light.	Heat.
Coach glass ·························	0	750	86	625*
Dark red glass ······················	999	573	999	526

3.) Refraction by lenses.

Lambert collected the rays of a fire by a large lens and found the heat scarcely sensible to the hand.

Sir W. Herschel (*Phil. Trans.*, 1800, pp. 272, 309, 327,) received the rays of a candle on a lens, with a pasteboard screen, having an aperture nearly equal to that of the lens; the thermometer in the focus rose $2\frac{1}{2}°$ Fahr. in 3 minutes; the same with the rays from a fire, and from a mass of red hot iron.

M. Brande found the rays of a flame, concentrated by a lens, produced an effect on a blackened thermometer in its focus; the lens did not become heated.—(*Phil. Trans.*, 1820, Part I.)

4.) Dr. Ritchie found that if Leslie's photometer be placed opposite a ball of iron heated almost to redness no effect whatever will be produced; but if the temperature of the ball be raised so as to shine in the dark with a dusky red color, the fluid in the stem of the black ball will sink a considerable number of degrees. If the temperature of the ball be raised still higher it will produce a greater effect upon the instrument than the flame of the finest oil-gas, though the one possesses a much greater illuminating power than the other.

Dr. Turner and Dr. Christison have found that Leslie's photometer "is powerfully affected by heat" when placed "before a ball of iron heated so as not to be luminous, or even before a vessel of boiling water." The opposite result of Dr. Ritchie may possibly be owing to some difference in the surface, substance, or thickness of the *black* bulb employed.—(*Edinb. Journ. of Science*, iv, 321.)

I have found differences, which I am at a loss to account for, between the effects on a differential thermometer with the bulbs of equal height, and one in which they are in a vertical line.

5.) That there exist essential differences between the constitution of the heating power of *luminous* hot bodies and that of the same power proceeding from those which are non-luminous was remarked by former experimenters. But it is a point which does not seem to have excited any close or systematic inquiry until the subject was

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*Out of 1,000.

taken up by M. De la Roche, whose researches are justly entitled to the high celebrity they have acquired. The report of the French Institute upon them will be found in the *Annals of Phil.*, O. S., ii, 161; and a full account of the experiments in Biot's *Traité de Phys.*, iv, 640.

The whole series of results is as follows:

Source of heat.	RISE OF THERMOMETER IN 1 MIN. CENTIG.		
	No screen.	Transparent screen.	Blackened screen.
	°	°	°
1. Vessel of mercury, temp. 180° cent ___ ___	3. 94	0. 22	0. 07
2. Vessel of mercury, boiling, 346° _____	16. 33	1. 36	0. 17
3. Iron, 427° _____	32. 8	4. 70	0. 31
4. Copper, 960°, (1) _____	38. 97	11. 83	0. 40
5. Ditto _____ (2) _____	71. 54	21. 41	0. 73
6. Argand lamp—no chimney _____	21. 12	7. 29	0. 21
7. Argand lamp—chimney___ _____	23. 44	12. 82	0. 23

The first two experiments of this series have been already considered. The third, or iron, at 427° centig. was at a red heat, its temperature of luminosity in the dark being about 400°. This, therefore, and the subsequent part of the series are affected by the consideration that light was emitted, which materially alters the case, as we shall presently observe.

De la Roche infers from these experiments that a portion of simple radiant heat is transmitted directly in the way of radiation through glass, and that this increases as the temperature is raised.

A thick glass, though very transparent, stops heat more than a thin glass less so; the difference is less as the temperature is raised.

A portion of the heat having been intercepted by one screen, a proportionally much less diminution is caused by the introduction of a second; hence he infers that the rays emitted of a hot body are of several kinds, possessing different degrees of power to pass through glass.

He views the results, when the source of heat is raised to the temperature of luminosity, as forming one connected series with those below that point, and thus conceives a gradual advance in the radiant matter or agent from the state of simple heat towards that of light or "luminous heat."

6.) The theory adopted by De la Roche, as well as by Biot (*Traité de Phys.*, iv, 640) and Leslie, is that of one simple agent, which, as the temperature of the source is raised, is gradually brought more into the state of light, which on absorption is reconverted into heat. At low temperatures it is wholly or nearly all stopped by transparent screens. At increasing intensities more of it is enabled to pass in the way of direct radiation.

In order to establish this theory, it would be necessary to show that whatever may be the particular law of relation to the surfaces of bodies by which the action of the "igneous fluid" is determined at any stage of its evolution, the portion transmitted by a screen should act upon any two given surfaces in precisely the *same ratio* as the part intercepted, or as the whole.   Such a ratio will obviously differ at different stages of incandescence or inflammation; but at the same stage it ought to be found exactly the same—only diminished in the actual magnitude of its terms when the glass screen is interposed, as when there is none.

But no such experimental proof had been offered by any of the experimenters before named.   It was obviously called for to support or refute their theory, and was capable of being easily supplied by experiment.   That the conclusion is not a necessary one will be evident by merely observing that the phenomena may just as well be explained by supposing two distinct heating influences, one associated in some very close way with the rays of light, carried, as it were, by them through a glass screen without heating it; the other being merely simple radiant heat stopped by the screen, exactly as in the case of a non-luminous hot body.

To ascertain by experiment which of these suppositions was the true one, was the object of an inquiry which I communicated to the Royal Society, and which is published in the *Phil. Trans.*, 1825, Part I, p. 187.   I also gave an abstract of the results, accompanied by other illustrative remarks, and some theoretical views in a paper in the *Quarterly Journal of Science*, No. XIX, p. 45.   Some remarks also on the experiments are made in the *Edinb. Journ. of Science*, N. S. No. VI, p. 304.

These experiments combine the examination of the effect of *screens* with those of *surfaces*.   It is assumed, on the authority of previous experiments, that *simple* heat affects a thermometer in proportion to the *absorptive* nature of its surface: for example, a surface washed with a paste of chalk is rather more absorptive than one coated with Indian ink; and this kind of heat is *stopped* by transparent screens of ordinary thickness.   It would seem, from some experiments already mentioned, that from *luminous* hot bodies the effect is greater in reference to the *darkness of color* of the surface, and is *transmitted* through glass.   But when a body is heated to luminosity, how does this change in its properties take place?   Are its relations gradually altered *in themselves?* or are there *two sorts* of heating effect emanating from it at the same time?   These are the questions which my experiments were directed to answer, and the mode of trying the point is extremely simple; it is only to ascertain whether of the total heating effect from a luminous hot body, the portion intercepted by a transparent screen is of the same nature as, or different from, the part transmitted, in its relation to the surfaces on which it acts.

The experiments were conducted simply by having two thermometers, one coated with *smooth black*, the other with *absorptive white*, observing the ratio of the effects when they were exposed together

to the direct influence of a luminous hot body, and comparing it with the ratio similarly observed when a glass screen was interposed.

The screen acquiring, and therefore radiating, heat from the first moment of the experiment, will affect the thermometers in a ratio (as before observed) differing little from equality; and these equal quantities added to the terms of the ratio of the direct effects of the luminous body, will, of course, diminish the inequality of that ratio. This cause of error may not have operated to any great degree, but its *tendency* is obviously to a *diminution* of the *ratio*.

Notwithstanding this, the observed result in all cases with a lamp, or with iron raised to a bright red heat, was, that the *ratio* of the effect on the black to that on the white thermometer was *increased* by the interposition of the *screen*.

A summary of the results of two sets of experiments, (conducted with some slight variation,) and in the second of which the tempera‑ ture acquired by the screen was carefully noted, is as follows :

		Rise of thermometer (centig.) in 1 min.			
		Glass screen.		No screen.	
		White	Black.	White.	Black.
Iron bright hot	(1.)	$1^\circ.25$	$2^\circ.75$	$7^\circ.0$	$8^\circ.75$
	(2.)	0 .6	1 .25	2 .95	3 .75
Argand lamp	(1.)	0 .6	2 .0	1 .8	3 .4
	(2.)	1 .3	2 .35	2 .35	3 .2

These numbers are the means of several repetitions.

The necessary conclusion from this difference in the ratio of the direct and screened effects is, that the portion of heat which has the property of *permeating* the screen, has also the property of affecting the two surfaces in a *ratio different* from that in which the part *inter‑ cepted* acts upon them.

As in researches of this kind great numerical precision is unattain‑ able, I was especially, at every step of the inquiry, anxious to devise as many variations of the experiment as possible; these all tended to confirm the results just given.

Thus I used a large differential thermometer having its bulbs dif‑ ferently coated, and exposed each of them in turn to the luminous source of heat, the other being completely screened, and invariably found the ratio of the effects on the black and white bulbs considerably greater when affected only by the transmissible part of the heat than when exposed to the whole. As before, the part added on the remo‑ val of the screen was of a nature tending to add to the terms of the former ratio quantities in a ratio much nearer equality, viz: that which the effects of simple radiant heat would give when acting respectively on the two bulbs.

Other variations of the fundamental experiment were as follows:

A differential thermometer having one bulb black was exposed to the radiation from luminous hot bodies, first with and then without the interposition of a glass screen, the same position being preserved.

If the screen had no influence, it is evident that in whatever pro‑

portion the radiant matter affects the two bulbs, if it be of *one* simple kind, the only difference on removing the screen will be that its *intensity* will be increased, but will act on the two bulbs in the same proportion as before. Consequently an increase of effect, or motion of the liquid in the tube in the *same direction* as before, must take place.

In various experiments of this kind, after using several precautions against the influence of the screen, I *never found* an *increase*, and *generally* a decrease; that is, the action on the other bulb was now increased, or the portion of heat before intercepted and now admitted *has a different relation to surfaces* from that transmitted.—(*Quarterly Journal of Science*, xix, p. 45.)

Similar experiments were tried with the two bulbs in a direct line from the hot body, each placed nearest alternately, with and without a screen. The difference of ratios in the two cases was very striking. —(*Annals of Phil.*, June, 1825, p. 401. See, also, *Edinburg Journal of Science*, No. IV, 323.)

Upon the whole, the unavoidable conclusion is, that if the total direct effect were the result of one simple agent, the intervention of the glass would, by intercepting some portion of it, produce no other alteration than a diminution of *intensity;* the *ratio* of the two effects would remain unchanged. But the reverse being the case, it follows that there are *two distinct* agents or species of heat acting together.

Upon combining these results with those of previous experimenters, we are led to the following general statement of the case:

When a body is heated, at lower temperatures, it gives off radiant heat *stopped* entirely by the most transparent *glass*, and affecting bodies in proportion to the *absorptive texture* of their surfaces.

At *all higher temperatures* it continues to give off such radiant heat, distinguished by exactly the same properties.

At a certain temperature it begins to give out *light;* precisely at this point it begins also to exercise another heating power distinct from the former; this is capable of direct *transmission through glass,* and affects bodies in proportion to their *darkness of color.*

This second species appears to agree with what the French philosophers have called "calorique lumineux," or the "igneous fluid" of Professor Leslie, but they seem to have considered it as constituting the entire effect.

The distinction thus established easily applies to the explanation of De la Roche's results, before stated. On inspection, it appears that the numbers in the column belonging to the blackened screen are almost exactly in the same ratio to the first or direct effect throughout the whole series.

Upon the principle here laid down, the effects with the *blackened* screen would be those arising from the absorption and subsequent radiation of both species of heat; these in each instance being absorbed in the proportions in which they existed in the original radiation, produce a secondary effect proportional to the primary.

The effect with the *transparent* screen does not follow any proportion to the primary; and this is explicable as due to the glass inter-

cepting the one kind of heat which follows no proportion to the other, this last being wholly transmitted. Also by comparison of the latter experiments with the two first of the series, it is probable that, throughout, a certain degree of heat was in this case also absorbed and radiated again by the screen.

The existence of this distinction, and the proportion between the two species of heat in the radiation from different sources, as various kinds of flame, metal at successive stages of incandescence, &c., afford many topics of inquiry, on some of which I attempted some rough determinations, confessedly very imperfect.—(*Annals of Phil.*, N. S. liii. 359; liv, 401.) The distinction applies to some results of Mr. Brande on the flames of different gases, (*Phil. Trans.*, 1820, Part I, p. 22,) and of Count Rumford on increased intensity of combustion and on the coalescing of several flames.—(*Essays*, i, 304.)

7.) Melloni states (*Ann. de Chim.*, December, 1831, p. 385,) that by using his thermo-multiplier he has found the permeability of transparent bodies to *heat* to be also dependent on their *refractive power*. He has compared twenty such media, and finds the order of permeability constantly the same, whatever be the *temperature* of the source. Chloruret of sulphur has the greatest power, oil next, and water least; he exposed them to the rays of a candle, an Argand lamp, or the sun. He finds the differences of permeability less, the higher the temperature. The full account is promised in another memoir.

All this obviously applies only to *luminous* hot bodies.

MM. Melloni and Nobili, in their former paper, (*Annales de Chimie*, October, 1831, p. 211,) also speak of the heat from phosphorus having been by these means found sensible, though it is often supposed to give light without heat.

8.) For information on various points connected with the subject, and on the theories of the evolution of light and heat, the following references may be useful:

Wedgewood (*Phil. Trans.*, 1792, p. 28) thinks that light from attrition is produced by a heat of from 400° to 600° Fahrenheit.

Dizé on Heat as the Cause of Shining.—(*Journ. de Phys.*, xlix, 177. Gilbert, *Ann.* iv, 410.)

Fordyce on Light from Inflammation.—(*Phil. Trans.*, 1776, p. 504. Morgan, *Phil. Trans.*, 1785, p. 190. M. Hermstaedt, *Nicholson's Quarto Journal*, v, 187.)

Mr. Davies on Flame.—(*Annals of Phil.*, December, 1825.)

Mr. Deuchar on Flame.—(*Edinb. Phil. Journal*, iv, 374.)

M. Seguin on Heat and Motion, &c.—(*Edinb. Journ. of Science*, xx, 280.)

DIVISION III.

HEAT OF THE SUN'S RAYS.

Speaking according to our ordinary sensations, we are accustomed to say that the sun communicates both light and heat. Light is transmitted in a way which we term radiation. The heat from non-lumi

nous hot bodies is transmitted to a distance in a way closely analogous, and to which the same name has been applied.

In the first instance, we might suppose that the sun sends out two separate emanations—one of light, and another distinct from it, and similar to that of radiant heat from a mass of hot water; and this, perhaps, was the first view taken of the subject, though a confused idea of some very close and intimate connexion subsisting between the solar light and heat appears to have prevailed.

This subject, as might naturally be expected, attracted the early notice of experimenters. A very slight examination sufficed to show that the rays of solar heat, (whatever their nature might be,) differed essentially in many properties from those of terrestrial heat, whether radiated from luminous or non-luminous bodies. Whether there existed a separate set of heating rays distinct from those of light, and at the same time differing in many respects from rays of terrestrial heat; or whether these differences depended on some unknown property of the rays of light, was a question which for a long time remained without any direct investigation, and on which even now we have, perhaps, no very precise ideas.

I.—Solar rays in their natural state.

a.) *Nature of radiation.*

1.) The solar heat is transmitted through the air without heating it. It invariably accompanies the light.

Scheele conceived that the sun's rays of *light* produced heat not when in motion but when stopped by the interposition of solid bodies. —(On Air and Fire, &c.)

Mr. Melville seems to have adopted nearly the same theory, and to have conceived reflection at an opaque surface to be the cause of an excitation of heat from the sun's rays.—(Evans on the Calorific Rays, &c., *Phil. Mag.*, June, 1815.)

In general, for light of the same composition the heat appears nearly proportional to the illuminating intensity.

2.) Measures of radiation.

Theory of the sensibility of thermometers especially for experiments of this kind.—(Sir W. Herschel, *Phil. Trans.*, 1800, Note, p. 447.)

Leslie contends for the exact proportionality of intensity of light and heating power.—(*Inquiry*, pp. 160 and 408.)

Theory and construction of his "Photometer," ch. xix, p. 403.

Ritchie's "Photometer," of the same kind.—(*Phil. Trans.*, 1825, Part I, p. 141.) See his Remarks on Leslie's Photometer, *Edinb. Journ. of Science*, No. IV, 321, and V, 104.

Mr. Daniell, in his work on Meteorology, has collected a great number of observations on the heating power of the sun's rays in different latitudes from the polar to the equatorial regions. Most of these observations were made by comparing two thermometers, one of which was kept in the shade, whilst the other, having its bulb

blackened, was exposed to the direct rays of the sun; but, as Dr.
Ritchie observes, no correction seems to have been made for the
variable causes which abstract caloric from the blackened ball of the
exposed thermometer.—(*Edinb. Journ. of Science*, v, 107.)

In the same paper is described the method proposed by Sir J. F.
W. Herschel; his object was to ascertain, by direct experiment, the
relative heating power of the sun's rays; this he did by exposing in
a glass vessel, or large thermometer, at different times and places,
a deep blue liquid, for a given time, to the direct rays of the sun,
noting the increase of temperature, which was purposely rendered
very small by properly adjusting the capacity of the instrument, then
shading the sun's direct rays, and leaving it exposed for an equal
time to the free influence of all the other heating and cooling causes,
radiation, conduction, wind, &c., and again noting the effect of these.
The same difference of these, according to their signs, was the effect
of the mere solar radiation. Dividing this by the time of exposure,
he had the momentary effect or differential co-efficient, which is the
true measure of the intensity of radiation.

Professor Cumming has been engaged in researches, the object of
which was to obtain a measure of the total heating effect of the sun's
rays.   He has communicated for this report an account of his inves-
tigations, of which the following is the substance.

His instrument consists of a bent tube in the form ∩, one side ter-
minating in a black bulb containing ether, or sulphuret of carbon; the
other a graduated tube closed at the bottom; into this, on exposure
to the sun, some of the liquid is distilled over from the bulb; and the
quantity measured on the scale is proportional to the amount of radia-
tion, when all interfering causes are allowed for; and these are esti-
mated by comparative observations.

The experiments have been varied by exposing the bulb and
screening the other part, or by exposing the whole instrument equally
to the sun; and by making contemporaneous observations with the in-
strument wholly uncovered, or covered totally or partially by a glass
to protect it from currents of air.

The Professor has endeavored to make a standard scale by regis-
tering the sun's radiation on clear days every half hour, or hour, in
the usual manner, and comparing them with the contemporary distil-
lation; or by placing the two sides of the instrument in two vessels of
water at unequal temperatures, and noting the distillations in given
times by ascertained differences of temperature.

The instrument is filled with ether in the same manner as Wollas-
ton's Cryophorus (from which the suggestion was taken;) but there
is an inconvenience, arising from the circumstance of the difference
of pressure under which the instrument is hermetically sealed, which
renders two instruments not strictly comparable; this he proposes to
remedy by sealing a standard instrument when exhausted to a known
pressure by the air pump.

The ether or sulphuret of carbon employed must be perfectly *pure*,
or there is a re-absorption.   The circumstance of being exposed to
the air, or covered, makes great differences in the indications;

especially in windy weather. To avoid an inconveniently long scale, there should be two instruments constructed, one for winter and the other for summer. The Professor has kept for nearly a year a register of *sunshine*.

b.) *Reflection of solar heat.*

1.) It takes place exactly by the same laws as that of the light.
The heat is collected in the focus of concave reflectors along with the light.

2.) The sun's rays reflected from the moon are probably much too feeble to allow of any heat being made sensible.

Dr. Howard, however, states that, with a peculiar differential thermometer, he has obtained an effect.—(*Silliman's American Journal*, vol. ii, 329.)

MM. Melloni and Nobili (with the apparatus before described) tried to detect heat in the moon's rays, but without success; they mention, however, that terrestrial radiation interferes greatly with such experiments, and do not describe fully their contrivances for obviating this cause of error.—(*Ann. de Chimie*, Oct., 1831, p. 210.)

3.) Berard (memoir before cited) tried the *polarization* of the solar heat; that is, polarized the sun's light; and in the position of non-reflection found that the heat had disappeared with it.—(See *Edinb. Journ. of Science*, vi, 297.)

c.) *Under this head nothing known.*

d.) *Effect of surface on the absorption of solar heat.*

1.) I am not aware of any experiments directly showing how far the same relation to the *texture* of surfaces which has been found in absorption of simple heat may hold good in regard to the sun's rays. But for surfaces of the *same texture* it has been incontrovertibly established that the effect in this case increases in proportion to the *darkness of color*, or in proportion to the absorption of *light;* and it would seem most probable that this relation is the only one which really holds good, the texture of the surface being *probably* quite indifferent except so far as it tends to the better absorption of the *light*.

2.) Among the earliest experiments on the subject, if not actually the first, were those of Mr. Boyle, on the different degrees of heat communicated by the sun to black, white, and red colored surfaces.

He caused a large block of black marble to be ground into the form of a spherical concave speculum, and found that the sun's rays reflected from it were far from being too powerful for his eyes, as would have been the case had it been of any other color; and although its size was considerable, yet he could not set a piece of wood on fire with it, whereas a far less speculum of the same form, made out of a more reflecting substance, would presently have made it inflame.

It was remarked by Scheele that the thermometer, when filled with alcohol of a deep red color, rose more rapidly when exposed to the

sun's rays than another filled with the same kind of spirit uncolored; but that the fluid rose equally in both when dipped together into the same vessel of warm water.—(On Air and Fire, &c.)

Dr. Franklin found that the hand, when applied alternately to a black and to a white part of his dress in the sun, would feel a great difference in their warmth.

He observed that black paper was sooner fired by exposure to the focus of a lens than white.

His well known experiment of placing differently colored pieces of cloth on the snow in the sun, and observing them sink deeper in proportion to the darkness of color, was first suggested by Dr. Hooke.

3.) Cavallo observed that a thermometer, with its bulb blackened, stands higher than one which had its bulb clear when exposed to the light of the sun, or even of the clouds.—(*Phil. Trans.*, 1780.)

Pictet made a similar observation, observing that when the two thermometers remained for some time in a dark place they acquired precisely the same height. He also found that when they had both been raised to a certain point, the clean one fell much faster than the coated one.—(*Sur le Feu*, ch. iv. Thomson, i, 126.) This last statement is so contrary to all other experiments that we must suppose some mistake.

De Saussure received the sun's rays into a box lined with charred cork, containing a thermometer with a glass front; it rose in a few minutes to 221°, when the temperature of the air was 75°.—(*Voyages*, ii, 932.)

Professor Robison, in a similar experiment, employed three vessels of flint glass within each other at one-third of an inch distance, set on a base of charred cork, and placed on down in a pasteboard cylinder; the thermometer within, in clear sunshine, rose to 230°, and once to 237°.—(Black's *Lect.* i, 547. Thomson, i, 127.)

Sir H. Davy took several small disks of copper of equal weight, size, and figure, on one side painted respectively white, yellow, red, green, blue, and black. A mixture of oil and wax, which became liquid at a temperature of 76° Fahr., was attached to the other surface of each disk; and on exposing the colored surfaces together to the sun's rays, the length of time elapsed before the mixture on each began to affected was in the order in which they are above enumerated.—(Beddoe's *Medical Contributions*, p. 44.)

4.) The experiments of Sir E. Home (*Phil. Trans.*, 1821, Part I,) are particularly deserving of attention, as exhibiting what might at first sight be considered an exception to the above remarks a greater effect being produced in some instances on a white than on a black surface. A more attentive examination, however, will show us that these experiments prove thus much: The heat occasioned by the rays of the sun when received directly, or when in some degree intercepted, as by thin white cloth, on the skin, is greater than that communicated by conduction to the same skin through a black cloth in contact with it, which is itself, in the first instance, heated by absorbing the rays.

He observes, also, that a white skin is scorched, and that of a negro

is not, in 10 minutes, by the direct rays of the sun; that is, as before, the outer coat of the skin allows some of the direct rays to pass through and affect the sentient substance beneath; whereas, in the case of the black, the rays are absorbed and converted into heat of temperature, which diffuses itself equally, and does not produce the effect of scorching.

5.) The most singular facts connected with the absorption of the sun's rays, are those exhibited by the substances called "phosphori" or "pyrophori."—(Thomson's *Chem.* i, 17.)

The general fact is, that after exposure to the sun, on being removed into the dark they give out light, but it is after a time exhausted; it is given out more copiously and exhausted sooner if *heat* be applied. Many solar phosphori will always emit light of one color only, to whatever colored ray they may have been exposed. In a short notice given by Dr. Young, in his valuable Catalogue of Authors, it appears that M. Grosser found that such phosphori as emitted red light only were made to shine most by exposure to blue light.—(*Rozier*, xx, 270.)

Beccari, in a memoir "de Phosphoris," extracted in the *Phil. Trans.*, 1746, p. 81, gives as one of his results, that the light emitted was brightest when the surface of the mass was of a rough texture; those which were smooth and polished retained little or none, but (supposing the color the same) a rougher surface would evidently *absorb* more light than a smooth one, and therefore might emit more.

Mr. T. Wedgewood compared two pieces of phosphorescent marble, one naked, the other painted black; on applying uniform heat the coated marble gave out no light, though the other did.—(*Phil. Trans.*, 1792.)

But the coating increased the radiating power, and it therefore probably did not retain heat enough to cause the extrication of light.

Mr. Morgan, (*Phil. Trans.*, 1785,) after examining many of the phenomena of phosphorescence, generalizes his views by maintaining that all phosphori emit light, proceeding in order from violet to red, in proportion as the process is effected by the application of an increasing degree of heat.

This is a very curious subject, as connected with the whole theory of the relations of light and heat. Some valuable information might probably be obtained as to the degree of heat necessary, and whether there is any loss of heat when light is evolved, compared with cases when no light is evolved; as there should be, on the hypothesis of conversion of heat into light, or on that of heat becoming latent in the light.

In Mr. Wedgewood's paper, above cited, is an account of the principal researches on the subject.

e.) *Effect of screens.*

1.) That no diminution of the effect of the sun's rays on a blackened thermometer is occasioned by a transparent screen was remarked by several experimenters, particularly De la Roche.—(*Biot*, iv, 611.)

2.) I tried the point by two thermometers (as in the case of terres-
trial heat) and found *no perceptible difference* in the *ratio*, with and
without the screen, of the black and white thermometers.—(*Annals of
Phil.*, xli, 321.)

The same result was found with a differential thermometer, with a
glass screen over the bulb, which was not blackened. No difference
was observable between the indication under these circumstances,
and when both were exposed.—(*Annals of Phil.*, xlii, 401.)

Hence, I think we are entitled to conclude that there does not exist
in the solar beam, in its natural state, any *simple radiant heat*, (as
before defined;) but that the whole emanation consists of the other
species, distinguished by the two characteristics of affecting sub-
stances with heat in proportion to the *darkness* of their color, and
being wholly *transmissible* through *glass* without heating it, and in-
separable from the rays of light.

This applies to the rays of the sun which come within the reach of
our examination. It must, however, be admitted, as by no means
improbable, that the sun may *originally give out* a separate radiation
of *simple heat*. None of this kind *reaches us;* but we must consider
the very different degree in which any medium, as air, absorbs or
intercepts the passage of those two sorts of radiant agents. The *heat*
from a hot body will not be perceptible at a short distance, while its
light will traverse an amazing extent of length; and thus at different
distances the ratio between the two sorts of heating effect will be
very different. Some degree of *simple heat*, therefore, may actually
be initially radiated by the sun and be lost before it reaches us. We
do not know that there is any medium between the different parts of
the solar system capable of absorbing heat. The highest regions of
our atmosphere into which observation has penetrated are uniformly
the coldest; but they are known to have a greater capacity for heat.
Thus, though it is possible that some heat may reach to that distance
and be absorbed without becoming sensible to us, its quantity must
be very small; if, therefore, we suppose any simple heat to be initially
radiated from the sun, it must be all, or nearly all, absorbed by some
parts or appendages of that luminary exterior to the part where it
is generated.

3.) The concentration of the sun's heat *by a lens* is a familiar experi-
ment.

Sir W. Herschel (*Phil. Trans.*, 1800, Exp. 23) concludes that there
is a focus of greatest heat farther from the lens than that of light;
sealing-wax was scorched in the same time when in the luminous
focus, and at half an inch further from the lens; this affords no proof
of its being separated from the light.

That the heat is found to accompany the rays of light in the most
constant and inseparable manner through various refractions, as in
the instance of the four lenses in the eye-piece of a telescope after
reflection, is also remarked by Sir W. Herschel, (*Phil. Trans.*, 1800,
Exp. 11.)

II.—Solar rays subjected to analysis by the prism.

1.) The different heating powers belonging to different parts of the spectrum were probably first observed by the Abbé Rochon.—(*Phil. Mag.*, June, 1815; and Biot, *Traité de Phys.*, iv., 600.) He found the maximum in the yellow-orange rays: the prism was of flint glass: his thermometer was filled with spirits, probably therefore tinged *red;* this may account for his result.

I tried some experiments with the bulb of the thermometer painted *red*, which appeared to agree with his result.—(*Annals of Phil.*, li, 201.)

Professor Leslie applied his "photometer" to these experiments.— (*Inquiry*, p. 454.)

Dr. Hutton observed the different heating powers, and that they are not proportional to the illuminating.—(*Diss. on Light and Heat*, p. 38.)

Landriani found the maximum in the yellow rays, as also did Senebier.—(Volta, *Lettere, &c.*, 136.)

Berard (*Mém d'Arcueil*, iii; *Ann. de Chimie*, lxxxv, 309) repeated the experiment with a heliostat. He found the maximum *in* the red, but some heat beyond. He repeated the experiment in both the spectra formed by Iceland spar.

2.) Sir W. Herschel (*Phil. Trans.*, 1800, Part II) first observed the maximum of heat beyond the red end of the visible spectrum, and considered the effect as due to essentially invisible rays of a separate kind from those of light.

Yet he found them subject to the same laws of refraction, and their dispersion corrected by another prism: they were concentrated by a lens (*Ibid.*, p. 317,) and by reflection (pp. 298, 302.)

Leslie objects to the conclusion of invisible rays, and tries to account for it as owing to an optical cause.—(*Inquiry*, Note, p. 559; see also *Nicholson's Journal*, 4to, iv, 344 and 416.)

Sir H. Englefield (*Nicholson's Journal*, iii, 125,) found heat beyond the visible red; it does not appear whether it was there at a maximum: the rays were such as to be concentrated by a lens, and he compared the effects on a black and white bulb. The exterior effect on the white bulb was in a much less ratio to that within the visible spectrum than on the black.

Sir H. Davy repeated these experiments in the clear atmosphere of Italy, and with thermometers of extremely minute size, to secure an instantaneous effect: he found the maximum beyond the red.

These experiments were also tried by Ritter and by Professor Wünsch (*Magazin der Gesellsch, &c.*, Berlin, 1807.) He used *prisms of different substances;* with alcohol, oil of turpentine, and water, the maximum was in the yellow; with green glass in the red; and with yellow glass on the extreme boundary.

3.) But by far the most important and conclusive researches on this subject are those of Dr. Seebeck, who in a memoir read to the Royal Academy of Berlin, after discussing the conclusions and views of previous experimenters, proceeds to an elaborate series of experiments

of his own, in which he has discovered the cause of all their discrep-
ancies. The position of the maximum heat in the spectrum depends
entirely on the nature of the medium employed—a circumstance
almost wholly unnoticed by former experimenters.

The heating intensity is very small towards the violet extremity;
it thence gradually increases in prisms of water, alcohol, or oil of tur-
pentine; the maximum is in the yellow space: in those of solution of
sal-ammoniac and corrosive sublimate, or sulphuric acid, it is in the
orange; in crown glass and common white glass, in the middle of the
red; in those glasses which contain much lead, it is in the limit of the
red; and in flint glass, beyond the visible boundary, but nearer to it
with Bohemian than with English glass. In all cases it gradually
diminishes from the maximum, and is perceptible to some distance
beyond the visible boundary.—(Schweigger's *Neues. Journ.*, x, 129.
Annals of Phil., Sept., 1824; *Abhandl. der Königk. Acad. Wissenschaf-
ten in Berlin*, 1818–'19, *p.* 305; *Phil. Mag.*, Nov. and Dec., 1825;
Edinb. Journ. of Science, No. II, 358.)

4.) Analysis of the solar rays by the absorption of media.

In respect to light, the remarkable variety in the absorption of dif-
ferent rays exhibited by different media has been well established,
and affords a new sort of analysis of light.

In regard to the solar heat, similar researches have been made,
though as yet to little extent. The first observations of the kind
were those of Sir W. Herschel (*Phil. Trans.*, 1800.) He found the
absorption of several kinds of glass for his invisible rays and for the
middle red to be proportional to the following numbers out of 1,000
rays incident:

	Invisible rays.	Red rays.
Flint glass	000	143
Coach glass	143	200
Crown glass	182	294
Dark red glass	000	692

5.) Sir D. Brewster has lately been engaged in some researches on
this subject, an abstract of which he has kindly communicated in
manuscript for this report. Agreeably to the view he has established
of the solar prismatic spectrum as consisting of spectra of *three* primary
colors *superposed*, and having their *maxima* at different points, he re-
gards the *heating power* as due, in like manner, to another primary
spectrum superposed in the same way, and similarly the chemical rays.
He makes the following statements with respect to the heating rays:

1st. There is no proof whatever of the existence of *invisible rays*
of any kind beyond the red or the blue extremity of the spectrum.
Sir W. Herschel's experiments prove the existence of *heat* beyond
the *visible* extremity *of the spectrum which he used*, but Sir D. Brewster
has succeeded in rendering the spectrum *visible* at every point where
any heat was produced.

By particular processes he has traced the light at that end greatly
beyond the place where Frauenhofer makes the spectrum terminate.

The same he considers established in regard to the blue end of the spectrum and of the deoxidizing rays. He thinks it extremely probable that the heating and illuminating rays are different rays, but they have never yet been found in a state of complete separation.

2d. Until it is proved, therefore, or rendered probable, that the same intensity of light of different colors, *as it proceeds directly from the sun*, is accompanied with different degrees of heat, we must assume it as true that the heating power is proportional to the illuminating power of the different rays of solar light.

3d. It appears from Dr. Seebeck's experiments on the water spectrum, that this relation holds generally in it, as he found the *maximum of heat* to be in the yellow rays, or *coincident* with the *maximum of light*. Hence Sir D. Brewster draws the important conclusion that *water has the same degree of transparency for the solar heating rays that it has for light*, which is the same as all *colorless* transparent media have for light; that is, water absorbs equally all the different rays of solar heat in the same manner as it does all the different rays of solar light.

4th. It has been found by experiment that with prisms of *crown glass* the maximum heating effect is in the *middle of the red space*. Unfortunately the relation between the *maximum heat* in the water spectrum and in the crown glass spectrum has not been ascertained. If we suppose them equal, it appears that the crown glass must have exercised *a greater absorptive action* than the water upon the more *refrangible rays*, and *a less* absorptive action upon *the less refrangible rays*, in the same manner as is done by red glasses upon light.

A prism of sulphuric acid gives the maximum ordinate of heat in the *orange* space, or the fluid absorbs more of the red rays than crown glass, and less of the rays on the other side of the orange.

In flint glass, where the maximum heat is at the very *extremity* of the spectrum, scarcely any of the red rays are absorbed, while great proportions of all the others are.

Dr. Turner (*Chem.*, p. 84, 3d edit.) says that it is difficult to account for Seebeck's results, without supposing that different media differ in their power of refracting caloric, (*i. e.*, the heating rays of the sun.)

Sir D. Brewster considers that the true explanation is that which the above principles afford, viz: that colorless transparent bodies, in acting upon the solar heat, exercise the same sort of absorptive action upon it that colored transparent bodies do upon light, the maximum ordinate shifting its position with the nature of the body. Colored media give sometimes two or more maxima of light, with large spaces and small lines entirely defective of light, in consequence of the absorption being total at those places.

In like manner he is persuaded it will be found that there are defective spaces and lines in the spectrum of solar heat; these he thinks may possibly be detected by using as thermometers the minute natural cavities in topaz, &c., filled with fluid or vapor, and not more than 0.001 inch in magnitude.

5th. These views are exactly accordant with the results of Sir W. Herschel above stated.

They are equally consistent with the facts, whether the curve of heat terminate abruptly at the extremity of the red space, or continue beyond the visible spectrum.

Sir D. Brewster has, by particular methods of condensation, succeeded in detecting both heat and light at considerable distance beyond the maximum of heat with a flint glass prism, that is, rays undergoing very little refraction.

He considers it highly probable that the deoxidizing rays will be found to be subject to the same laws of absorption as those of heat and light; the media we commonly use may absorb them copiously, whilst others may be found which may transmit them more abundantly.

Similarly with the magnetizing rays. And thus we may account for the contradictory results hitherto obtained on this point by supposing that some ingredient rendered one prism absorptive of these rays, and another not so.

6th. Sir D. Brewster extends these views to the analogies between solar and terrestrial heat.

He considers those rays of the solar spectrum just mentioned, which undergo little refraction, to be analogous to those thrown off by bodies slightly heated. The waves of heat are broad and slow in their motion; as the temperature is raised they are thrown off with more velocity, and become smaller and suffer a greater refraction. When the velocity is such as to give them a refraction equal to that of the red rays, then red light is produced; and successively the other colors are added, till at a very high temperature white light is radiated.

He proposes to examine what transparent body transmits most heat, and, by converting it into a lens, expects to find a series of foci at different distances, beginning from that of the violet rays to that of those corresponding to rays of very little refrangibility.

7th. He applies these views as affording an explanation of De la Roche's result before mentioned, viz: that a second screen intercepts a much smaller proportion of the heat after passing a first than the first did of the whole effect. This De la Roche ascribed to something analogous to polarization.

On the principle just stated the explanation is very simple. The first plate intercepts those rays which it has a tendency to absorb and transmits the rest; the second, being of the same kind, of course will transmit these with scarcely any further diminution.

He observes that thick masses of colorless fluid or of glass transmit scarcely any radiant heat in a way analogous to that in which thick masses of colored glass are opaque to all rays of light.

He conceives that substances may be found which are *opaque* to *light*, and yet *transparent* to *heat*. These should be carefully sought for, as they would be of great practical value. Red glass, for example, which scarcely transmits any light, or one ray in 2,000, transmits all the invisible rays of Herschel, 692 of the 1,000 red rays, 606 rays out of 1,000 of solar heat, and 630 of "culinary" heat, according to Sir W. Herschel. We may expect, therefore, to find an opaque metallic glass, or thin plate of metal, which, though quite opaque for light, may transmit heat copiously.

Sir D. Brewster considers Sir W. Herschel's experiment on the refraction of "culinary" heat by lenses to be very unsatisfactory, as before noticed. He recommends a lens composed of zones, so as to have no greater thickness in the middle than towards the edges, a construction which he has described in his "Optics," p. 322, (*Cabinet Encyclopedia,*) and made of glass, which unites the highest refractive power with the smallest absorptive power for heat.

It is also important to find, as sources of heat, bodies which do not become luminous till at extremely high temperatures.

6.) The researches of M. Melloni have also been extended to this part of the subject.—(*Annales de Chimie,* December, 1831, p. 388.)

From known observations on the spectrum, he remarks that there exists, on opposite sides of the maximum, *isothermal* points—one in a colored part, the other without the red end of the spectrum.

On causing the different rays to pass through a plate of water, and noting the effect on the thermo-multiplier, the heat of the violet ray was undiminished, but its isothermal totally intercepted.

That of the indigo slightly diminished; its isothermal not totally intercepted.

Proceeding in this way with the other rays, he found in general that the portions of heating power intercepted in the colored rays, and those which are transmitted in their isothermal rays, increase in proportion as they approach the position of the maximum, where, of course, upon the whole, the interception is greatest; or, in other words, the rays of the *calorific* spectrum undergo an interception by water in proportion as their refrangibility is less.

He gives a table of the numerical results. He views his results as precisely according with and explaining those of Seebeck. With a water prism the heating orange and red rays are more intercepted than the yellow; in this, therefore, the maximum appears.

Conclusion.

We have thus far taken as close a survey as is consistent with the limits of a report like the present, of the successive and varied researches which have been made with the view of tracing the laws of radiant heat. In the present state of our knowledge it must, upon the whole, be avowed that we have little to contemplate but an assemblage of facts, or alleged facts, determined with more or less accuracy; few, indeed, with any great precision—many resting upon very vague evidence, and in several instances the results of different observers exhibiting a wide discrepancy, or even direct contradiction; whilst, with very few exceptions, any general laws can hardly be said to be established with that certainty which can substantiate their claim to be received as legitimate physical theories.

In offering suggestions for the advance and improvement of this branch of science, the first and most essential point to which attention ought to be directed is the improvement, or rather invention, of the means of obtaining accurate indications of radiant heat down to its most minute and feeble effects. In reference to this point good

determinations are much wanted of the degree to which the expansion of the bulb influences the accuracy of air thermometers. The improvement of mercurial thermometers so as to produce an instrument of extreme sensibility to the minutest effects of heat, is an object the attainment of which would probably be more important than that of any other means for accomplishing the end in view. But other methods, founded on good principles, should be diligently sought for and tried; for example, it might be matter of inquiry whether we could render available to this purpose the incipient melting or softening of some substances by a very slight increase of heat, or the evaporation of volatile liquids.

But it is more particularly desirable that the instrument of MM. Nobili and Melloni should be tried, and a precise examination set on foot of its real accuracy and the causes of error to whose influence it may be liable. This is the more necessary from the very remarkable character of many of their results, whilst the alleged sensibility of the instrument, as they describe it, is such as almost to exceed belief.

When we shall have succeeded in obtaining that prime requisite, an unexceptionable measure of minute effects of radiant heat, we may then proceed with some hopes of success to examine the points on which there at present prevails so wide a discrepancy between different experimenters.

The polarization of heat is, perhaps, the question which, of all others, requires the most extreme sensibility in our thermometer, or rather thermoscope, in order to its satisfactory determination. It may be tried, either directly, with the simple heat from non-luminous hot bodies, or with luminous sources, with and without a glass screen, comparing the total compound result with that due to the transmissible part, or heating power of light alone, and thence deducing the part due to simple heat. The main difficulty is that of *getting any indication at all*, after two reflections from plane surfaces.

Another point which requires further investigation is the apparent transmission of simple heat through *very thin transparent* screens, but *not* through *opaque*. This should be examined in connexion with the acute remark of MM. Nobili and Melloni, that a thin stratum of soot may retain its low *conducting power*, and thus intercept the effect. This, of itself, would form a subject for an accurate series of experiments, viz: whether the ratios of the *conducting powers* of substances remain the same for *all thicknesses*.

The very nature of the transmissive and interceptive powers of screens is little understood. Supposing simple heat transmitted without diminution, how far is the mode of such transmission analogous to that of light? what time is required for a body to commence radiating heat after it has begun to acquire it? whether it acquires it from a distant source *instantaneously?* how the heat distributes itself upon or through a screen? what is precisely the effect of a coating on one side of the screen in relation to the last question? upon what the singular exceptions and anomalies pointed out by Melloni and Nobili depend? whether any other such apparently anomalous cases can be found? These are a few of the most obvious questions which

arise out of the slightest survey of the present state of our knowledge, and on which accurate determinations are wanted before we can be said to possess even the elements of a scientific theory.

May it not be the law that if a body be placed in the rays from a source of heat it will be acquiring and giving out heat till the intensity of radiation at the points before and behind it resumes its original proportionality?

The *time* in which this takes place will depend on the *extent* of the body, *its thickness, its conducting power, its capacity for heat, and the state of both its surfaces.*

These may be such that the effect may be sensibly instantaneous, and the radiation therefor appear to go on without interruption. In this case, also, the distance of the screen from the source (within moderate limits) may make no sensible difference, though if any of the above circumstances retard the effect to a sensible amount, then there will be a difference with the variation of distance. In this way we may, as it were, regard the medium between the source and the thermometer as merely a compound, of which the screen is one portion and the air the other.

Another class of questions respecting which little, if anything, is accurately known may be put with regard to the modification (if any) which radiant heat may undergo in passing through small apertures. This will again be connected with the interceptive power of *net-work.* A very curious and delicate subject of inquiry is the *repulsion* exerted between heated bodies at sensible distances, of which a short notice is given in the *Quarterly Journal of Science,* xxxix, 164.

The *reflection* of heat has been little examined, except in the single case of its concentration by spherical reflectors; and here (according to Leslie) it is not brought to the same focus as light; this requires examination, as well as the simpler case of plane surfaces, and the proportion of heat reflected at different incidences. There will probably in all cases be a very large deduction to be made for the heat acquired by the reflector and radiated again.

But another class of such questions yet remains in connexion with that fundamental point which was the object of my first inquiries. The conclusion from my experiments, viz: that *luminous hot bodies* are sending forth at the same time *two distinct species of heat* distinguished by different properties, is the unavoidable conclusion from the experiments, depending on the mathematical truth, that if a ratio be altered by the addition or subtraction of quantities from its terms, the quantities added or subtracted must be in *a different ratio* from the original one. I here repeat this, because the nature of the reasoning has not been perceived by some persons. This conclusion undoubtedly introduces a complexity into the view we must take of the phenomena; whereas, if we were at liberty to adopt the simpler theory of De la Roche and others, many of the apparent anomalies would be reconciled. Hence the verification of my results becomes a point of considerable importance. If any experimenter with more accurate apparatus shall succeed in showing them to be erroneous, he will achieve

an important step towards simplifying the theory. In this instance again the improvement of the thermometer is a primary requisite.

I may here mention that I have recently had a more delicate apparatus made, with which I have repeated my former experiments still with the same result; it consists of two thermometers mounted together, as before described. They were contrived for me by Mr. Cary, so as to have very large degrees for a small part of the scale a little above ordinary temperature. 1° Fahrenheit occupies about half an inch; but the bulbs are large, which is unfavorable to the rapid communication of the effect. These experiments are of a very tedious nature to repeat with precision, owing to the necessity of waiting between each repetition for the thermometers to cool and become stationary.

But it should be observed that there is nothing in my results which contradicts the idea that simple heat may have in a very slight degree a power of transmissibility through glass; all I have assumed is, that it is sufficiently distinguishable in this respect from the heating power which accompanies the light, and which undergoes no diminution. Connected with these points, again, is the question, whether if simple heat can radiate *through* solid transparent media, it cannot also commence radiating IN them. It is commonly asserted that radiation can only take place, or commence, *in elastic media*. This, then, is an inquiry which will lead into a wide field of research, and may be found connected with the intimate nature of radiation. It will also be a question whether, and how far, radiant heat passes through elastic media without heating them, and what support this gives to Leslie's theory of pulsations. The whole subject should be viewed in connexion with the admirable remarks of Sir J. Herschel in his *Discourse on the Study of Natural Philosophy*, p. 205.

The radiation of heat *in vacuo* is another point on which further inquiry is much wanted. The greater capacity of air for heat, as it is more rarefied, would occasion a more rapid abstraction from the hot body; and thus in an atmosphere of extreme rarity the cooling ought to be extremely rapid, and this must be accurately estimated in measuring the radiation. But it appears from the experiments of Gay-Lussac, (see *Edinb. Phil. Journ.*, vi, 302,) that when air is reduced to the most extreme degree of rarefaction possible a very considerable compression makes so little difference in its actual density that the giving out of heat which ought to take place from diminishing its capacity is absolutely insensible.

But even in this case it is very questionable whether so complete an approach to a real vacuum is obtained as to warrant inference respecting the radiation of heat in an actual vacuum.

In fact, we want a connected series of determinations to show the order and increase of conducting powers, as connected both with the radiation in and through different media, and the interception which they offer to its passage.

In solids *it is presumed* no radiation can *commence;* it is disputed whether it can *continue* even partially; but conduction goes on rapidly

In liquids it has been disputed whether there can be radiation; and they are worse conductors than solids.

In elastic media radiation can commence and continue; but they are still worse conductors.

In vacuo it might be presumed by analogy that a yet more free radiation might take place; yet some experiments (as we have seen) show the contrary; and here there is no conduction.

With regard to that portion of the heat which accompanies or belongs to light, the theory which I originally suggested, (merely as an hypothesis representing the facts,) viz: that it was simply *the latent heat of light*, developed, of course, when the *light was absorbed*, is connected with the hypothesis of the materiality of light; but it may be worth inquiry whether it does not apply even better to the *elastic œther*, in whose undulations light is now proved to consist.

REPORT FOR 1840.

Having been one of those who at the first institution of the British Association were applied to to prepare reports on the state and progress of the different branches of science, and having in consequence laid before the Association at the Oxford meeting in 1832 such a review of the subject of Radiant Heat, I have felt peculiar satisfaction in being again honored by a request from the council to furnish a second report supplementary to the former, embracing the progress of knowledge in that department from the period to which the first report extends, up to the present time.

Such a supplementary account has been rendered peculiarly necessary, from the great number and high importance of the results which have been arrived at by several eminent experimenters in the interval which has elapsed; and though much is still required to be done before we attain complete and satisfactory grounds for an unexceptionable theory of radiant heat, yet the discoveries recently made have at least tended greatly to modify all our previous conceptions, and to enable us to refer large classes of the phenomena to something like a simple and common principle.

In my former report I divided the subject under various heads, derived from what appeared, in the existing state of our knowledge, well-marked distinctions between several kinds of effects ascribed to radiant heat. The more recent discoveries have in a great degree so changed our views of the subject that these divisions cannot with any advantage or convenience be adhered to. One grand principle of arrangement, however, has been newly supplied in the capital discovery of the polarization of heat; so that all the researches we have to describe will be conveniently classed under two heads, as they relate—first, to radiant heat in its ordinary or unpolarized state; and secondly, to its polarized condition.

DIVISION I.—UNPOLARIZED HEAT.

Transmission and Refraction of Heat: Melloni.

Since the period to which my former report extends, various notices
have from time to time been given to the British Association relative
to the more important discoveries connected with radiant heat. My
former report includes a statement of some of the first researches of
M. Melloni. At the Cambridge meeting, in 1833, Professor Forbes
gave some account of the further investigations in which M. Melloni
was then engaged, including a brief abstract by M. Melloni himself
of the chief results he had then obtained.* The full details were
subsequently embodied in his several memoirs.

In the earlier part of these researches, M. Melloni had found that
the quantity of calorific rays which traverses a screen is proportional
to the temperature of the source: but the difference constantly dimin-
ishes as the thickness of the screen is less, until with very thin
laminæ it is insensible.

This proves that the resistance to the passage of heat is *not* exerted
at the *surface*, but in the interior of the mass.

With the solar rays, he observed that with various thicknesses of
sulphate of lime, water, and acids, the increase of interception, owing
to increased thickness, is *greater* for the *less* refrangible rays of the
spectrum.

With terrestrial sources he found that a plate of glass, 2 mm. in
thickness, stops, out of 100 rays, from flame 45, from copper at 950°
cent. (incandescent) 70, from boiling mercury 92, from boiling water
100.

Comparing the transmissive powers of a great number of substances
in a crystallized state, he concluded that the diathermaneity for the
rays of a lamp was proportional to their refractive powers; but in
uncrystallized bodies no such law could be traced.

It was in the course of these researches that the author made the
important discovery of the singular property possessed by ROCK SALT
viz: that it is almost entirely permeable to heat even from non-lumi-
inous sources. He found its transmissive power six or eight times
greater than that of an equal thickness of alum, which had nearly
the same transparency and refractive power. He also discovered
that (unlike other diathermanous media) it is *equally* diathermanous to
all species of heat, *i. e.*, to heat from sources of all degrees of lumin-
osity or obscurity; or that it transmits in every case an equal propor-
tion of the heat incident.

Thus he found a plate of 7 mm. (.28 inch) in thickness transmit
about 92 out of 100 rays, whether from flame, red-hot iron, water at
212°, or at 120° Fahrenheit. A plate 1 inch thick gave a similar
constant ratio.

M. Melloni's "Memoir on the Free Transmission of Radiant Heat

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* See Third Report, p. 381-'82.

through Solid and Liquid Bodies," was presented to the Academy of
Sciences at Paris, Feb. 4, 1833, and published in the Ann. de Chimie,
No. liii, p. 1; a translation of it is given in Taylor's Scientific
Memoirs, Part I.

The author commences with a slight sketch of the researches of
previous experimenters, but omits to notice any distinctions between
the *characters* of the heat from different sources, or the different
kinds of heat from one and the same source, when luminous, espe-
cially as indicated by my experiments published in the Phil. Trans.
for 1825.

He then proceeds to some "general considerations on free trans-
mission of caloric through bodies, and the manner of measuring it by
means of the thermo-multiplier." This, in fact, constitutes a sup-
plementary and more enlarged portion of his former researches. He
goes into extensive details on the precautions necessary to be used in
such investigations; especially for guarding against the interference
of secondary radiation: as this changes with the change of place of
the screen, he thus allows for its effects. He also gives some general
observations on the use of the galvanometer, and the correct estima-
tion of the forces acting upon it.

The next subject of inquiry is the effect due to "the polish, thick-
ness, and nature of the screens." The source of heat being a lamp,
screens were employed of glass rendered of different degrees of
opacity by grinding, &c.; and the effects by transmission through
them were found to be in proportion to the transparency, or that the
heat follows the same proportion as the light.

The effect of liquids between glass plates was then tried; and more
rays were found to be absorbed in proportion to the increase of thick-
ness. Different numbers of glass screens were also employed in com-
bination; the same conclusion also held good.

The results with a numerous series of screens of various media,
solid and liquid, were then tried, and are stated in a series of tables:

Table I. Various kinds of uncolored glass.

Table II. Liquids: to give a general sketch, the order of trans-
mission was as follows, beginning with the greatest:

  Carburet of silver.

  Chlorides.

  Oils.

  Acids.

  Water.

Table III. Crystallized bodies, transparent and opaque; the results
follow no relation to transparency; the following is the general order:

  Rock salt.

  Various crystals.

  Alum.

  Sulphate of copper—no effect.

Table IV. Colored glasses. Red and Violet transmitted most—
yellow, green, and blue, least—heat.

The author concludes, in general, (the source being a lamp,) that
the diathermancy is not proportional to the transparency; and makes

some general remarks on these results as related to those of Seebeck on prismatic dispersion.

A supplement to the last paper was presented by the same author to the Academy, April 21, 1834, entitled "New Researches on the immediate Transmission of Radiant Heat through different Solid and Liquid Bodies." It is published in the Ann. de Chimie, lv, 337, and translated in Taylor's Scientific Memoirs, Part I, p. 39.

The author first investigates "the modifications which calorific transmission undergoes in consequence of the radiating source being changed."

He employs four sources of heat. 1. A Locatelli lamp. 2. Incandescent platina. 3. Copper heated by flame to about 730° Fahrenheit. 4. Hot water in a blackened copper vessel. The heat from each of these sources was first compared as transmitted through plates of glass of different thicknesses, from .07 millims. to 8 millims. The results are given in a table, from which it appears that with copper and hot water the diminution of effect is rapid, with an increase of thickness in the screen; with water it is nothing beyond a thickness of .5 mm. A second table gives results for about 40 solid media of different kinds of the same thickness; most of them were wholly impervious to dark heat; the most remarkable exceptions being fluate of lime and rock salt.

In another table are the results with black glass and black mica; these substances, though diathermanous to the lamp and incandescent platina, are wholly impervious to the rays from hot water, and nearly so to those from heated copper.

The discovery of the entire diathermancy of rock salt has been before referred to, and has furnished the means of prosecuting the author's yet more remarkable researches on the REFRACTION OF HEAT.

To this important point M. Melloni devotes a portion of the same memoir. After a sketch of previous attempts to establish this property, he describes his successful experiment by concentrating to the focus of a *rock-salt lens* the rays of *dark heat* from hot copper and hot water. *A similar lens of alum* produced *no* effect. This proves that the effect is not due to the mere heating of the central part of the lens.

He next advances to the refraction of heat by a rock-salt *prism;* describing an apparatus for the purpose. That the effect is not due to secondary radiation is shown by turning the prism on its axis into a different position, when no effect is produced.

He then discusses the "properties of the calorific rays immediately transmitted by different bodies." Under this head are detailed one of the most remarkable species of effects which the whole range of the subject presents.

The rays of the lamp were thrown upon screens of different substances in such a manner that either by changing the distance, or by concentration with a mirror, or a lens of rock salt, the effect transmitted from all the screens was of a certain constant amount. This *constant radiation* was then intercepted by a plate of *alum*, and it was found that *very different proportions of heat were transmitted by the alum*

in the different cases. This very singular result is established by numerous detailed experiments, of which a tabular statement is given; and the author states it in the following terms: "*The calorific rays issuing from the diaphanous screens are, therefore, of different qualities, and possess, if we may use the term, the diathermancy peculiar to each of the substances through which they have passed.*"

He next investigates the effects of *different colors* in *glass* on the absorption of heat. He infers, in general, that the coloring matter diminishes the power of transmission, and examines the question, Does it stop only rays of a definite refrangibility analogous to what happens in the absorption of light?

With this view (following a similar mode of operation to that adopted in the last instance) he used, successively, glasses of different colors, for each of which the distance of the source was varied till a standard effect (about 40° deviation of the needle) was produced on the galvanometer. In this position, in each case, a plate of sulphate of lime was then interposed, and diminished the deviation to about 18° for all the colored glasses, except green, in which case it was to about 8°. When alum was substituted the deviations were reduced, in the first case, to 8°, in the second to 1°.6. Hence he concludes that all the colored glasses, except green, produce no "elective action" on heat; green glass, on the contrary, transmits rays more easily stopped than the others.

Connecting this with his other inference, that rays are stopped in proportion to their refrangibility, he instituted another series of experiments to put this to the test. The sources of heat compared were an argand lamp and incandescent platinum, the rays of heat from the former being the more refrangible. The quantities of heat from the lamp and the metal transmitted by the green glass were nearly equal; by all the others, nearly in the ratio of 2 to 1. Hence he infers that green glass is more diathermanous for rays of less refrangibility.

Again, the rays transmitted by citric acid and some other substances are those only of the greatest refrangibility. They should, therefore, be the least transmissible by green glass. This was found to be the case. Of 100 rays passed through citric acid, all the other glasses transmitted various preparations, from 89 to 28, while green glass transmitted only from 6 to 2.

Without the citric acid the rays from incandescent platinum were more copiously transmitted by the green glass than by the others.

The whole of the rays of low refrangibility emitted by the platinum, and for which alone the green glass is transparent, had been stopped by the interposition of the plate of citric acid, which had, as it were, sifted it free from these rays.

Hence, the author concludes that "*green glass is the only kind which possesses a* COLORATION *for heat, (if we may use the expression,) the others acting upon it only as more or less transparent glass of uniform tint does upon light.*"

In a subsequent part of the memoir, M. Melloni gives a tabular view of the effects, observed in the same manner, of the constant

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radiations emitted from six different substances, each intercepted successively by 24 minerals and 10 colored glasses, from which it appears that the transmission is very different, according to the nature of the first medium.

He afterwards describes an experiment with the solar rays transmitted by a green glass, and then intercepted by other media. They pass copiously through rock salt, but feebly through alum. Hence he concludes *that there are among the solar rays some which resemble those of terrestrial heat,* and, in general, that *" the differences observed between solar and terrestrial heat, as to their properties of transmission, are therefore to be attributed merely to the mixture in different proportions of these several species of rays."*

In a note to this memoir, M. Melloni refers to my original experiment, *(Phil. Trans.,* 1825,) in which the action of the rays on *surface* is observed in *connexion* with their *transmissibilty.*

He confirms the accuracy of my result by a careful repetition of the experiment *with the thermo-multiplier,* but makes no reference to the conclusion I had drawn, viz: the coexistence of two distinct sorts of heat in the radiation from luminous sources, one of which is the same as that from dark sources. He explains the result by supposing the transmitted rays to acquire, in and by the act of transmission through the glass screen, new properties in their relation to the surfaces on which they fall, *i. e.,* to the degree of absorption they undergo respectively on a black and a white surface.

He extends the investigation by a table of results of the same kind with a series of screens, both transparent, and of various degrees of opacity. The ratio of the effects on the black and white surfaces is nearer to equality as the screen is more opaque.

On this subject there appears a short paper by M. Melloni in the London and Edinburgh Journal of Science, vol. vii, p. 475, to which I replied in the same journal, January, 1836.

While referring to my own experiments, I may be allowed to add that in Dr. Thomson's Treatise on Heat, &c., first edition, the bearing of my investigation was incorrectly represented, and accordingly pointed this out in the London and Edinburgh Journal of Science, Nov., 1830.

In the second edition of Dr. Thomson's work, which has lately appeared, the author omits all mention of the subject whatever.

Transmission and Refraction of Heat: Forbes.

The subjects of transmission and refraction of heat were taken up by Professor Forbes, and Melloni's experiments repeated and extended by him, the details being given in the first and part of the second sections of his first Memoir " on the Refraction and Polarization of Heat," read to the Royal Society of Edinburgh, January 5 and 19, 1835, and published in their Transactions, vol. xiii; also in the London and Edinburgh Journal of Science, vol. vi.

The first section contains an account of various experiments with the thermo-multiplier. The principal object was to verify the several

points already stated, and especially to determine the degree of accuracy of the instrument. From a comparison of its sensibility with that of air thermometers, the author concludes that 1° of deviation of the needle corresponds to an effect indicated by about $\frac{1}{60}$ of a centigrade degree. Without increasing the dimensions of the instrument, by which its sensibility would be impaired, he has been enabled, by the adaptation of a small telescope, readily to measure $\frac{1}{10}$ of its degrees, that is, about $\frac{1}{600}$ of a centigrade degree.

One of the most interesting points to which the author directed his attention, was the possibility of detecting *heat in the moon's rays.* These rays, concentrated by a polyzonal lens of 32 inches diameter, and acting on the thermo-multiplier, gave no indication of any effect, so that Professor Forbes considers it certain that if there be any, it must be less than $\frac{1}{500000}$ of a centigrade degree.

He repeated Melloni's experiment of the *refraction of heat by a rock-salt prism,* and was enabled to obtain some approximate quantitative results, giving the index of refraction for heat in this substance, which was a little less than that for light.

In the course of his second section he describes further experiments relative to the question discussed by Melloni, of the separation of the effects due to heat and light, especially the peculiarity (before mentioned) attending green light; he tried flames variously colored with salts—giving red, yellow, green, and blue light; but found the proportions of rays transmitted by alum, glass, and rock salt to be nearly constant for each substance.

To this part of the subject Professor Forbes again directed his attention, in a later series of experiments, in which he has obtained numerical results of the highest value. These are detailed in the last part of his third series of Researches on Heat, read before the Royal Society of Edinburgh, April 16, 1838, and published in the Transactions of that body, vol. xiv. To the earlier portion of this memoir we shall refer, under another division of this report.

The third section relates to the *index of refraction* for heat of different kinds as compared with that for light in the same medium. The method of observation adopted is indirect, turning upon the determination the *critical angle* of total internal reflection. This was ascertained in *rock-salt* prism, having two angles of 40°, and one of 100°. The sentient surface of the pile is so placed with regard to the prism that it continually receives rays coming from the source of heat, after undergoing two refractions and one reflection, whatever be the angle of incidence, which is effected by a very simple but ingenious mechanical construction. Every kind of precaution to avoid error was adopted. And in this way the author obtained a series of indices "for the mean quality of the heat most abundantly contained in the rays obtained from various sources." These values are given in a table, and are professedly but approximate. Professor Forbes has, however, subsequently favored me with an unpublished communication, in which he states that while the numbers may be regarded as *relatively* correct, in order to become *absolutely* so, they

must all be reduced by about .05.   This will give the corrected series
of results as follows:

Source of heat.	Index of refraction for rock salt.
Locatelli lamp	1·521
Locatelli lamp, transmitted through alum	1·548
Locatelli lamp, transmitted through glass	1·537
Locatelli lamp, transmitted through opaque glass	1·543
Locatelli lamp, transmitted through opaque mica	1·533
Incandescent platina	1·522
Incandescent platina transmitted by glass	1·538
Incandescent platina transmitted by opaque mica	1·534
Brass at 700°	1·518
Brass at 700° transmitted by clear mica	1·527
Mercury at 450°	1·522
Mean luminous rays	1·552

From the experiments described in this section the following gen-
eral conclusions are deduced:

1. The *mean* quality, or that of the more abundant proportion of
the heat from different sources, varies within narrow limits of refran-
gibility.

2. These limits are very narrow, indeed, where the direct heat of
any source is employed.

3. All interposed media, (including those impermeable to light,) so
far as tried, *raise* the index of refraction.

4. All the refrangibilities are inferior to that of the mean luminous
rays.

5. The limits of dispersion are open to further inquiry; but the
dispersion in the case of sources of low temperature appears to be
smaller than in that from luminous sources.

Reflection of Heat : Melloni.

A short paper, by M. Melloni, entitled "Note on the Reflection of
Heat," was read to the Royal Academy of Sciences, November 2,
1835, and published in the Ann. de Chim., ix, 402, of which a trans-
lation appears in Taylor's Sci. Memoirs, Part III, p. 383.

After referring to the experiments of Leslie, to show that the re-
flection of heat depends materially on the texture, polish, &c., of the
reflecting surfaces, he proceeds to consider what takes place in dia-
thermanous substances, as in rock salt, where, there being no absorp-
tion, the difference of the heat transmitted gives the quantity reflected
at the first and second surfaces.   With other media, as glass, rock
crystal, &c., very thin plates exercise no sensible absorption; hence
heat, after traversing a thick plate, being intercepted by a very thin
plate, the loss which this occasions is due solely to the two reflections.
These considerations afford the means of estimating the intensities of
reflected heat from different substances; and the author, in conclu-

sion, gives a comparative statement of the reflections from rock crystal and copper.

Analogies of Light and Heat: Melloni and Forbes.

M. Melloni's "Observations and Experiments on the Theory of the Identity of the Agents which Produce Light and Heat" were read to the Academy of Sciences, December 21, 1835, published in the Ann. de Chimie, No. 50, p. 418, and translated in Taylor's Scientific Memoirs, Part III, p. 388.

In this paper the author combats the views of M. Ampère, who had proposed some ingenious speculations for explaining, on the theory of undulations, the identity of light and heat, the difference of effect being dependent solely on the different wave-lengths, those producing heat being supposed longer than those giving rise to light. Athermanous media, such as water, intercept the longer waves, but not the shorter. Thus the aqueous humor of the eye prevents the retina from being affected by heat as well as light.

The author admits that many phenomena may be sufficiently accounted for by the mere supposition of the difference of wave-lengths; but he mentions some experiments in which he thinks decisively that this will not hold good.

The spectrum formed by a rock-salt prism gives the maximum of heat considerably beyond the red end. On interposing water of increasing thickness, the maximum successively occurs *in* the red, and thence upwards to the *green*. A similar effect is produced by colorless glasses; but with *colored* glasses, whilst the *luminous* spectrum is variously absorbed and altered, the place of the maximum of heat *remains unaltered*, and the decrease from it quite regular.

Another experiment consists in interposing a diaphanous body, which absorbs all the calorific, but only a part of the luminous rays. On using in this way a peculiar species of green glass colored by oxide of copper, the greenish light transmitted "*exhibits no calorific action capable of being rendered perceptible by the most delicate thermoscopes, even when it is so concentrated by lenses as to rival the direct rays of the sun in brilliancy.*"

On these points Professor Forbes has made some remarks in the London and Edinburgh Journal of Science, March, 1836.

Such experiments as these, he justly observes, and indeed many more simple, clearly show that heat is not light, but nothing more. It is a question, then, what is the point really aimed at in these speculations. The author agrees with Melloni in the result, "that one and the same undulation does not invariably impress the senses of sight and feeling at once. The great difficulty is this: to account for the equal refrangibility of two waves having different properties."

New Phenomena of Transmission: Melloni and Forbes.

It appears by the *Comptes Rendus* that on September 2, 1839, M. Arago communicated to the Academy of Sciences a letter by M. Melloni, containing some new and highly interesting experiments on the

transmission of radiant heat. He found that *rock salt* acquires, by
being *smoked*, the power of transmitting most easily heat of low
temperature, or of that kind which is stopped in the greatest propor-
tion by glass, alum, and (according to his view) all other substances.

Upon this point Professor Forbes was led to some further considera-
tions, and thence to fresh series of researches "On the Effect of the
Mechanical Textures of Screens on the Immediate Transmission of
Radiant Heat," an account of which he communicated to the Royal
Society of Edinburgh December 16, 1839.

Upon the above-mentioned result of Melloni, Professor Forbes re-
marks that, according to the conclusions indicated in his own Re-
searches, (third series,) Melloni's view of the interception of heat of
low temperature by all substances alike is equivalent to saying that
substances in general allow only the more refrangible rays to pass,
or that while rock salt presents the analogy of white glass, by trans-
mitting all rays in equal proportions, every other substance hitherto
examined acts on the calorific rays as violet or blue glass does on light,
absorbing the rays of least refrangibility and transmitting only the
others.   And to this rule Melloni now makes out the first exception,
or the first analogue of red glass, to be rock salt, having its surface
smoked.

Now, Professor Forbes, in his third series, had also pointed out
another substance having the same property, viz: mica split by heat.
In March, 1838, he had established, by repeated experiments, that
the previous transmission of heat through glass, far from rendering
it less easily absorbable by mica in this state, had a contrary effect;
and also that heat of low temperature, wholly unaccompanied by light,
was transmitted almost as freely as that from a lamp previously passed
through glass.

Mica not laminated possesses no such property; hence the effect is
due to the peculiar mechanical condition of the substance, and hence
it occurred to the author that the effect of smoking the rock salt was
owing merely to a mechanical change in the surface; he therefore
proceeded to try the effects of surfaces altered by mechanical means.

The surface of rock salt being roughened by sand-paper, it trans-
mitted non-luminous heat more copiously than luminous. Mica
similarly scratched showed the same result.

This effect is not attributable to differences in the proportions of
heat reflected, for in this respect, at a polished surface, all kinds of
heat are alike, as he had before shown; whilst by direct experiment
he found that, at least for the higher angles of incidence, reflection
is most copious from rough surfaces for heat of low temperature, or
the same kind which is most freely transmitted—proving incontestab
that the *stifling* action of rough surfaces is the true cause of the ine-
quality.

That there is a real modification of the heat in passing through a
roughened surface, as well as through laminated mica and the smoky
film, appears from some direct experiments on heat *sifted* by these
different media, which, when transmitted by any one of these, is found
in a fitter state to pass through each of the others; and this modification
is the more perceptible as the character of the heat is more removed

from that which these media transmit more readily; that is, as the temperature of the source is higher. The following results were stated:

Heat from lamp through smoked rock salt.	Rays out of 100 transmitted.
Direct ·································	36
Previously sifted by another plate of smoked rock salt·	44
Do ············· do ······· laminated mica ··	44
Do ··········· ·····do ······· roughened salt ··	40½

The author then proceeded to try the effect of *fine wire gauze* and fine *gratings of cotton thread*, but no difference could be detected coresponding to the different kinds of heat; in every case the interception was proportioned to the fineness of the gauze.

When *fine powders* were strewed between plates of rock salt, or *fine lines* were ruled upon the surface, or the surface *tarnished* by mere exposure to the air, the easier transmission of heat of low temperature was rendered apparent.

These effects the author considers as evidently pointing to *phenomena in heat resembling diffraction and periodic colors in light.*

Such was the general sketch of his researches which Professor Forbes gave at the period above mentioned. Subsequently (up to March, 1840) he continued engaged on the same subjects, and on May 15, 1840, laid before the council of the Royal Society, Edinburgh, a more extended account of the entire investigations, which appears in vol. xv, Part I, of their Transactions, under the title of " A Fourth Series of Researches on Heat." Some remarks by M. Melloni appear in the *Comptes Rendus,* March 30, 1840, on the same subject.

For obtaining a general view of these results the main point to be kept in sight is the relation which the transmissibility of each sort of heat appears to bear to its refrangibility; and hence the analogy of diathermanous media, which transmit the less refrangible heat, to transparent media, which transmit the red rays of light, the transmission of the more refrangible heat being analogous to that of violet light.

Upon this important point Professor Forbes enlarges in the introductory part of his memoir; he justly observes that such a generalization carries us forward a step, by teaching us to refer to the quality of *refrangibility* certain properties of heat which before were connected only with certain vague characters in the nature of the source whence it was derived. Among other things we find, what was long suspected, but what Melloni first conclusively proved, that it does not essentially depend on the presence or absence of *light.* This refers to his singular discovery of the change produced by the intervention of certain screens.

Heat from *any* source, if it admit of transmission at all through glass, alum, or water, will ultimately have the character of glass-heat, alum-heat, or water-heat, just as light from the sun or from a candle becomes red, blue, or green, by transmission through glasses of those colors.

The author gives, as an illustration, the following scale of different kinds of heat, in the order of refrangibility, beginning with the lowest:

1. Heat from ice.
2. Heat from the hand.
3. Heat from boiling water.
4. Heat from a vessel of mercury under its boiling temperature.
5. Heat from metal, smoked; wholly non-luminous in the dark, heated by an alcohol lamp behind it.
6. Heat from incandescent platina, (over a spirit lamp.)
7. Heat from an oil lamp, (direct.)
8. *Oil-lamp heat* transmitted by common mica.
9. *Oil-lamp heat* transmitted by glass, (argand lamp.)
10. *Oil-lamp heat* transmitted by citric acid.
11. *Oil-lamp heat* transmitted by alum.
12. *Oil-lamp heat* transmitted by ice.

Melloni having shown that a portion of the heat from a luminous source is transmitted through certain screens, which are wholly opaque to light, it became natural to inquire whether the rays so passed possessed the properties of heat from dark sources. This he found to be partly the case and partly not.

The direct test of examining the refrangibility of the heat-rays issuing from the screen occurred to Professor Forbes, who found that opaque glass and mica act as clear glass and mica do in *elevating the mean refrangibility of the transmitted heat*, an action analogous to that of *yellow* glass upon light.—(See 3d Series, art. 73, 81, &c.)

But in all this there was nothing exactly equivalent to the action of red glass; this, however, was discovered by Melloni, by the happy suggestion of covering the surface of rock salt with smoke.

These remarks introduce more clearly the main object of Professor Forbes in following up the inquiry. In the present paper the details of many series of experiments are given, and the more precise results now established may be stated as follows:

I. The peculiar character of the film of smoke on the surface of a diathermanous medium, analogous to redness in glass for light, was found to be possessed by—1. The simple powder of charcoal. 2. Some other dull earthy powders. 3. Surfaces simply dull, or devoid of polish. 4. Surfaces irregularly furrowed, as with emery or sandpaper. 5. Polished surfaces, on which fine distinct lines have been drawn. 6. Transparent mica, when mechanically laminated, which, as a continuous medium, possesses opposite properties.

II. All kinds of heat (*i. e.*, of all refrangibilities) seem affected indifferently by the following media:

1. The thinnest leaf-gold, *which is impervious to any kind of heat.*
2. Fine metallic gratings, which transmit all kinds of heat in a proportion probably exactly that of the areas of their interstices.
3. Thread gratings.
4. Most crystalline bodies in a state of powder, in which case they approximate to a condition of opacity for heat.

III. The following substances, in addition to those before known

transmit most heat of high temperature or high refrangibility, analogous to violet light:

1. Several pure metallic powders. 2. Rock salt, in powder, and many other powders. 3. Animal membrane.

IV. Heat of low temperature is most regularly reflected at imperfectly polished surfaces. It is also, as has been shown above, most regularly transmitted. These facts are in themselves very remarkable, and especially so with reference to the *theory* of heat, and its analogies to that of light, particularly with respect to absorption. Some of these considerations, which bear on the undulatory doctrine, are noticed by the author in section 24.

The curious question relative to the analogies of the action of gratings, &c., to the parallel cases in the interference of light, has been recently illustrated by some mathematical investigations by Professor Kelland; and the author concludes his memoir with some highly ingenious and interesting suggestions for further inquiry bearing on these topics.

Radiation of Heat: Hudson.

At the meeting of the British Association, 1835, Dr. Hudson, of Dublin, communicated some researches on radiant heat, of which notices appear in the report of that meeting.—(p. 163, and Proceedings of Sections, p. 9.) A paper by the same author on the subject is printed also in the London and Edinburgh Journal of Science, vol. viii, p. 109.

In the paper last mentioned, besides making some critical remarks on the results of Melloni and others, the author describes a very simple and effective mode of arranging the apparatus for experiments on diathermancy with the thermo-multiplier, so as completely to exclude the influence of secondary radiation. The source of heat is a canister of hot water, which can be so placed in two different positions that it is exactly at the same distance, and presents the same surface; but in one case the pile receives the heat both direct and secondary; in the other only the secondary, derived from the heating of the screen.

In his communication to the British Association the same author examines principally certain questions bearing on the supposed radiation of cold, and the theory of Leslie. These were performed by a differential thermometer, and a concave reflector, with a hollow back, so that the mirror itself could be heated to any required point by filling the hollow with hot water. The source of heat was a canister of water, with one surface varnished, another metallic.

The main results were as follows:

1. The mirror being at the temperature of the air, and the canister *cooled below it*, the varnished side produced a greater *cooling* effect on the focal bulb than the plain, in the same ratio as that in which it produced a greater *heating* effect when the canister was heated *above* the air.

2. The mirror being heated to 200° Fahrenheit, and the canister at the temperature of the air, both bulbs were so placed as to be

equally affected by the heat of the mirror; when the canister displayed a cooling effect, the varnished side being the most efficacious.

3. Again, with the same conditions, except that the canister was heated 10° or 12° above the air, it was placed at different distances; at near distances it showed a cooling effect; at a certain point this ceased, and beyond it it began to produce a slight heating effect.

4. Some attempts were made to try the effects while the bulb was kept cool by evaporation; the canister being also cooled below the air, the cooling of the bulb was increased beyond what took place when the canister was at the temperature of the air. These experiments were confessedly imperfect, from the difficulty of regulating the evaporation.

The author considers them as favorable to the theory of *the radiation of cold;* he also refers to them as in some degree confirmatory of Leslie's view of pulsation.

The most remarkable result is that of case 2; it seems to prove that a mirror, when heated, will still reflect rays of heat, thrown upon it from a source of much lower temperature.

The results are viewed by the author as supporting the theory of the radiation of cold. I believe the doctrines of that theory may in all cases be equally well expressed in other language, in conformity with the view to which I referred in my former report, [p. 300.]

Dr. Hudson has speculated with much ingenuity on another point of great interest, the different radiating powers of different surfaces. Understanding by the surface a certain physical thickness, he conceives the *radiating power to depend on the capacity for heat* of the substance of the lamina, which seems perfectly conformable to the general law of the equilibrium of temperature.

Influence of Surface and Color on Radiation: Stark and Bache.

The influence of the *color* of a surface on its powers for absorbing and radiating heat is a question which has long attracted notice, and has often been involved in no small confusion from false analogies. The sun's rays, and, in general, what is called luminous heat, are absorbed by surfaces (*cœteris paribus*) in proportion to the *darkness* of their colors ; but it has been too hastily assumed that the same would hold good with non-luminous heat, and still more groundlessly, that the *color* would influence the *radiating* power of the surface ; the *texture* of the surface, however, is known to exert a powerful influence. These distinctions are fully insisted on in my former report.

Since that period, however, the subject has been taken up by Dr. Stark, who, in an elaborate paper in the Phil. Trans. for 1833, details a number of ingenious experiments, which he conceives support the doctrine of the influence of color, not only on the absorption of dark heat, but even on odors, miasma, &c.

The object of the present report is not controversial ; I will therefore merely state that I discussed in detail Dr. Stark's reasonings, in a paper published in the Edinburgh New Philosophical Journal, October, 1834, where, though allowing the value and accuracy of the

experiments, I have expressed my objections to the inferences made from them.

It appears in general that the *texture and nature* of the surface most unquestionably exert a great influence. Now, wherever, there is a difference in the *color*, there *must* be *either* a difference in the mechanical structure of the surface, *or* some new matter added or abstracted. When, therefore, we consider the changes which thus occur, we cannot infer that the effect is not owing to these instead of to color as such. The question, however, is a highly curious one, and worthy the most accurate investigation.

Having in some measure called attention to it in my former report, it was with no small gratification that I found the subject had excited interest, not only in this country, but also in America; and to Professor Bache (since appointed principal of Girard College) we owe by far the most extensive and valuable series of experiments on this important but difficult point of inquiry; they are given at length in the Journal of the Franklin Institute, November, 1835.

The notices of these experiments which had been published in this country not appearing to convey adequate notions of their nature or value, I endeavored to bring them more prominently forward by some remarks in the Physical Section of the British Association at Liverpool in 1837.* In my former report I had thrown out some suggestions both as to the want of such a series of experiments, and as to the fundamental difficulty arising from the variety of causes which must influence the results; but more especially the differences of thickness in the coatings, which in the ordinary mode of operating could not be estimated, yet must greatly modify the effects.

With reference to the necessity of equalizing the coatings, Mr. Bache refers to an important observation of Leslie, viz: that radiation takes place not merely from the *actual surface*, but from *a certain depth*, or lamina of the surface, the thickness of which is quite appreciable in good radiators, and differs for different substances.

Proceeding upon this fact, the author justly observes, that "the radiating powers of substances would not be rightly compared by equalizing their thicknesses upon a given surface, nor by equalizing their weight; *but by ascertaining for each substance that thickness beyond which radiation does not take place.*"

It is, then, on the original application of this fundamental idea that his whole series of experiments is conducted.

Upon this principle the first object was to obtain some data as to thicknesses of different pigments necessary to be employed.

The method adopted throughout was to employ tin cylinders of the same size, filled with hot water, and having thermometers inserted through a hole in the top, while their surfaces were coated with the different substances under trial. The radiation was estimated by the observed rates of cooling.

To find the critical thickness of the coating just spoken of, the time of cooling a certain number of degrees was accurately observed, first

with a thin coating, then with an additional layer of the pigment, and so on, until it was found that additional thickness did not increase the rate of radiation, but began to diminish it; thus each coating was adjusted precisely to that thickness at which it produced its maximum effect.

Every precaution to insure accuracy appears to have been most diligently taken, and several series of preliminary experiments are recorded for the purpose of ascertaining the limits within which the precision of the results may be relied on. A standard cylinder, coated with aurum musivum (as being found not liable to tarnish or alteration,) was used in all the experiments, and the effect of each coating compared with this under similar circumstances.

The results of different sets of experiments are given in the tabular form, and apply to coatings of a great variety of substances differing in their chemical nature, as well as in roughness, texture, and color. The following table is extracted as fully exhibiting the general result of all the experiments; the substances being arranged in the order of their radiating powers, beginning with the *highest:*

Nature of coating.	Color.	Surface.
Litmus blue	Blue	
Prussian blue	Blue	Rough
Ammon. sulphate of copper	Greenish blue	Rough
Peroxide of manganese	Brownish black	Not shining, but uniform
India ink	Black	Not smooth
Bichromate of potash	Brown	Streaked; smooth streaks
India ink	Black	Smooth
Alkanet	Crimson	Not shining, but uniform
Carb of lead in oil of lavender	White	Smooth, not shining
Sulphuret of lead	Black	
Alkanet blue	Blue	
Carb. magnesia	White	Rough
Carb. lead, in gum	White	Smooth
Carb of lime	Dingy white	Medium
Vermilion	Red	Smooth
Sulph. baryta	White bluish	Rough
Golden sulphurate of antimony	Brown	Smooth, in streaks
Indigo	Blue	Smooth
Cochineal	Crimson	Smooth
Red lead	Orange	Smooth
Sulph. baryta	White	Medium
Plumbago	Black	Not shining, but uniform
Chrom. lead	Yellow	Smooth
Gamboge	Olive green	Smooth, in streaks
Bisulphuret of tin	Yellow	Smooth

It thus distinctly appears that through so extensive and varied a range of differences in the state of the radiating surface *no determine relation* subsists between the *radiating power* and either *darkness of color* or any other distinctive character of the coating employed; not even its roughness or smoothness.

Repulsive power of Heat: Powell.

Closely connected with the radiation of heat is its property of exerting or exciting a repulsive force between particles or masses of matter at small though sensible distances.

Such a property was first announced by Libri in 1824; and was further examined by Fresnel (*Ann. de Chim.*, xxix, 57, 107) and Saigey, (*Bull. Meth.*, xi, 167;) but their results seem to have been open to some doubt.

A new interest attached to the subject from the reference made to this property by Professor Forbes, (in a paper read to the Royal Society of Edinburgh, March, 1833, and since published in their Transactions, vol. xii,) in explanation of certain vibrations of heated metals, first observed by Mr. Trevelyan.

A paper from me was read to the Royal Society June 19, 1834, and printed in the Philosophical Transactions, 1834, Part II, containing an account of experiments on a different principle from any of the preceding, which appeared to furnish a decisive proof of the fact of repulsion.

The essential principle is the employment of the colors of thin plates, as a *measure* of the separation produced between two surfaces, by the repulsive action of heat applied to one of them. I also made observations on several particulars attending the mode of action, both in that paper and in a communication to the British Association at the Edinburgh meeting.[*]

Formation of Ice: Farquharson.

An interesting case. in which the principles of the theory of radiant heat are related to the explanation of natural phenomena, occurs in the instance of the formation of ice exclusively at the *surface* of *still* water, but occasionally at the *bottom* of *running* water. This point excited attention some years ago, and was partially discussed by Mr. Knight in the Philosophical Transactions, 1816. Mr. MacKeevor and and Mr. Eisdale subsequently investigated the theory, and M. Arago gave a discussion of the whole question in the *Annuaire*, 1833, and in the Edinburgh New Philosophical Journal, vol. xv, p. 128; lastly, a highly curious paper appeared in the Philosophical Transactions for 1835, Part II, "On the ice formed under peculiar circumstances at the bottom of running water," by the Rev. J. Farquharson, F. R. S., of Alford, Aberdeenshire. In this paper the author details various new and highly interesting particulars as to the mode of the formation of the spongy masses of spiculæ of ice at the bottom of certain rivers in his neighborhood, and the peculiar circumstances under which alone it is formed. He examines acutely the several explanations which have been suggested, which he shows are all insufficient to explain the *whole* of the circumstances, and then proceeds to suggest

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[*] See Report, 1834, p. 549, and Dr. Thomson's Records of Science.

his own theory. which is grounded essentially on the assumption that the *radiation of heat* from substances at the bottom goes on *through the water ;* and partly, also, on the supposed greater radiation from *dark-colored* surfaces.   Neither of these assumptions, it appears to me, are admissible; the former, especially, is directly at variance with the experiments of Melloni.   Some suggestions, at least, towards a theory not open to these objections are given by an anonymous writer in the Magazine of Popular Science, vol. i, p. 157.

<div align="center">DIVISION II.—POLARIZED HEAT.</div>

<div align="center">*Polarization of Heat :* Forbes.</div>

The original statement by Berard, of the polarization of heat by reflection, and the attempts to verify it, are mentioned in my former report.*   In 1833 Melloni tried to repeat the experiment with tourmalines, but unsuccessfully.†

In 1834 Nobili attempted it by reflection, employing the thermo-multiplier, but without success.‡   The disbelief in such a result, at least with dark heat, seems now to have prevailed generally.   Mrs. Somerville, in the second edition of her " Connexion of the Sciences," (in 1833,) speaks of it as altogether without experimental proof.

Professor Forbes took up the inquiry in November, 1834; and in his first memoir, already referred to in section 2, announced his complete success, after having in the first instance failed from the influence of secondary radiation, which disguised the real effect.

(1.) He proved distinctly the stoppage of a considerable proportion of heat when the tourmalines were crossed, not only with a lamp, but with brass heated below luminosity.

(2.) In the third section of the same memoir he details his researches on the polarization of heat by refraction and reflection.   In the former he employed piles of mica, and through these found even dark heat very freely transmitted at the polarizing angle.   Without (in this stage of the inquiry) aiming at quantitative results, he found in general that the proportion of heat polarized varied with the source in the following order, beginning with the highest:

> Argand lamp.
> Locatelli lamp.
> Spirit lamp.
> Incandescent platina.
> Hot brass, about 700° Fahrenheit.
> Mercury, 500° in crucible.
> Water under 200°.

(3.) The polarization of heat by reflection at the surface of a pile of plates of mica was also established; and with regard to the reflection from glass, Professor Forbes has also remarked that, from the known

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* Pages 301, 312.
† Second Memoir, *Ann. de Chim.*, 55.   Taylor's Scientific Memoirs, Part I, p. 59.
‡ *Biblioth. Univ.*, September, 1834.

proportions of heat reflected, the quantity, even at the maximum which would reach the thermoscope after two reflections, would be so extremely small that no difference of effect in the two rectangular positions could really have been perceptible in the form of the experiment adopted by Berard.

(4.) In the fourth section the author enters on the modifications which polarized heat undergoes by the intervention of crystallized plates between the polarizing and analysing parts of the apparatus— an inquiry suggested by the obvious analogy in the case of light. In the crossed position, when polarized heat is stopped, (if the analogy hold good,) the *intervention* of a plate of double refracting crystal would *restore* the effect. This apparently paradoxical result was fully verified with plates of mica, and subsequently with selenite and other substances, not only in the case of luminous sources, but even with water below the boiling temperature. Of 157 experiments, with three different mica plates, only one gave a neutral and one a negative result. Of these 157, 92 were made with heat below luminosity.

The apparent paradox was increased by the circumstance that a thin plate of mica which "depolarized" but feebly seemed to stop more heat than a thick plate which depolarized more completely.

The main fact was ascertained for the first time on December 16, 1834. The professor justly censures the use of the term "depolarize," and suggests "*di*polarize" as preferable.

(5.) From the result thus unequivocally established, a train of highly curious consequences follow. We have hence, as direct corollaries, the *double refraction* of the rays of heat by the mica, and their *interference* according to the same laws as those of light. Hence also follow the constancy of the sum of the intensities of the rays in the rectangular positions, or their *complementary* character, agreeably to the formulas of Fresnel for light. This again involves their *retardation*, according to the well-known principles of the undulatory theory; and hence, from Fresnel's formulas, we are assured theoretically of the existence of circular and elliptic polarization in the rays of heat under the appropriate conditions. We have thus also the means of deducing the length of a wave of heat.

The whole of this most important series of investigations was completed between November, 1834, and January, 1835, and their originality and priority are thus placed beyond dispute. The main *practical* improvement (which led to all the rest of the discoveries) was the employment of the piles of mica for polarizing the heat. In the summer of 1835, Professor Forbes was at Paris; and finding both M. Biot and M. Melloni sceptical as to his results, he exhibited them with mica piles, which he himself prepared on the occasion, and which he left in M. Melloni's hands.

In these experiments the utmost care was taken to guard against all the sources of fallacy from secondary radiation, &c.; but, as Professor Forbes observed, these always tended to disguise and not to exaggerate the results. One consideration of this kind arising from the mere mathematical question of the different amount of heat which might be radiated from one pile to the other in the two rectangular

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positions, (regarded merely as a mathematical problem,) was proposed by myself at the Dublin meeting of the British Association, 1835, but was completely shown to be inapplicable as a practical objection by Professor Forbes, in a short paper in the London and Edinburgh Journal of Science, November, 1835; and further by direct experiment described in the same journal for March, 1836.

Circular and Elliptical Polarization of Heat: Forbes.

On the 1st of February, 1836, Professor Forbes announced to the Royal Society of Edinburgh, that he had that day succeeded in establishing the *circular polarization* of heat, even when unaccompanied by light, by *direct experiment*. It has been already noticed, that *theoretically* this would follow from the laws of depolarization. But in the present instance, Professor Forbes, following up the analogies of Fresnel with regard to the internal reflection of light, found the very same thing verified with heat by similar internal reflection *in a rhomb of rock salt*, where the plane of reflection is inclined 45° to the plane of primitive polarization.

A short notice of this discovery appears in a paper by the author, in the London and Edinburgh Journal of Science, March, 1836, in which he also states the inference from the same considerations, that the waves are of the *same kind* as those of light, viz: formed by *transverse* vibrations.

In a paper reported in the proceedings of the Royal Society of Edinburgh, March 21, 1836, and printed along with the second series of Professor Forbes's Researches in the London and Edinburgh Journal of Science, vol. xii, that philosopher describes some additional results which he has obtained respecting the polarization of heat. These are briefly as follows:

1st. Heat polarized in any plane, and then reflected from the surface of a refracting medium, changes its plane of polarization in a manner similar to what obtains in light; that is, the plane is on one side of the plane of reflection up to the maximum polarizing angle, and on the other side after passing that limit. This mode of determining the polarizing angle offers some advantages over the more direct methods.

2d. Metals polarize heat very feebly by reflection. Yet the effect is perceptible, and increases, through a considerable range of incidences, but it does not seem to attain a maximum; in this respect it seems to agree with what Sir D. Brewster has remarked in light, viz: that the maximum is greatest for the least refrangible rays, heat being less refrangible than light.

3d. Heat polarized in a plane inclined 45° to the plane of reflection at silver, has its nature changed, as in light, and presents the conditions of *elliptic polarization*, though the ellipse is much more elongated.

4th. *Two reflections* from silver increase the polarizing effect of metals, and an increased tendency to circular polarization under the

conditions of the last case. The effect increases with the obliquity of incidence.

All these results have been verified in the case of obscure as well as luminous sources of heat.

On the 15th Feb., 1836, the Keith prize was awarded to Professor Forbes by the Royal Society of Edinburgh; the Vice President, Dr. Hope, stating, in the course of a most able address delivered on the occasion, that several members of the council, as well as himself, had personally witnessed the satisfactory verification of the main facts announced before the medal was adjudged.

Polarization of Heat from different sources: Melloni.

M. Melloni's first memoir "on the Polarization of Heat," was read to the Academy of Sciences in January, 1836; it appears in the *Ann. de Chim.*, lxi, April, 1836, and is translated in Taylor's Scientific Memoirs, Part II, p. 325.

The author commences with a fair review of the previous investigations on the subject, admitting Professor Forbes's discovery, but remarking the very small amount of the effect in the case of obscure heat.

He adopts the supposition that "the different temperatures of the calorific rays are to radiant heat what the different colors of the luminous rays are to light." The latter, he observes, are all equally polarizable, and thus he is led to regard the difference of polarizability in the rays of heat as rather apparent than real. His object then, in this memoir, is to examine the question of the reality of the polarization of heat, and of the equality of the effect in different sorts of heat.

After some considerations on the general nature of the apparatus to be employed, and overcoming the difficulty arising from the small total intensity of the rays, by concentrating them by means of a rock-salt lens, he proceeds to detail his several series of experiments, the results of which he gives in the form of tables :

Table I gives the different indices of polarization obtained with nine sorts of tourmalines of different color,. the source of heat being a locatelli lamp.

He then tried the experiment, taking that pair of tourmalines which gave the greatest effect in the last set, with plates of various substances interposed between the lamp and the apparatus. Of these, opaque black glass rendered the effect nearly insensible, other solids and liquids of various degrees of transparency produced effects of different magnitude.

In Table II these results are registered, and the properties of the media, in this respect, were found to follow the same proportion as their diathermancy.

The author considers the difference of the tourmalines in this respect as referrible to the same cause.

Table III gives similar results with another pair of tourmalines, in which case the proportions are found to differ.

In Table IV are given the indices of polarization with four different

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pairs of tourmalines, each employed with different sources of heat, viz: the locatelli lamp, argand lamp, incandescent platina, and copper at 400°. The effect in the latter case was *very* small.

In recapitulating his views, the author refers to the *unequal* absorption of the two pencils in different tourmalines, as causing the differences observed.

A further paper by the same author, on Tourmaline, &c., in the *Ann. de Chim.*, April, 1836, displays much ingenuity, but nothing of peculiar novelty or fundamental importance.

From the *Comptes Rendus*, 1836, i, 194, it appears that on the 15th February, 1836, M. Arago communicated to the Academy of Sciences a letter from Professor Forbes, announcing his discovery of the circular polarization of heat of the rock-salt rhomb.

At the next meeting of the same body, (February 22,) MM. Biot and Melloni stated that in following up Professor Forbes's experiment, they had found that *quartz possessed the same "rotative" quality for heat as for light.*

Dr. Thomson, in *the second edition* of his Treatise on Heat, &c., (1840,) while giving an outline of the discoveries of Forbes and Melloni, has by no means clearly distinguished the share borne by each of those philosophers in the investigation. In particular, with respect to the fact of polarization, he has not given Professor Forbes the credit so unquestionably due to him for the priority of the discovery. He observes, (p. 139,) "In the earlier experiments of Melloni, he did not find that the rays of heat were polarized when passed through the tourmaline. But he afterwards found that this conclusion was hasty, and that the tourmaline polarizes heat as well as light. The truth of this statement is shown very clearly by Professor Forbes. They also polarized heat by plates of mica, and also by reflection," &c.

These expressions certainly assign the priority to Melloni, as well as an equal share in the subsequent results, both of which we have seen are greatly at variance with the truth.

Further Researches: Forbes.

Professor Forbes's second series of Researches on Heat was read to the Royal Society of Edinburgh, May 2, 1836, and printed both in the Edinburgh Transactions, vol. xiii, and in the London and Edinburgh Journal of Science, vol. xii, 1838.

The author remarks at the outset, that in his former memoir he had confined himself to the establishment of the general facts of the polarization and *di*polarization of heat, without pretending to accurate quantitative results; he now proceeds, therefore, to a more detailed investigation of the subject, with a view to more precise numerical determinations.

The first section relates to the *methods* of observation employed, and the examination of the values of the degrees of the galvanometer, which, for the most part, do not indicate equal increments of force. Two tables are given. By the first, the *statical* deviations of the needle are reduced so as to be measures of the force producing them;

by the second, the *dynamical* effect, or arc, moved over by the initial disturbing action, is reduced to the final or statical effect, and thence to the true measure of heat. Several peculiarities attendant on the use of the galvanometer are likewise discussed.

In section 2 the observations formerly published on the polarizing action of tourmaline are confirmed, including the case where heat, entirely unaccompanied by light, was employed. In this case, the author allows, the greatest difficulty was to be encountered.

The third section treats of the laws of the polarization of heat by refraction or transmission. Professor Forbes expressly observes that his former results were not held out as numerically precise; and with reference to Melloni's conclusion, "that all kinds of heat are equally polarizable at the same incidence," he confirms his former view of the incorrectness of this inference by a great number of experiments, which show that the heat from non-luminous sources is less polarizable by a given plate of mica, at a given angle of incidence, than that accompanied by light.

These experiments were performed with plates of mica, prepared in a way discovered by himself, to which reference is made (though without describing the process) in a paper before quoted in the London and Edinburgh Journal of Science, March, 1836. The method consists in applying sudden heat to a thick plate of mica, which splits into an infinity of extremely thin films, so thin as to be incapable of retaining heat; these form polarizing piles of great energy. With one pair of such plates the author obtained the following per centages of heat *stopped*, when the planes of refraction of the two plates were in the rectangular position:

Source of heat.	Rays out of 100 polarized.
Argand lamp	72 to 74
Incandescent platina	72
Brass about 700°	63
Brass with glass screen	72
Mercury in crucible at 410°	48
Boiling water	44

These observations were repeatedly made, and verified by others with other pairs of plates. The results agree with the analogy of light; those lowest in the scale being the cases of the least refrangible rays.

In the fourth section the law of polarization by reflection is discussed. A number of reflecting surfaces were tried, and split mica was preferred. The amount of polarization by reflection at a given angle is shown to vary with the source of heat; and it is probable that the kinds of heat do not rank in the same order when the angle is changed. This is the case with light. The change of the plane of polarization by subsequent reflection is similar to that which occurs when light is used.

The circular polarization of heat by total internal reflection is discussed in the fifth section. This, as before remarked, is a pheno-

menon really produced in the experiments on dipolarization, if the mica be of a suitable thickness.   The direct experiment with rhombs of rock-salt has been already mentioned also.   The author here gives a detailed account of them, and the laws of the phenomena deducible, in which the precise analogy with those of light is preserved.

Equal Polarizability of Heat from different sources: Melloni.

Melloni's second memoir on the Polarization of Heat appears to be founded on the second part of his communication to the Royal Academy of Sciences in January, 1836.   It is printed in the *Ann. de Chim.*, lxv, May, 1837, and the translation in Taylor's Scientific Memoirs, Part VI.

The principal points of these extensive researches may be reduced to the following heads :

(1.) Referring to Professor Forbes's researches, first series, Melloni contends that the differences of polarizability in the heat from different sources there exhibited are in fact due to differences of secondary radiation from the heating of the mica piles, and subsequently appeals to Forbes's second series, in which he conceives the approach to equality is much nearer, as this source of error was more avoided.

At lower temperatures of the source, he observes that mica transmits less heat in proportion, and therefore absorbs more; thus the secondary radiation is greater, and the apparent difference in the two positions, or index of polarization, is less.

(2.) He remarks that Professor Forbes had found the heat from a dark source, after transmission through glass, to become as polarizable as that from incandescent platina, whereas he considers that the glass plate absorbed the greater part of those rays which otherwise would have heated the piles, and that thus the *apparent* polarization was increased.

(3.) Melloni describes his apparatus, and the precautions for avoiding secondary radiation, &c., employing piles of *split* mica, and throwing *parallel* rays on them by means of a rock-salt lens, having its principal focus at the source of heat.

He then enters upon the details of his results, in several series, with piles of different numbers of laminæ, and at different inclinations to the axis, (the source of heat being a lamp,) giving in each case the calorific transmissions in the rectangular positions, or proportions of heat polarized.   These are comprised in a series of eight tables, from which the author derives the following conclusions:

I. The proportion of heat polarized increases as the inclination of the piles is diminished.

II. It attains a maximum at a certain inclination.

III. This inclination is greater as the number of laminæ is increased.

He points out the close agreement of these results with the phenomena of light according to Brewster and Biot.

(4.) The author pursues a further series of experiments on polarization by reflection, and arrives at the conclusion that the angle of

complete polarization by reflection is very nearly the same for light and for heat.

(5.) If any diathermanous substance be interposed between the luminous source and the piles, the index of polarization does not vary with the substance employed.

This, he contends, proves that the *nature* of the heat does not alter its polarizability.

But also from direct experiment with the radiations from different sources he makes the same inference, employing, instead of a lamp, incandescent platina, metal heated to 400°, or boiling water, with the same results of uniform polarizability.

He maintains that the difference of polarizability by refraction, arising from the different refrangibility of the rays of heat, is too minute to be sensible.

And for all experiments on obscure heat he proposes to substitute as the source a black glass heated by flame.

(6.) On the depolarization he refers to Forbes's experiments, in which he contends the difference in the rectangular position is very small, but nearly equal with different sources.

He repeats the experiment, with black glass interposed, and finds the effects much greater, and nearly equal in the different cases.

He endeavors to explain Forbes's result of the difference with different sources, by secondary radiation.

Further, by the same method, (of interposing a black glass,) he finds the equal *depolarizability* of every kind of heat.

In an attempt to pursue the analogy of the *tints* of depolarized light, he acknowledges a failure, and thence considers the interference of calorific rays as not yet proved.

Upon these investigations the following remarks may be offered:

Under the first head, it should be recollected that Professor Forbes's first memoir was avowedly only directed to ascertain general facts, not numerical values; while, with regard to the more precise results of the second memoir, it would appear from the details there given that the secondary radiation could not affect the results. The screen between the source and the piles was removed only during the few seconds required for observing the first or impulsive arc of vibration, the time of which was wholly insufficient for the conduction of heat; besides, such an effect was disproved by direct experiment, as mentioned above.—(p. 352.)

Of the second point we shall presently have to notice a complete investigation by Professor Forbes.

As to the third, with this construction, the heat absorbed by the mica was very trifling; but by the more improved process since used by Professor Forbes, (p. 355,) we have seen this source of error is wholly got rid of. The employment of a pencil of parallel rays does not seem, upon consideration, materially to increase the intensity.

The fourth point is no more than what has been already established by Professor Forbes.

With respect to the fifth head, (including the most important part of these researches,) it must be observed, that the differences in the

nature of the heat obtained by the intervention of diathermanous sub-
stances are not the same as those between heat from luminous and
dark sources. And further, in the experiments mentioned with ra-
diations from different sources, no numerical results are stated. On
this point we shall presently notice some more detailed researches of
Professor Forbes.

The sixth point, on the subject of depolarization, is confessedly one
of the most delicate in the whole inquiry; but for the same reasons as
before, the effect of secondary radiation cannot be referred to as ca-
pable of having produced the differences observed.

Unequal Polarizability of Heat from different sources : Forbes.

Professor Forbes's third series of Researches of Heat appears in
vol. xiv of the Edinburgh Transactions, having been read before the
Royal Society of Edinburgh, April 16, 1838. It is also printed in
the London and Edinburgh Journal of Science, vol. xiii.

In the first section the author discusses the variable polarizability
of the different kinds of heat. The establishment of this fact was his
object in one portion of his second memoir. But these investigations
having been objected to by some, and opposite results (as we have
seen) obtained by Melloni, the author now repeated the inquiry with
every precaution. He rendered the rays parallel by a rock-salt lens,
as Melloni had done, and operated at a sufficient distance from the
pile; still the differences in the rectangular positions, when different
sorts of heat were employed, were as unequal as formerly.

Having varied the experiments in every possible way, he still
comes to the same conclusion as before, and gives the following results:

Source of heat.	Rays out of 100 polarized.
Argand lamp	78
Locatelli lamp	75 to 77
Incandescent platina	74 to 76
Incandescent platina with glass screen	80 to 82
Alcohol flame	78
Brass, at 700°	66.6
Brass, with mica screen	80
Mercury in crucible, at 450°	48
Boiling water	44

Melloni's opposite result of apparent uniform polarizability, the
author then shows, must necessarily arise from the use of mica piles,
consisting of a number of distinct plates superposed. Such a thick-
ness of mica modifies heat from dark sources in such a way as to give
the portion which it transmits the same character as to polarizabili
as luminous heat; whereas Mr. Forbes's results were obtained by the
use of mica split by *heat*, (as before described,) which includes so
many surfaces within a very small thickness that the polarized heat
is comparatively unaltered in its character. He shows directly that
these piles transmit heat from a lamp sifted by glass, and from brass

at 700° in nearly equal proportions, while mica .016 inch thick transmits five times less of the latter than of the former.

The second section relates to the dipolarization of heat. Pursuing the methods given in the first series, the author ascertained the proportion of heat dipolarized by five different thicknesses of mica. From the numerical results thus obtained he deduces the value of the expression in Fresnel's formula for the retardation divided by the wave-length, either of which quantities being assumed, the other becomes known.

In pursuing this calculation, the author finds that if the numerator (or difference of paths) be assumed to be the same as in light the length of a wave of heat would result three times as great as that for red light.

Upon this he is led into some important considerations bearing on the theory of undulations as applicable to heat.

Almost exactly similar numerical results were obtained for the heat from an argand lamp, from incandescent platina, and from brass heated to 700°.

At the anniversary meeting of the Royal Society of London, November 30, 1838, the Rumford Medal was adjudged to Professor Forbes, "for his discoveries and investigations of the polarization and double refraction of heat." And in the report of the council announcing the award a brief but appropriate testimony is given to the value of these researches.

Intensity of Reflected Heat: Forbes.

On March 18, 1839, Professor Forbes communicated some remarks to the Royal Society of Edinburgh on the Intensity of Reflected Light and Heat.

The theoretical law for the intensity of reflected light, originally proposed by Fresnel, has been confirmed on quite different grounds by the mathematical investigations of Mr. Green and Professor Kelland. Yet scarcely any attempt has been made toward its verification by direct experiment, except in the critical cases for polarized light originally assumed as the basis of the formula, and a few intermediate photometrical determinations by M. Arago. The uncertainty attending all photometry led Professor Forbes to conceive (about the end of 1837) that perhaps some confirmation might be obtained by ascertaining the law which prevails with respect to the intensities *of heat* in the corresponding cases; an analogy which seemed extremely probable from the facts already ascertained relative to the change of polarization, &c., before noticed.

In December, 1837, he made some first attempts, which were not altogether satisfactory. In the following winter he resumed the subject, and by a suitable apparatus for measuring the angles of incidence he endeavored to measure the intensity of heat reflected from surfaces of glass, steel, and silver; and though the results can hardly be yet considered completely accurate, yet in the case of glass the approximation to Fresnel's law is closer than any as yet exhibited by *photo-*

metrical observations; while the observations accord much better with the law of Fresnel than with that deduced by Mr. Potter. In the instance of *metals* Professor Forbes considers Mr. Potter's discovery verified, that the reflection is less intense at higher angles of incidence; he has not yet been able to verify Professor Maccullagh's inference, that it has a minimum before reaching 90°; and, lastly, he observes that the quantity of heat reflected from metals is so much greater than Mr. Potter's estimate for light as to lead him to suspect that all that gentleman's photometric ratios are too small; this would nearly account for their deviations from Fresnel's law. He has also made some attempts for verifying that law by observations on heat polarized in opposite planes.

Mr. Potter, it is well known, mainly founds his objections to the undulatory theory on the discrepancy between Fresnel's law for the intensities of reflected light and his own photometrical determinations. He has therefore naturally been led into some controversial remarks on Professor Forbes's results in a paper in the London and Edinburgh Journal of Science, to which Professor Forbes has replied.

Considering that the whole inquiry is as yet confessedly in an incomplete state, any further observations upon it in this place would be premature.

Conclusion.

In thus reviewing the different points of inquiry which have been of late pursued relative to radiant heat, and the several important discoveries with which that research has been rewarded, I have for the most part preserved, under each head, the chronological order.

The progress of discovery is here, I trust, too clearly marked to allow any real ground for these questions as to priority and originality, which have given rise to so much unhappy controversy between rival philosophers, or to the less open but equally lamentable manifestations of jealousy, in ambiguous expressions of claims, into which men of science have been sometimes betrayed. The dispassionate reviewer of the history of discovery at once best avoids all such controversial topics, and fulfils the demands of critical justice, by a simple but careful statement of facts.

In the present instance it appears to me that the share of credit due to the distinguished parties, respectively, who have co-operated to introduce the discoveries above reported is sufficiently well-marked, and certainly ample enough in each instance to confer the highest celebrity on those who have borne the chief portion of the labor.

To the continental philosophers belongs the first invention of the instrument, without whose aid none of these investigations could have been accomplished; while all the earliest and most important discoveries of the varying diathermancy of substances; the knowledge of the singular constitution of rock-salt, (which has placed a new instrument in the hands of the experimenter;) and the capital fact, disclosed by means of it—the *refraction* of heat from dark sources; together with the very singular phenomena of the changes in the nature of heat by transmission through certain substances; the remarkable effect of smoked rock-salt; the circularly polarizing power of quartz for heat—

all these important discoveries (besides others of minor value) are imperishably associated with the name of Melloni. Our own country as fairly and incontestably boasts, besides improvements in the apparatus and methods, many important results connected with the transmission of heat, accurate *measures* of its refraction, together with some indication of phenomena analogous to those of *diffraction*. In addition to these, the sole and undisputed credit of first unequivocally establishing the grand facts of the *polarization of heat*, even from nonluminous sources, by transmission through mica, through tourmaline, and by reflection; together with the peculiar and invaluable property of mica split by sudden heating, (a fact holding a parallel rank with that of the diathermancy of rock-salt;) the dipolarization of heat; its consequent double refraction and interference; its circular and elliptic polarization; its length of wave, and the production of that wave by transverse vibrations; the confirmation of the circular polarization by the rock-salt rhomb, and the peculiar effects of metallic reflection; these constitute the unquestionable claims of Professor Forbes.

On the main point in controversy between these two philosophers, the equal or unequal polarizability of heat from different sources, I have endeavored to place the facts and arguments clearly before the reader; but must confess my own conviction to be in favor of the *unequal* ratio of polarizability in the radiations from luminous and from obscure sources, while in some instances the apparently opposite results seem distinctly traced to known causes, and in others the equalization of the effects appears to depend on some of those modifications which the intervention of screens produces in the nature of the rays of heat.

The very remarkable class of phenomena just referred to is, perhaps, of all the recent discoveries, that which seems most singular and anomalous. That the same ray should acquire an entire change of property and nature by and in the act of simply passing through certain media seems little in accordance with any conception we can form of such radiation. Is this, we may ask, a real change of constitution, or is it a separation or analysis of the ray into its components?

I have elsewhere remarked that the terms "luminous" and "dark" heat are of somewhat barbarous appearance; and the objection is more than etymological, especially as we now find the luminosity of the source is not the essential characteristic of the qualities of the rays. And again, in the compound radiation from luminous sources there is included a considerable portion of "dark" heat as disclosed by its relation to surfaces in absorption.

The relations of heat to *surfaces* in *absorption*, and in the corresponding inverse effects of *radiation*, are among the most important portions of the subject, and I have in consequence been desirous to draw particular attention to the very valuable investigations of President Bache.

The properties which characterize the different species of heat (as we have seen) have been most remarkably developed and principally studied in the phenomena of *transmission*. A wide field is open to the experimenter in *connecting* these properties with those belonging

to the conditions of surface which produce the absorptive powers of bodies for different species of heat; and these, again, with those which mark the differences in conductive power, and perhaps also capacity for heat.

With regard to the establishment of a *theory* of the nature of radiant heat, we have seen, that the hypothesis of undulations certainly supplies a clue to a vast range of phenomena, especially those connected with polarization.

The question of the identity of the heating and illuminating radiations seems clearly negatived by many experiments, if we mean it to apply in the sense of one physical agent.   But if we refer to the possibility of accounting for the different effects by sets of undulations of the same etherial medium differing in their wave-lengths, this probably presents fewer difficulties than any hypothesis of peculiar heat.

We may, perhaps, suppose some other element besides the wavelength to enter into the explanation; or while we find that the heating effect is due to waves of greater length, it may also be true that the intensity or accumulation of waves which is necessary for producing the sensation of light follows a very different and much higher ratio than that requisite for producing heat, and that this latter effect may be produced in the highest intensity by longer waves of the same etherial medium but not sufficiently accumulated to impress our visual organs.

The difference in the polarizability of heat from different sources is not explained by the slight difference of refrangibility, and Professor Forbes is of opinion that we must, in consequence, look for its solution to a mechanical theory of heat in some respects, at least, different from that of light.   It is even a question of some difficulty why any portion of the heat should not be subject to the law of polarization which the rest obeys, unless we suppose the heating effect to be of so complex a nature that some part of it only is properly due to *rays* analogous to those of light, while the other part of the effect is produced by a mode of action altogether different.

To any such questions, however, we are hardly yet in a condition to give a satisfactory answer; but among the numerous points open to inquiry I have dwelt more particularly on those which appear to me pre-eminently to require more extended investigation before we can hope to obtain materials for constructing any substantial and unexceptionable theory.

REPORT FOR 1854.

PART I.

Having been honored, for the third time, with a request from the general committee to continue my former Reports on the state of our knowledge of Radiant Heat, from the date of my last report (1840) up to the present time, I feel bound to explain that the resolution conveying that request was passed so long since as the meeting at York in 1851, and that it was complied with on the understanding that the

report was not required immediately. But at the present date, being unwilling longer to delay, and finding that, from the pressure of other avocations, there is little probability of my being able to complete it, rather than withdraw altogether, I am induced to ask the indulgence of the Association for submitting only *a portion* of such a report.

Preliminary remarks.

Before commencing any analysis of recent investigations, it will be necessary (for reasons which will become apparent) to take, in the first instance, a very brief *retrospective* glance at certain fundamental distinctions affecting the whole subject, long ago pointed out, but too much overlooked by some writers since. It results from researches pointed out in detail in the two former reports, that, under the somewhat wide term "radiant heat," several totally distinct species of effect have been included.

(I.) The simplest form of radiant heat, and most properly so called, is that which arises simply from the cooling of a hot body, and which emanates from terrestrial hot bodies of *all* temperatures, from those which are the least elevated above that of the surrounding medium up to the highest incandescence or combustion, and is distinguished by two properties:

(*a.*) A tendency to be absorbed by bodies in proportion to a certain peculiarity of *texture* in their surface, but wholly independent of their color.

(*b.*) A total incapacity to pass by direct radiation through many media, such as glass, &c., though transmissible freely through rock-salt, and partially through certain others, called diathermanous media, as found by the experiments of Melloni.

(II.) At a certain stage of incandescence, *other rays*, also capable of exciting heat, begin to be given off *along with the former;* these are distinguished by the properties different from the former:

(*a.*) A tendency to be absorbed by bodies in proportion to the darkness of their *color*, or their absorption of light.

(*β.*) A power of transmissibility through *all transparent* substances, without diminution through colorless media, and in various proportions through colored media, according to their action on light.

(*γ.*) The power of exciting also the sense of *vision*, or being luminiferous.

This second species *coexists* in various proportions with the first, and is most copious from the most intensely ignited bodies.

(III.) Closely analogous to this last species, or identical with it, are the heat ing rays of the *sun*, characterized by the same properties, (*a.,*) (*β.,*) and (*γ.,*) and distinguished by being totally free from all admixture of the first kind, (or Species I.)*

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* This result was formerly obtained by exposing together a blackened and a white-washed thermometer to the solar rays, first with, and then without, a thick glass screen; in this instance there was only the smallest difference in the absolute values, and *none whatever in the ratio* of the rise of the two thermometers. In the brightest terrestrial rays there is a marked and often very great difference in *ratio* as well as amount This constituted the ground of my conclusions as to the heterogeneity of the species of rays in the latter case.

When by artificial means the luminiferous rays are *analysed*, some rays are found in which great heating power coexists with feeble illuminating power; but under the *same* conditions and for the *same ray* the heating is probably directly proportional to the illuminating intensity.

In this way it was that from the distinctions obtained in my original experiments, (1825,) I was led to describe generally the communication of heat as effected in *three* distinct ways:

(1.) By conduction (including what some term convection.)

(2.) By radiation (in the ordinary sense of the term) or mere cooling of a hot body, (Species I above.)

(3.) By the agency of light, whether from the sun or flames, (Species II and III.)

In the first instance, it was natural to regard these as so many distinct kinds of physical action producing heat, but more recent researches, especially those of Professor Forbes, have enabled us to connect them by the simple and uniform analogy supplied by the undulatory hypothesis. If we adopt the hypothesis of undulations of decreasing lengths, those of the greatest length correspond to the Species I, these continue to be given out as the temperature is raised, or as combustion proceeds more intensely, along with others of successively less wave-length, until we arrive at Species II, or those which are of the proper length to affect also the retina with the sense of vision, and at last the wave-lengths are too small to produce either luminiferous or calorific effects; but here they seem to obtain their maximum of chemical action. But rays of *all* wave-lengths thus continue to be given out *simultaneously*. They all produce or excite heat, more or less, when stopped or absorbed; and this probably depends in some direct ratio, on the greater wave-length simply; while the illuminating effect depends on some peculiar relation to certain wave-lengths only determined by some physiological conditions of the retina at present unknown. Substances which are transparent transmit freely rays of the visual wave-lengths, which of course carry with them their heating powers.

Opaque substances which are diathermanous transmit in the same way rays of the longer wave-lengths, but not those of shorter.

The action of the *texture* of surfaces seems purely mechanical, and probably influences the absorption of all rays, but its full effect is produced on the rays of longer wave-length; while that of *color* is purely *optical* and applies to rays of luminiferous wave-lengths only.

Refraction, polarization, interference, and the like properties of *light*, it would be easily seen, must be accompanied with just such indications of *heating* effect as might consist with the modification which the *light* in the respective cases might undergo. If the *light* were extinguished in certain conditions of polarization, or of interference, the *heat* would of course disappear with it, and the changes of intensity would be similar; that is, as regards either the solar rays (III) or those of species (II); and consequently, in the case of luminous hot bodies, in which Species I and II coexist, the effects in ques-

tion might be expected to occur, but only as applying to that *portion* of the rays which consists of Species II.

Thus, in a multitude of such cases, these effects might be very small, or altogether disguised and hidden. And, moreover, if they were real and sensible for Species (II), it would not follow that they existed for Species (I), yet the wave analogy would render it highly probable; and experiment has now proved it to be the fact. This constitutes the peculiar value of Professor Forbes's researches. In the instance of *luminous* bodies, then, all the combined heterogeneous species of rays undergo these modifications, though possibly in different degrees, and liable to modifications from the different nature of the media employed.

Yet many recent researches seem altogether to ignore these distinctions; and the "radiant heat" from different sources is commonly spoken of as if all of one kind, and of which a certain *per centage* is stopped or transmitted, in particular cases, by the interposition of certain substances; whereas the body of rays is *heterogeneous*, and certain integrant rays only are totally stopped or totally transmitted in the respective cases, showing not only quantitative but qualitative differences.

In many researches of this kind the "diathermancy" of bodies is spoken of, and experiments made to measure it, as if it applied indifferently to all species of heating rays, and without any reference to the consideration that for certain species of rays bodies are *diathermanous* simply in proportion as they are *diaphanous* to the particular luminous ray in question; while for other species of rays their *diathermaneity* follows some totally different, and as yet unknown law. The confusion of ideas introduced by the adoption of that term, unqualified by any reference to this distinction, and applied to general conclusions respecting "radiant heat," ought to be sufficiently manifest, yet has been but little considered. And in cases where the rays transmitted by a partially diathermanous body are in fact different in kind (agreeably to the above distinction) from those stopped, there is an ambiguity in the mode of expression; in fact it is not merely a *per centage* of rays, but an analysis of the whole heterogeneous body into more homogeneous or simple elements, *i. e.*, rays of different wave-lengths, or combinations of several such rays.

In my former report (1832, p. 332) it is mentioned that I had then repeated my original experiment (Phil. Trans., 1825) with a pair of thermometers of a peculiar construction made on purpose, in which very small differences were appreciable; but I did not there state any of the results. As attention has now been recalled to my original experiment, both by the repetition of it by Melloni with the thermomultiplier, and the more recent confirmatory remarks of Knoblauch, and as results obtained in this way (notwithstanding the superior claims of the thermal pile) may still possess some interest, it may not be irrelevant here briefly to state the results of a set of observations which have been lying by me since that period. The instrument was unfortunately broken soon afterwards. These observations were made on the rays of an Argand lamp; the thermometers, fixed on one frame

at the distance of about three inches apart, were exposed first with, and then without, a screen of plate glass one-eighth of an inch thick. The mode of observation was to note the rise of the thermometer blackened with China ink, in the time which it occupied the white-washed thermometer to rise 1° Fahrenheit.

Screen.		No screen.		Number of observations.
White.	Black.	White.	Black.	
° 1	° 3	° 1	° 1.6	20

Another set gave the *first momentary* rise—

| 0.5 | 2 | 1 | 1.5 | 5 |

A strong and curious confirmation of the heating power derived from *luminiferous rays simply*, has been furnished in a fact mentioned to me by Dr. Bennett, Professor of Physiology in the University of Edinburgh, viz: that in the exhibition of anatomical subjects by means of the oxy-hydrogen microscope, *the light* concentrated *by a lens*, at a few inches distance from the source, had so energetic an action as to burn up and destroy the specimens placed in the focus.

On the Theoretical Explanation of some former Experiments.

The experiments of Melloni (of which some general abstracts are given in my former report (1840, p. 335,)) certainly seem at first sight to present some anomalous results.

The main question which seems to arise is, whether the effects of heat, as compared with those of illumination, do not follow such widely discrepant laws as to make it difficult to ascribe them to the same set of waves; and this both with regard to terrestrial and to the solar heat.

To take a single instance: *rock-salt* is said to be analogous, for *heat*, to colorless media for *light; alum* is described as totally impermeable to "dark heat," and *partially* so to the rays from a lamp, that is, it may be *wholly impermeable* to *those* rays of the lamp which are identical with "dark heat" (Species I) in their relation to absorption, according to the *texture* of surfaces, and *wholly permeable* to those (Species II) which are associated with *light*, and produce this effect in proportion to the absorption of light by dark *colored* surfaces; or in the language of the wave-theory, wholly impermeable to rays of longer wave-length, and wholly permeable to *certain* rays of smaller wave-length.

In some of Melloni's experiments, rays from the lamp transmitted in *different proportions* by various screens, and then equalized, were afterwards found to be transmitted by *alum* in *similar proportions*. This he describes by the expression that "they possess the diathermancy peculiar to the substances through which they had passed." Yet the fact surely implies no new property communicated to the rays. It merely shows, that as different specific rays out of the compound beam were transmitted in each case by the first screen, alum, though impervious to the lower heating rays, (*i. e.*, of lower refrangibility or longer wave-length,) is permeable to those higher rays; and in different degrees according to their *nature;* an effect simply dependent on the heterogeneity of the compound beam from the flame. Again, with differently colored glasses peculiar differences of diathermancy were exhibited with the rays from a lamp, incandescent metal, and the sun; but not more various or anomalous than the absorption of specific rays of light by such media. And besides these considerations, it must be borne in mind that a smooth *blackened* surface is itself *unequally absorptive* for the *different rays*, acting (from its color) more energetically on those of a refrangibility within the limits of the visible spectrum, and which affect the eye as rays of light; and more feebly on the rays of lower refrangibility, and which act more energetically on bodies with reference to the absorptive *texture* of their surfaces.

As (according to my experiments) the solar heat is wholly of that kind which is freely transmitted through all colorless media along with the light, it does not appear that there would be any particular advantage in operating on the solar spectrum with a rock-salt prism. Melloni, however, with such a prism found, on interposing a thick screen of water, the most heating rays (*i. e.*, those toward the red end) intercepted, (as they are known to be by water,) and this caused the position of the maximum to be *apparently* shifted higher up the spectrum, even to the position of the green rays.

On the other hand, many colored glasses he found absorbed the rays in various proportions, yet they left the point of maximum heat unaltered, *i. e.*, though variously absorptive for the higher rays, they were not of a nature to stop the lower or most heating rays.

It appears also to be questioned whether the solar beam does not actually contain some rays of Species I, that is, of wave-lengths greater than any of the prismatic rays, including the ordinarily invisible extreme red. My original experiment on this point, above referred to, may probably be unworthy of comparison in accuracy with those which may now be made with the thermo-multiplier.

It would therefore be highly desirable if an experiment on the same principle as mine, viz: a black and white thermoscope exposed together, first with and then without a screen, were repeated on the *solar rays*, with a variety of screens, including especially rock-salt, with all the increased accuracy and sensibility now attainable by the use of two *thermo-multipliers*, by which the differences or identities of *ratio* in the two cases would be rendered evident in the most

satisfactory manner. It would be also highly important to make similar observations on the oxy-hydrogen and the electric light.

The theory of unequal wave-lengths, as the sole explanation of the different species of "radiant heat," whether solar or terrestrial, or in other words, the identity of the rays which produce alike the sensations of light, of heat, or other effects, each in some peculiar relation to the wave-length, certainly applies in a very satisfactory manner to a large portion of the phenomena. There may, indeed, be some minor objections or difficulties, but the only formidable outstanding objection seems to arise from a single result, announced long ago by Melloni, and referred to in my former report, viz: the fact that a certain kind of green glass transmits the solar *light* in high intensity *while it deprives it of all heating power.* This anomaly is indeed in itself so singular as to require very positive authority to substantiate it; and in M. Melloni's statement there is, as appears to me, a certain degree of vagueness, and it is not supported by any numerical results, or even any detailed account of the mode of operating*.

An alleged isolated fact of so extraordinary a character has long appeared to me to demand a strict re-examination. I had hoped that on presenting this report, in many other respects so imperfect, I might have been able to announce the result of such a repetition. But, unhappily, a variety of causes have hitherto prevented me from carrying it into effect.

Theoretical Refraction of Heat.

An important point bearing on the theory was indicated very shortly after the communication of my last report, (1840,) in my treatise "On the Undulatory Theory applied to Dispersion," (1841.) I have there shown (see pp. 71 and 122) that the formula for the refractive index in terms of the wave-length deduced from Cauchy's theory, furnishes a striking coincidence with Professor Forbes's determination of the index of refraction for a ray of dark heat in *rock-salt.* The formula in question way be expressed thus:

$$\frac{1}{\mu^2}=P-Q\left(\frac{\Delta x}{\lambda}\right)^2+R\left(\frac{\Delta x}{\lambda}\right)^4-\&c.,$$

where μ is the index, λ the wave-length, Δx the small interval of the molecules, and P, Q, R constants. From the nature of this formula, it is evident that as λ is increased the changes corresponding in the value of μ are very small; and when λ is very great, or $\frac{\Delta x}{\lambda}$ extremely small, the value of μ is susceptible of a limit, which will be

$$\mu_0=\frac{1}{\sqrt{P}}.$$

This will represent the physical condition, that as we take rays of successively greater wave-length, they will be crowded together into

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* On the Identity of Light and Heat, Taylor's Foreign Memoirs, Part III, p. 388.

one position of refraction, which will have a bounding or limiting position, beyond which no ray of however great wave-length can be refracted. This will be different for each medium, but will in general correspond to a refractive index not greatly below the index for the extreme red ray, and which is calculated, in my treatise just referred to, for various media. For rock salt it is a little lower than Professor Forbes's index for dark heat.

The data will be best seen as collected in the following table.[*]

Rock salt.

Ray.	Value of μ.		
	Obs. Forbes.	Theory, Powell.
Mean light	1.558	
Red	1.540	
$\lambda = 000079$		1.529
Dark hot metal	1.528	
Limit		1.527

M. Knoblauch's Researches.

Among the most important of recent researches on the subject of radiant heat, are those of M. Knoblauch, Professor of Natural Philosophy in the University of Marburg, which are not to be surpassed for elaborate extent and accuracy of detail. They are given in Poggendorff's "Annalen," January and March, 1847, and translated in Taylor's "Foreign Scientific Memoirs," Parts XVIII and XIX, 1848.

The memoir is of great extent, and is divided into six sections.

Section I is entitled "On the Passage of Radiant Heat through Diathermanous Bodies, with especial regard to the Temperature of the Source of Heat."

The author commences with a summary of the results previously obtained, in which he cites the results of Delaroche and others, without reference to the different interpretation which *must* be put upon them if the experiments and conclusions just referred to be admitted.

He observes that, from the experiments of Melloni, rock salt appears equally permeable by heating rays of *all kinds;* from those of Forbes, prepared rock salt would seem penetrated by heating rays in a greater degree when the source was at a lower *temperature.* Here I would observe that the *temperature* of the source, as such, manifestly bears no direct proportion to the degree of *luminosity,* it being perfectly well known that the temperature of luminosity is very different for different bodies; and these are also of very different illuminating powers. Now, as in all cases there are several different species of

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[*] On this point some notices were submitted to Section A, in 1840 and 1841. See Report, 1840, Sect. Proc., p. 14, and 1841, p. 25.

heating rays emanating at the same time from the source, some lumi-
niferous, and some not so, it is in no way a matter of surprise, or an ex-
ceptional case, that the transmissive power of rock salt, or of any
other substance, should bear no proportion to the *mere temperature* of
the source.

M. Knoblauch, however, instituted an elaborate set of experiments
to ascertain whether any such relation could be maintained.

The experiments were all conducted by means of the thermo-mul-
tiplier, which in this instance was constructed with especial precau-
tions to insure extreme accuracy and sensibility.

M. Knoblauch's first series of experiments included, as sources, al-
cohol flame, incandescent platinum, hydrogen flame, Argand lamp, of
which the *temperatures* were in the order of enumeration, the *first*
being the highest. The effect of each on the thermo-multiplier was
observed with the intervention of a series of screens, colored glass,
alum, mica, colorless glass, calcspar, gypsum, &c. The transmitted
effects varied, of course, with the different screens; but in every in-
stance they were *smallest* with the *first* source, and *increased* in the
order of enumeration, or in the inverse order of the temperatures.—
(Table I.)

In this series the real nature of the results is, in fact, evident from
the distinctions above drawn. The effect is simply dependent on the
light, or heating power of Species II, mixed, no doubt, to a certain
extent in some cases with that of Species I; which last probably *does*
bear a close proportion to the *temperature* of the source, but is in
these instances overruled by the effect of Species II being very feebly,
or not at all, transmitted by the screens.

In this series also M. Knoblauch found that the transmission through
rock salt was *not* exactly equal for all the sources, contrary to the as-
sertion of Melloni; the difference, however, is very small.

In the second series the sources were a vessel of hot water of dif-
ferent temperatures, from 93° to 212°, the radiating side being in
each instance covered successively with lamp-black, glass, wool, and
in each instance the thermo-multiplier being placed at a greater dis-
tance, in the ratio of the increased temperature, so that the effects of
direct radiation were equalized. In each instance, then, a series of
screens (the same as before) were interposed, each screen transmitting
a different amount of heat, but the results with each temperature be-
ing found equal.—(Table II.)

In the third series the sources were hollow cylinders of iron and of
copper closely surrounding the flame of a lamp, and heated by it to
several temperatures, from 234° Fahrenheit up to a little below red-
ness; in each instance the same equalization was effected as in the
last series, and the same series of screens gave varied effects for each
screen, but equal effects for each temperature, as in the last series.—
(Table III.)

The fourth series included platinum in successive stages of heat:
1st, dark; 2d, just red; 3d, yellow; 4th, partly white. In each in-
stance the series of screens was applied. In the 1st, 2d and 4th stages
of heat these gave uniform results, increasing with the heat; but in

the 3d case certain screens gave results *less* than in case 2, while another and smaller number only gave them *greater*.—(Table IV.)

The peculiar, and at first sight apparently anomalous circumstance, that platinum, at a stage intermediate between red and white heat, transmits through certain of the screens employed rather *less* heat than when at the *lower* stage of red heat, may however be explained, if we suppose that the rays given off at this intermediate stage are of such a wave-length as to be subject to a peculiar absorption by these particular screens. He then shows, from the conditions observed, that the effect of secondary radiation was fully guarded against.

Hence the author draws the conclusion, "that the passage of radiant heat through diathermanous bodies is not in immediate connexion with the temperature of the source, as was probable from previous experiments; but is alone dependent upon the structure of the diathermanous substance, which is penetrated by certain rays of heat in a greater degree than by others, whether this occurs at a lower or a higher temperature."

In all this there appears nothing to remark, except, perhaps, the observation that previous experiments might make a contrary result probable, which does not appear to be the case, since (as already observed) the temperature of luminosity has been long known to be very different for different substances. It were to be wished, that when the author speaks of "certain rays" being transmitted, he had more distinctly indicated the species to which they belong, but which seem to conform to the classification before noticed.

Some further experiments were made on sources of different form and size; cubes and cylinders of hot water of several magnitudes, and small and large flames, having successively different screens interposed between them and the thermoscope. The results were very uniform for all the sources, proving that the differences in question produced none in the transmission, as indeed might have been expected.—(Table V.)

Section II is "On the Heating of Bodies by Radiant Heat."

Here, after observing in general as a well-known fact, that the effect is greatly influenced by the *structure* of the surface, he observes:

"More recent experiments by B. Powell and Melloni have shown that one and the same body is not uniformly heated by rays of heat emanating from different sources, which exert the same direct action upon a thermoscope coated with lamp-black."—(p. 205.)

And he details some experiments, (Table VI,) showing that with a lamp a greater effect is produced on a surface coated with black paper than one with carmine, but with dark heat a less—a result which might indeed have been expected from what was well understood before the date of the researches alluded to, and which it was by no means their object to establish.

A set of experiments (Table VII) proved that for small thicknesses of coating (within the limit of those employed by Leslie and Melloni) the absorption of heat is proportional to the thickness.

Also, that "the temperature of a body, when the thickness in-

creases, is more raised the less it is diathermanous to the rays transmitted to it."

In Section III, "On the Property of Radiating Heat in Bodies," the author examines various cases in which the state of the surface, as in *cast* and *rolled* lead, smooth and scratched more or less closely, was observed as to its influence on the radiation, when the plates were kept heated by boiling water.—(Table VIII.)

Similar experiments with copper (Table IX) confirmed Melloni's conclusion, that the action is purely mechanical.

In another set of experiments (Table X) the increase of radiation with increased thickness of coating confirms the conclusion of Rumford and Melloni, that radiation commences from a certain depth below the surface.

The next set of experiments was directed to answer the inquiry, "Does the radiating power of one and the same body vary according as it is heated to a given degree by rays from different sources of heat?" The answer was distinctly in the negative, the sources of heat being a lamp and a hot cylinder, and the body heated and then radiating, being successively paper coated with carmine, and with lamp-black on the absorbing side, and lastly on both sides; also a plate of charcoal, and carmine spread upon wire gauze (Table XI;) and again, using carmine blackened next the thermoscope, and plain; and black paper coated with lamp-black next the thermoscope, and plain.—(Table XII.)

The author thus arrives at the conclusion, "under those circumstances in which the same bodies exhibit an unequal absorptive power their radiating power is one and the same; and those differences which have hitherto been observed when they are not heated to the same extent are, therefore, pure functions of the former and independent of the latter."

The conclusion, if we understand it rightly, would appear capable of being more easily stated, and, indeed, rendered at once obvious, from the distinctions at first pointed out; whence it is evident that the rays of Species I (as before described) from the cylinder, and those of Species II from the lamp, will necessarily act very unequally, according to the texture and color of the surface.

But when a body has received the radiation, from whatever source, and converted it into heat of temperature, as in these cases, to an equalized degree, it will necessarily radiate it again in an equal degree with the same surface, from whatever *species of rays* it was originally obtained.

Section IV is headed "Comparison of the Heat radiated from different bodies within a certain range of Temperature."

The author commences by remarking that "all former observations upon radiation have only related to the *quantities of heat* emitted by different substances at certain temperatures." The object of the present investigation is to ascertain whether there are any *qualitative* differences; or, as the author expresses it, "whether the heat which radiates from certain bodies at one and the same temperature, or within certain limits of temperature, is of a different kind, according

as it is emitted by different bodies, or is excited in them in a different way."

This inquiry was pursued by a series of experiments, in which (1) a vessel of water at 212° and (2) the flame of a lamp had in *contact* with them various *adiathermanous* substances, such as metal, porcelain, leather, wood, &c., in the heat given out, by which the series of screens gave exactly similar series of effects. As also with the heat from a hollow cylinder of copper or iron surrounding a flame. But when the direct radiation of the flame was employed, the series of results were in proportions considerably different.—(Tables XIII to XVI.)

The author, indeed, remarks, at the conclusion, that these last differences are due to the heat *transmitted* by the screens, *i. e.*, the heat conveyed by the luminous rays; as, indeed, would be manifest, according to the views at first noticed.

In another series (1) an adiathermanous body at several temperatures, from 88° to 212°, (2) the flame of a lamp and (3) a metal cylinder round a flame below 234°, were severally tried with the same series of screens; the results in cases (1) and (3) being found exactly similar, in (2) in a very different proportion.

The same sources were next tried with two screens interposed; the first being, successively, metal with holes, silk, ivory, &c., and each of these combined with the several screens of the former series. In all cases the results gave *proportional* series.—(Tables XVII to XIX.)

Another series was conducted with, 1st, a flame, and, 2d, water at 212°; each in succession with a screen used first plain and then blackened, the screens being black glass, lac, ivory, paper, &c.; the results being always less with the blackened surface, very similar in each case, and all less with the hot water.—(Table XX.)

Another set, with a heated mass, and with the hand at its natural temperature for sources, gave similar results with various screens.—(Table XXI.)

This, the author says, "disproves the opinion of Forbes, that the heat emitted by boiling water and the hand must be considered as different." I am unable to find in what respect Professor Forbes supposes them different.

At the conclusion of this section the author adverts to two *practical* inferences from what has preceded.

(i.) The fact that the amount of heat absorbed by a given body is the same, from whatever source it was derived, is important in regard to the determination of *specific heats by the colorimeter;* for if the heat absorbed by the ice were different as it might be derived from *different sources*, no correct measure of specific heat would be obtained. That it is not so insures the accuracy of the results, so far as *this* source of error is concerned.

(ii.) The second application is, that "these results lead to a new method of ascertaining whether any substance transmits rays of heat or not," (235;) that is, of determining whether any given instance of transmission of heat is really due to *diathermancy*, or is merely secondary radiation.

Thus, to determine whether ivory, *e. g.*, is really diathermanous; the source of heat is a known adiathermanous substance kept heated by a lamp; the effect is observed; a known diathermanous screen is then interposed and the effect again observed; the ivory is then substituted for the diathermanous body, and the direct effect equalized to the former; the same screen is then interposed, but now a *greater* effect is transmitted. It follows that part of the original heat is *transmitted directly* by the ivory, along with that *radiated from* it; or *the ivory is diathermanous.*

Section V, on "the Comparison of the amount of Heat diffusely reflected by different bodies," refers to that kind of irregular reflection, or dispersion (as it has been sometimes called) of the rays from the roughened, or at least unpolished surfaces of bodies, and which is distinguished from regular reflection, which is governed by the law of equal angles of reflection and incidence by occurring equally at all angles. And the object is stated by the author to be the determination "whether heat, or diffuse reflection, experiences changes in its properties which distinguish it from that which is not reflected."— (384.)

The heat being incident on a rough surface is, of course, partly absorbed and radiated again; to guard against error from confounding this with the proper *reflected* heat, various and careful precautions were adopted.

The author then proceeds to detail the observations, which are of voluminous extent, and the results recorded in a long series of tables.— (Tables XXII to XXXII, inclusive.)

In all this first series the source employed was an Argand lamp without its chimney, and in all cases the mode of operating was similar.

The unpolished surface under examination was exposed to the rays of the lamp at different distances and at different inclinations, and the direct effect noted; the experiment was then repeated with the insertion of a series of variously diathermanous screens.

The substances used as reflectors were extremely varied; such as bodies agreeing in one property and differing in another, or totally homogeneous, or totally heterogeneous.

Thus an immense range of substances of animal, vegetable, and mineral origin, opaque or transparent, of all colors and textures, were examined, and the agreements or discordances, according to their various properties, were found extremely varied and curious.

By these results, the author observes, "it is placed beyond all doubt that heat, on diffuse reflection, is very differently modified by some bodies to a great extent, while by others it is unchanged."— (400.)

And these results completely confirm the position already advanced, "that the transmission of heat through diathermanous media depends solely upon the nature of these bodies, by virtue of which they transmit some rays more easily than others."—(402.)

In a second series the same subject is continued with reference to *different sources of heat*, which were, besides the lamp, platinum at a

red heat, the flame of alcohol, and a metallic cylinder heated by being placed over the flame of a lamp, as before:

(i.) The reflections were first made from that series of substances which had displayed the *greatest differences* in the former instance. The results are given in a similar tabular form.—(Tables XXXIII to XXXVIII.)

The author's general conclusion is, "that the modifications which heat experiences on reflection are very considerable in the case of the heat emanating from an Argand lamp; that with the heat of red-hot platinum they diminish; with the heat of the flame of alcohol they are still less; and in the case of the heat emitted by a heated iron cylinder, of whatever temperature it may be, between 79° and about 234° Fahrenheit, they absolutely vanish."—(407.)

Or more generally, "the changes undergone by heat on diffuse reflection are occasioned both by the nature of the sources of heat and the properties of the reflecting body."—(408.)

(ii.) It remained, as the author expresses it, to determine "whether those surfaces which exert a *similar influence* on the rays of the Argand lamp, *i. e.*, which they reflect in such a manner that the heat reflected by the one is transmitted by the diathermanous media used for testing in the same proportion as that reflected by the others, would also reflect the heat from the other sources, so that the rays reflected by them would pass through these substances in the same manner."—(409.)

The results of these experiments are given in detail.—(Tables XXXIX to XLIV.)

The question then arose with regard to the *explanation* of these phenomena: Are they owing to any change undergone by the rays in permeating the diathermanous substances, or were they "the consequences of a *selective absorption* of the reflecting surfaces for certain rays of heat transmitted to them, as appeared the most probable view from the experiments of Baden Powell and Melloni?"—(415.)

This question the author proceeds to examine by a detailed comparison of the foregoing results, exhibited in new tabular arrangements of every case.—(Tables XLV to LI.)

Upon a minute discussion of all these results, the author decides in favor of the second alternative; or "that the changes experienced by heat on diffuse reflection are merely the result of a selective absorption of the reflecting surfaces for certain rays of heat transmitted to them."—(423.)

The author also adverts to some other inferences from these experiments; as that, "excepting charcoal and the metals, it cannot be said that any body reflects heat better or worse than any other, because this relation varies with each kind of radiation."

Again: certain bodies of the same color reflect different kinds of heat, and others of different color the same kind; and this is connected with the fact that "every luminiferous source of heat emits a large number of invisible rays, which are susceptible of reflection and affect the thermal pile."—(424.)

These, and some other remarks on the dissimilarity in the diffuse

reflections of luminiferous and calorific rays, are not perhaps expressed with that clearness which might be wished.

The subject of Section VI is "On the Sources of Heat;" which is further explained to relate to the differences in the nature of the rays, or in general the *heterogeneity* of the rays, emitted from one and the same source at the same time; and the differences in this respect of different sources.

From the previous experiments the author concludes, in general, that "the variety of the rays of heat emitted is greatest with the Argand lamp, less with red-hot platinum, still less when the flame of alcohol is used, and has entirely disappeared with the cylinder heated to 212° Fahrenheit."—(426.)

But he now proceeds to test and extend such conclusions in another way, viz: by the differences exhibited by the rays in the different action of diathermanous bodies upon them, according as they have in the first instance passed through certain diathermanous bodies, or proceeded direct from the source. The differences thus exhibited give increasing proofs of heterogeneity.

And one more important point belonging to this inquiry he investigated, by platinum heated to successive stages, (1) below 234°, (2) at a red, (3) at a yellow, and (4) at a white heat; while in each case the heat was reflected diffusely by various surfaces, and in every instance intercepted by the same series of screens.—(Tables LII to LV.)

From these he draws the conclusion that the differences which the rays evolved at the successive stages exhibit after diffuse reflection, on transmission through diathermanous media, are in every instance greater at the stage (4) than at (3;) these greater than at (2,) and these than at (1.) Or, in general, that the heat emitted by platinum at these successive stages is successively more heterogeneous as we advance from the lower to the higher.

Again, with the same body it is not in the mere proportion of the increase of *temperature*, as such; nor in different bodies does it follow any proportion to the temperature (as before observed.)

The author hints generally at the relation between these degrees of heterogeneity and the differences in the nature of the *luminous* rays emitted; but without laying down any very precise or clearly drawn distinctions as to their characteristic properties.

Transmission of Heat through Crystals.

Melloni had raised the question whether in one and the same body, in a crystal, for instance, the quantity of radiant heat transmitted was different along the different axes. In experimenting on this subject, in connexion with M. Knoblauch, he found that with transparent rock crystal and with calcspar no difference of this kind was detected.

In a memoir (in Poggendorff's Annalen, 1852, No. 2, translated in Taylor's Foreign Memoirs, November, 1852, and February, 1853,) entitled "On the Dependence of Radiant Heat in its Passage through Crystals on the Direction of Transmission," M. Knoblauch has pursued the subject commenced by Melloni.

He took in the first instance a cube of *brown quartz*, having two of its sides perpendicular to the axis of the crystal. The rays of the sun were reflected into a room by a heliostat, and the mirror being metallic they remained unpolarized, and after passing through the crystal, (which could be turned with its axis in different directions with respect to the rays,) were received on a thermo-multiplier. The effect was considerably *less* in the direction *perpendicular* to the axis. The same result was found with *beryl;* but with *tourmaline* the effect was *greatest* perpendicular to the axis.

The rays were next polarized by a Nicol's prism, before incidence on the crystal. When the plane of polarization *coincided* with the axis the heat transmitted was the *same* in all directions. But when *perpendicular to the axis the differences* before observed in the unpolarized rays were *increased*.

No difference could be detected between the cases when the rays passed *along the axis* of the crystals, and the plane of polarization was respectively horizontal or vertical.

M. Knoblauch now proceeded to try whether the rays which exhibit *quantitative* differences as above, would show *qualitative* differences in the same cases, that is, differences in the power of transmission through diathermanous bodies.

The diathermanous bodies employed for screens were blue, yellow, red and green glasses.

After transmission through a brown rock crystal, the proportion of rays penetrating the different screens differed in the two positions, parallel and perpendicular to the axis, only within errors of observation.

With the rays previously polarized (as before) sensible differences were observed, the plane of polarization being vertical.

The heat was in all cases greatest through the yellow and red glasses, rather less through the blue, and least through the green.

Another set of experiments, in which the plane of polarization was *horizontal*, gave no sensible differences.

Rays traversing the crystal along its axis, also exhibit no differences, as was likewise the case with rays perpendicular to the axis.

With *beryl* similar observations were repeated; with blue glass the difference is very small, for yellow rather greater; the author infers a real difference.

With polarized light and with the plane of polarization vertical, the differences are much greater with both glasses.

With the plane horizontal no difference was found.

Common light, passed through two cubes of beryl according as their axes were parallel or perpendicular to each other, gave great differences with the yellow and blue glasses, and in an opposite ratio in the respective cases.

With *tourmaline* exactly similar results were obtained in the corresponding cases.

With dichorite also the author says, "so far as the examination extended, qualitative differences dependent on the direction of transmission have also been observed."

At the conclusion the author makes some observations in explana-tion of the phenomena; these may be more briefly and clearly expressed thus:

The unpolarized rays incident along the axis of the crystals examined or parallel to it undergo a certain absorption dependent on the nature of the crystal; and this is no further modified, since in this direction there is no double refraction.

But if the ray be incident perpendicular to the axis, it is divided into two, oppositely polarized, and these are differently absorbed.

That which has its plane of polarization parallel to the axis has the same absorption as along the axis. That which has its plane of polari-zation perpendicular to the axis is absorbed more or less than the former, in different degrees in different crystals, and for the different component rays.

When the unpolarized rays are incident, then the result is com-pounded of these separate effects.

When rays previously polarized are employed, the effects are displayed singly.

The author considers that these distinctions fully account for the observed phenomena.

Upon this we may observe—

The investigation cannot with correctness be called one on the transmission of "radiated heat" in general; it is restricted to that peculiar form or case of radiation which is manifested in the *solar rays*, and proves nothing as to the radiation from hot bodies, or even that conveyed in the rays from artificial lights, unless, as inferred, by analogy. All the differences observed depend simply on the unequal absorption of the rays of light by the crystals and the colored glasses.

Melloni's recent Experiments.

In the "Comptes Rendus," No. 10, p. 429, March 6, 1854, Melloni gives some brief remarks in reply to certain objections raised by MM. Provostaye and Desains against the accuracy of experiments with the thermo-multiplier on the passage of heat through screens.

He points out as the sources of discrepancy the *oblique* passage of the rays through a thick diathermanous screen, which is greater or less according to the distance, and gives different effects of internal reflection and absorption in different cases.

To show that the errors objected arise solely from this source, he describes a careful repetition of his experiments in which it was guarded against.

The series of experiments included the usual set of sources, viz:

(1.) The flame of an oil-lamp.
(2.) Incandescent platina kept up by vapor of alcohol.
(3.) Plate of copper heated by lamp.
(4.) Vessel of hot water.

Equalizing the effects on the thermo-multiplier by changing the dis-tance and interposing a rock-salt screen, the diminution of effect appeared the same for the first three sources, but greater for the fourth.

But this last result he contends was simply due to the greater prox-
imity of the source, and consequent greater differences of inclination
of the rays; and when equalized in this respect, the difference in
question disappeared, or might even be reversed.

In the same notice Melloni refers briefly to other results which have
been obtained by means of rock-salt.

(1.) That with a prism of rock-salt the maximum of caloric effect in
the solar spectrum is thrown further from the limit than with other
"prisms thermochroiques."

(2.) That the radiation from the sun diminishes from the centre to
the circumference; that the radiation from the spots is less than from
the rest of the surface, and that of the equatorial region of the sun
greater; these results were obtained by M. Secchi.

It may be right to add that I have been informed that in a work
entitled "Thermo-chrose," not long since published at Naples, M.
Melloni has somewhat modified his former opinions, and seems disposed
to assent to the doctrine of the identity of the rays which produce
light and heat, or heat alone, according to their greater wave-length;
and has explained and reconciled some of his former difficulties. I
regret to be unable at present to give a more precise account.

Mr. Draper also, I am informed, has been led to admit that the
chemical effects belong properly to the same set of rays, differing only
in the characteristic of peculiar wave-lengths.

On the whole, the question of the evidence for and against this
theory is one eminently deserving of being fully discussed. I can only
pretend in this imperfect report to have suggested some of the ma-
terials which may assist in forming some judgment on this point.

Analogies of Transmission of Light and Heat by Waves.

The very important researches of Mr. Joule on heat and the dy-
namical theory to which they lead, though referring directly to heat
in its action on bodies as *temperature* or as *latent heat*, yet are not
without a bearing on the subject of *radiant heat*, as has been in some
degree pointed out in the excellent address of our President of last
year.— (Report, 1853, p. xlvii.)

Mr. Joule's theory, though not as such dependent on the wave-
theory of heat, is yet eminently in accordance with it, and so far lends
it much support. If we suppose the temperature of a body to arise
from vibrations of its molecules, such vibrations may be excited in it
by the vibrations of an ethereal medium surrounding and penetrating
that mass of matter. In this respect the close analogy with sound is
preserved. These vibrations of heat, however, produce mechanical
changes in the constitution of the medium. They cause it to expand;
i. e., they drive its molecules to greater distances apart; and when
carried to a certain extent, cause a fresh and sudden separation to a
far greater extent, accompanied with a new arrangement of these
molecules, or a change of *state* in the body from solid to fluid or from
fluid to aëriform. Here the analogy with sound ceases to hold good,
except so far as that a temporary new arrangement of the molecules

is occasioned by the sonorous vibrations. The transmission of lumi-
niferous waves has a velocity which, though enormous, is capable of
measurement. Whether that of the longer non-luminiferous but
caloriferous waves is the same has not been, I believe, experimentally
verified, but must theoretically be supposed the same, unless, indeed,
it be only *approximately* the same for waves within the narrow limits
of the luminiferous scale, and diverge from that value beyond those
limits.

Again, the passage or process of the vibrations in a body receiving
heat *is slow ;* to this *conduction* of heat there does not seem to be any-
thing strictly analogous in *sound.* In general the passage of light
through transparent bodies excites in them no vibrations capable of
affecting our eyes with the sense of light; *i. e.,* the medium does not
become luminous, unless we except the case of the phosphorescence
of fluor-spar and some other bodies after exposure to light. So far,
indeed, as the transparency is *imperfect,* and in all *opaque* bodies the
vibrations which constitute light are stopped, or changed in such a
manner that they give rise to vibrations in the body constituting *heat;*
just as those longer vibrations do which constitute that species of
radiation which is derived from the mere cooling of a hot body; but
this does not occur in *transparent* bodies. It would seem to be the
law that if a ray, or a series of waves of the proper length to be
luminiferous, impinge on an opaque body, they communicate vibra-
tions to its molecules, which again transmit to the surrounding ether
other waves of *greater length,* which in like manner traverse space and
can again excite vibrations in bodies on which they impinge; or if
from any source a body have internal *vibrations of a certain intensity*
(whether forming *waves,* or of what lengths, we have no means of
deciding,) it can transmit to the surrounding ether vibrations which
constitute waves of lengths *greater* than a certain given length, viz:
that which belongs to the deepest red luminiferous rays. If its
internal vibrations are increased in intensity beyond a certain point,
it then acquires the power of communicating (*in addition* to the last)
other vibrations to the ether forming waves of other and smaller
lengths, so as to give rise to *light.*

Origin of the Solar Heat. Professor W. Thomson's *Theory.*

Some very important speculations have been brought forward on
the *source,* and thus bear on the *nature,* of the solar heat, by Professor
Thomson[*], in immediate connexion with the theory of Mr. Joule, and
on the principle that the energy of the heat thus emitted must be
accompanied by an equivalent expenditure of mechanical force. On
this principle he institutes numerical calculations, the main results of
which, together with a brief exposition of the principles, may be
given as follows:

The kind of force acting, or the source of solar heat, the author
conceives may be expressed by several hypotheses, each of which he
examines:

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[*]Transactions of the Royal Society of Edinburgh, vol. xxi, part 1. On the Mechanical
Energies of the Solar System.

I. The supposition of the sun being simply a body intensely heated and losing its heat by radiation or simple cooling.

This he considers quite untenable, as well on theoretical grounds advanced in some other papers, as on the simple consideration that if this were true the sun would be extinguished in a very short time.

II. The hypothesis of chemical action or combustion of any kind.

Supposing one of the combining bodies to be supplied from any atmosphere, the products of combustion would be so enormous as to choke the fire, if gaseous, by preventing the access of the air in question, or, if solid or liquid, by preventing the supply of fuel; and according to the mechanical theory before mentioned, a numerical calculation shows that the whole mass of the sun could scarcely last 8,000 years without being all consumed, if generating, by its own burning, the heat which is actually emitted. Hence if the sun is a fire, the fuel must be supplied from external space. But a mass of coal or iron or potassium could not reach the sun from external space without generating thousands of times as much heat from its motion as it could possibly do by its combustion. Combustion is probably, therefore, insignificant, if it exists at all, as a source of solar heat.

III. The hypothesis of meteors falling into the sun and expending force *mechanically* has been started by Mr. Waterston*, who supposes such bodies to be attracted and fall directly into the sun from remote *extra-planetary* regions.

The supply of meteoric matter necessary according to this theory is estimated to amount to such a mass as would cover the sun's surface to a depth of thirty feet in one year.

The author, however, considers it probable that meteors actually fall into the sun, not directly from distant spaces, but by the action of a resisting medium surrounding the sun, which *contracts* the orbits in which they are revolving round him. He conceives that these meteors must be moving *within* the limits of the earth's orbit, or we should be continually struck by them, and that they are probably the matter of the zodiacal light.

It is, however, quite conceivable that that cloud of small planetary masses may once have extended beyond the limits of the earth's orbit, and thus in remote periods the earth may have been exposed to such falls of them as to have materially raised its temperature; and hence a possible source of those high temperatures which once existed.

But to return to the effects of the resisting medium. Owing to its retardation, the approach of these bodies to the sun is gradual, and on this hypothesis a calculation similar to the former would give the result that the sun must be covered to a depth of sixty feet in a year, or a mile in about eighty-eight years, which would occasion an increase of 1″ apparent diameter in 40,000 years—a change, of course, utterly inappreciable to observation.

The amount of matter thus abstracted by the sun would be equal to the mass of the earth in about forty-seven years; but it is quite conceivable that a quantity of one hundred times this amount would

*British Association, Hull Meeting, 1853.

not be missed from the zodiacal light. Thus the sun's heat might be kept up for five thousand years to come at least. This transfer of matter to the sun coming from a source within the earth's orbits would not affect the conditions of the system as to the effects of gravitation.

Some meteors may possibly come direct on the sun from the extra-planetary spaces, but the quantity of such is probably very small in comparison with those that have been revolving in approximately circular or elliptic orbits before falling in.

If we imagine a dark body moving through space and coming into a locality abounding with meteors, their impact may raise it to incandescence, which will cease when it moves out of that space. Thus the author suggests a possible explanation of variable stars.

(Addition I.) The author gives a calculation of the quantity of matter necessary to be added to the sun on the *extra-planetary* hypothesis, and finds it gives too great an increase of central force to consist with the historical conditions of the earth's motion. He concludes the supply must have been from *within* the earth's orbit for thousands of years at least.

(Addition II.) He shows that the resistance must be very small even close to the sun, since such light bodies as comets pass through it at perihelion.

The solar atmosphere may be conceived to be carried round in a vortex by these revolving masses, but not more rapidly than a planet would be at the same distance. Hence the meteors must long continue to revolve before reaching the sun, *and must get so near as to be completely evaporated before they fall in.*

Hence the solar heat is produced, not by solids *impinging on the sun, but by the violent friction of the rotating vortex of evaporated meteor matter.*

(Addition III.) The temperature of the different parts of the sun's surface may undergo great changes from the eddies and streams occurring in this revolving mass. Hence many of the appearances of the solar spots and streaks, &c.

(Addition IV.) "On the age of the sun." At the rate of meteoric incorporation above calculated, the present rotation of the sun would be produced from rest in thirty-two thousand years. We may infer (since it appears very improbable that the sun has had a contrary rotation destroyed by meteoric incorporation) that the kind of agency now going on cannot have been going on and alone generating heat at the present rate for more than that period. For the future we know that the mass of the zodiacal light is small, in comparison with that of the sun, from its producing no sensible perturbation on the planets, and we may be sure it cannot keep up the supply for three hundred thousand years. The sun's rotation has been by no means accurately determined; it may possibly vary to an amount which future observations may detect, and thus test the theory.

Density of Æther.

Another speculation,[*] closely connected with the former and the general subject of radiation, has been pursued by the same author, on the probable density which can be assigned to the *luminiferous* and *caloriferous* æther.

This speculation is founded, like the former, on the data furnished by Pouillet's Researches on the Solar Radiation, and Joule's theory of the mechanical energy equivalent to the effect of heat produced. The calculation turns on the assumption that the velocity of *vibration* can only be a small fraction (probably not one-fiftieth) of the velocity of the *propagation of waves*, and from the *velocity of vibration* we may calcuate the *density*, or conversely.

Hence the author conceives that we may assign a *limit*, and that *a cubic foot of luminiferous æther, at the distance of the earth from the sun, cannot contain less than* $\left(\dfrac{1}{1560 \times 10^{17}} \right)$ *of one pound of matter*.

With regard to the results of Professor Thomson, especially when the novel character of some of the reasonings is taken into account, some difference of opinion may reasonably be expected. There are certainly many considerations involved which might suggest important topics of discussion. On these it is not my purpose to enter. I will merely remark, that in all these investigations the essential point is *the expenditure of mechanical energy in producing vibrations*, of whatever kind. The whole question then assumes a more *strictly mechanical aspect*. The sure indication that this entire branch of science is in a state of approximation at least towards that stage which characterizes the perfection of any branch of physical knowledge, when all its varied phenomena shall be shown to be susceptible of analysis up to simple combinations of the elementary laws of force and motion.

I would merely add that, in speaking of the effect of the evolution *of heat*, there is nothing in Professor Thomson's conclusion which restricts them to any *one species* of heat. The essential point is the production of *vibrations;* and his results are thus in entire accordance with the theory which refers all kinds of heating effect to the stoppage or absorption of rays; in other words, the extinction or destruction of the vibratory motions, constituting rays of different wavelengths, some of which are also within those limits, and belong to that part of the scale, which renders them capable of affecting our eyes with the sensation of vision; which (as already remarked) considerations on all hands seem now tending to show is the most probable hypothesis on the subject.

Radiation of Heat from the Zodiacal Light and from the Comet of 1843.

Some interesting observations on these points are given by M. Matthiessen in the "Comptes Rendus," April, 1843, (vol. xvi, p. 687.)

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[*] Professor W. Thomson "On the Possible Density of the Luminiferous Medium," &c.' Transactions of the Royal Society of Edinburgh, vol. xxi, Part I.

On the 27th of March M. Matthiessen placed at the focus of a *concave mirror* of one metre diameter an *air-thermometer*, which showed a similar rise above its indication in other positions, when the axis of the mirror was directed to the *zodiacal light*.

He next substituted for the thermometer a thermo-electric pile with great precautions; which, having its condensing cone

Directed to the nucleus of the comet, gave a deviation of······	2°
Directed to the zodiacal light near its summit···············	10°
Directed to the zodiacal light at base·····················	12°
Directed to the part of sky over the sun····················	3°
In other directions···· ·····························	0°

On removing the condensing cone to try whether the effect was due to atmospheric causes, he still found towards the base of the zodiacal light 2° or 3°.

Instead of the mirror he next used a *flint-glass lens*, 56 centim. diameter, 16 centim. focal length.

With the thermo-electric pile, as before, this gave,

Directed to zodiacal light, summit···················· ········	2°
Directed to zodiacal light, base····························	4°
Directed to sky over sun································ ····	0°

With a *tallow candle* at 10 metres distance (whether with the lens is not stated, but probably with it, as the experiments would not otherwise be comparable)—

With the condensing cone, deviation························ | 15°

This, he observes, shows "combien est minime la quantité de chaleur envoyée par la lumière zodiacale, et que l'influence de la comète doit être réellement imperceptible par notre température."

It was perhaps this somewhat ambiguous sentence which led Humboldt to represent these experiments as showing no sensible effect due to the zodiacal light.—(" Cosmos," Note 98, p. 394, vol. 1, Sabine's translation.)

In the face of the experiments, however, we must adopt another interpretation; and perhaps what the author means is the distinction between the *radiant* heat affecting the *thermoscope*, and the *temperature* communicated to the *atmosphere;* which are manifestly different things.

The experiment with the candle, if made (as seems to be implied) with the lens, is an important verification of the fact of the *heating power belonging to light* from terrestrial sources.

The result with the zodiacal light also shows that at least a portion of its effect *is of this species*, and not dependent on its mere loss of heat as a *hot* body cooling.

It is, however, but right to add, that indications of such extreme delicacy as those here referred to have been looked upon by some physicists as almost too liable to uncertainty to be entirely trustworthy.

DESCRIPTION OF THE MAGNETIC OBSERVATORY
AT THE SMITHSONIAN INSTITUTION.

[This observatory is supported at the joint expense of the Smithsonian Institution and the Coast Survey. The description here given is by J. E. Hilgard, esq., who had charge of the observations.]

The instruments of this observatory are designed to give, by means of photographic self-registration, a continuous record of the variations in the direction and intensity of the earth's magnetic force. They are similar to those employed for the same purpose at the Greenwich, Paris, and other European observatories, and also at Toronto, on this continent, and consist of a freely suspended *declination magnet*, a *bifilar magnetometer*, after Gauss' design, and a *balanced magnetometer*, after Lloyd; each provided with the apparatus for photographic self-registration, invented by Mr. Charles Brooke, under whose direction the instruments were constructed, as has been heretofore stated in the reports of the Secretary of the Smithsonian Institution.

The general plan of the photographic registry is as follows: each magnet carries a concave mirror, (speculum,) in one of the conjugate foci of which is placed a source of light; a pencil of rays is reflected by the mirror, and concentrated in the other focus upon a sensitive sheet of paper wrapped about a cylinder, which revolves in a certain time about an axis parallel to the direction of the changes of the magnets. The point of light traces a curve corresponding to the movements of the magnet; the angular value of the ordinates of the curve is, of course, measured by a radius equal to twice the distance of the mirror from the cylinder; or, in other words, we have an index for the movements of the magnet equal to twice that distance; which index or tracer, being without inertia, and acting without flexure or friction, is vastly superior to any mechanical arrangement by which such registry could be effected.

The magnetic observatory is contained in a small building expressly constructed for the purpose, situated about 150 yards southeast from the south entrance of the Smithsonian Institution. No iron whatever has been used in its construction—copper or brass being employed where metal was necessary. The instrument-room, occupying a space of twelve by sixteen feet, is wholly beneath the surface of the ground; it is closed in by an nine-inch brick wall, between which and the outer wall of two feet in thickness a space of two feet intervenes on three sides. This space is covered in at the surface of the ground by a coping, with a grating at two corners, and communicates at the bottom with the instrument-room by small air passages. A considerable uniformity of temperature is thus secured, as the air has to descend about nine feet before it enters the room. A flue at the north side of the room, communicating with a chimney, secures the requisite ventilation. On the fourth (the south) side of the room is the door and an ante-chamber, into which the stairs descend, and where there is a trough with supply of water from a cistern for performing the rinsing and washing necessary in the photographic

25

operations. The upper part of the building is occupied by a similar ante-room and a laboratory, where the preparation of the sensitive paper and other photographic operations are performed. A soapstone stove, with copper pipe, supplies in winter the necessary warmth to the room. The windows have close shutters, with an opening corresponding to a yellow pane of glass, which admits sufficient light for operating in the room, while it in no degree affects the sensitized paper.

No sunlight whatever penetrates to the instrument-room, which is only dimly lighted by the reflection at the ceiling of the burners used for the registration.

GENERAL DESCRIPTION OF INSTRUMENT ROOM.

The annexed diagram, Fig. 1, illustrates the disposition of the instruments. D is the declination magnet; H the horizontal force, and V the vertical force magnetometer; the letter n designates the north end of each magnet; the declination magnet being suspended

Fig. 1.
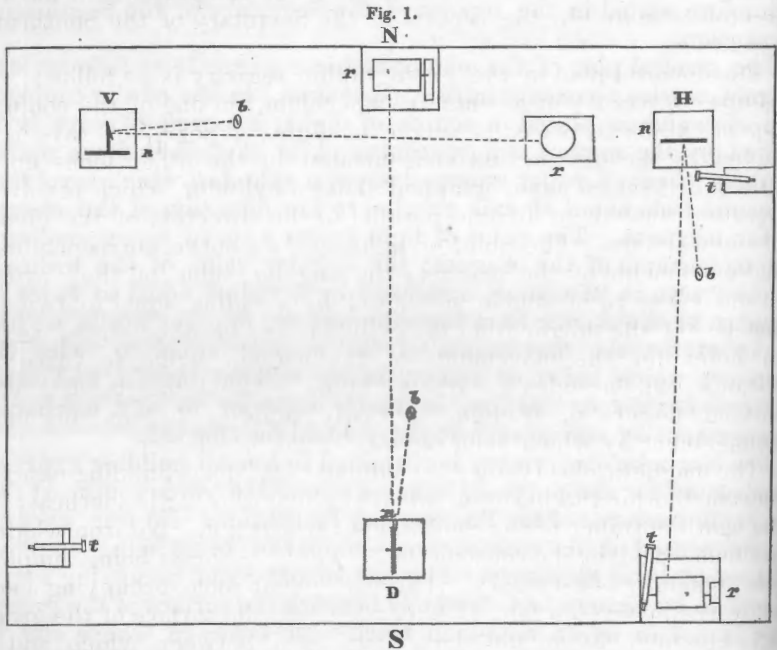
Plan of instrument room.

freely in the magnetic meridian, the horizontal force magnet in a bifilar torsion-balance, with its north end to the west, and the balanced vertical force magnet attached to a beam resting on knife edges, with its north end to the east. The line joining the centre of D and H makes an angle of 35° 16' with the magnetic meridian according to Lloyd's theorem, for the position in which small changes of direction in one magnet will have no effect upon the other. In b, b, b, on the gas-burners, from which the recording pencils of light emanate, which are reflected by concave mirrors attached to the magnets, and concentrated upon the record cylinders r, r, r. Each magnet carries, besides, a small plane mirror, by means of which

scale-readings are obtained on fixed scales attached to the reading telescopes t, t, t, the permanence of the direction of which is checked by reference marks on the opposite wall of the room.

The three instruments are supported on brick piers 16 × 16 inches, having no connexion with the floor, while the reading telescopes and the record cylinders for D and H rest on wooden brackets bolted to the wall; the record cylinder V is supported on a wooden stand which rests on the floor, as do also the tripods which support the gas-burners. The centres of the mirrors and of the cylinders are all in one horizontal plane three feet above the floor. The distance from each mirror to its record cylinder is 9 feet 6 inches, affording a scale value of one-fifteenth of an inch to one minute of angular motion of the magnet. The distance from burner to mirror is about 28 inches.

DECLINATION INSTRUMENT.

Fig. 2.

Fig. 2 is a side view, on a scale of one-tenth the actual size, of the instrument for recording the changes in the declination. A marble slab sustains a frame made of brass tubes, supporting a top piece, from which the magnet is suspended by a bundle or skein of parallel silk fibres. The suspension skein is enclosed in a glass tube, and the magnet, with its attachments, is also enclosed in a case with sides of plane plate glass. The top pane is likewise covered over by a round glass cap. The magnet is 9 inches long, $1\frac{1}{4}$ inch wide, $\frac{1}{4}$ inch thick, and is held by a clamping frame, the upper part of which carries the hook by which it is suspended, while to the lower part is attached the frame which holds the speculum and the plane mirror for scale readings. In Fig. 2 the speculum is seen edgewise; compare, also, Fig. 4 below, which gives a front view. The mirror frame is pivoted to the carrier, and is held in position by a tangent screw, by means of which its direction can be nicely adjusted to any part of the record cylinder.

The magnet is closely surrounded by a copper "damper," which, acting by induction when the magnet is in motion, checks the vibrations occasioned by the changes of direction, and keeps the oscillations within nar-

Plan of Damper.
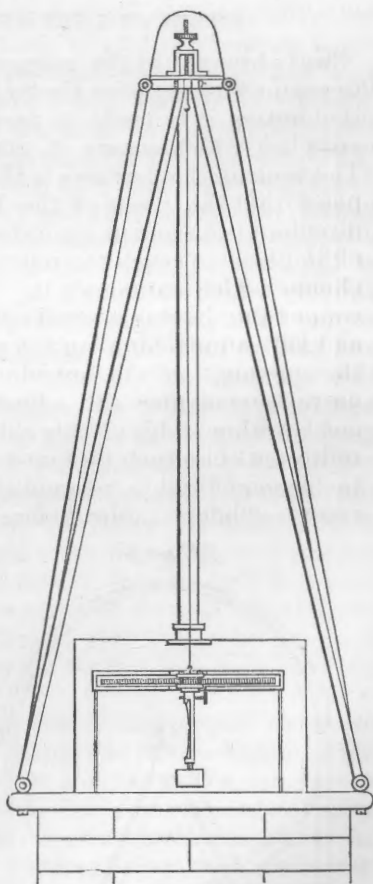
Declination instrument.

row limits. A screw at the top of the frame serves to adjust the magnet to the proper height. The circular top plate is graduated on its edge, and can be revolved about a central collar, by which means the suspension skein can be turned through any desired angle. In order to take out all twist, when the instrument is first mounted a bar of lead of the same weight as the magnet is suspended in its place, and after it has found its position of rest, the upper plate or "torsion circle" is turned until the bar comes to rest in the magnetic meridian. After suspending the magnet the effect of torsion of the suspension skein on the scale value is determined by turning the torsion circle through 90° on either side of its proper position, and observing the change of direction produced on the magnet. The ratio of this deflection to 90° is the proportion in which all observed variations require to be increased, in order to correct for the resistance of the suspension skein to torsion.

SOURCE OF LIGHT AND METHOD OF REGISTRATION.

Gas is brought to the magnetic observatory in a leaden pipe from the main which supplies the Smithsonian Institution. Before reaching the burners it is made to pass over naptha exposed in a suitable vessel, by which means it gives a more white and brilliant light. The shape of the burners is that known as *fishtail*, and they are so placed that the plane of the burner makes a small angle with the direction to the mirror, in order to allow light from the whole width of the flame to reach the mirror through the narrow aperture of the chimney which surmounts it. This chimney is an oblong blackened copper tube, having a small opening about one-tenth of an inch wide and half an inch high on the side towards the mirror, and close to this opening, on the outside of the chimney, is a screen, with a narrow rectangular slit, adjustable in width by a sliding plate. A good working width of this slit is found to be a little over one hundredth part of an inch by four-tenths of an inch in height, which forms the beam of light, a magnified image of which is thrown upon the record cylinder. Just before reaching the cylinder this beam is received upon a pair of cylindrical lenses, seen in section in fig. 3, by which it is refracted to a focus of about one-twentieth of an inch diameter, which makes the photographic trace. By this ingenious arrangement a sufficient quantity of light is concentrated to produce a very good trace. The lenses extend the whole length of the cylinder, and are
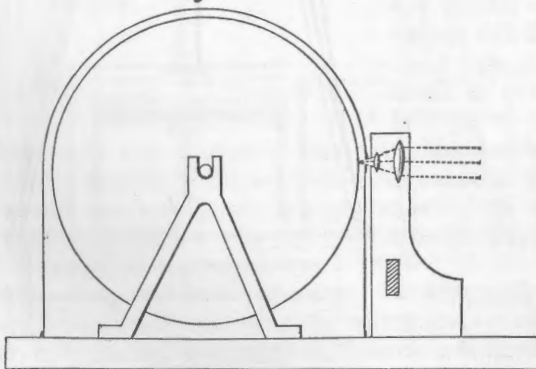
Fig 3

mounted in a frame, which can be removed and replaced in position every time the paper on the cylinder is changed.

The cylinder is 12 inches long, and as the space described on its surface, by a movement of the magnet of minute in arc, is equal to one-fifteenth of an inch—the distance to the mirror being 9½ feet—we have room for recording variations to the extent of 180' or three degrees, which will be approached only in extraordinary disturbances. The diameter of the cylinder is 8½ inches, and it revolves by the action of a time-piece once in twenty-four hours, giving a time-scale of a little over one inch for each hour. The cylinder is made of light staves of white pine, truly turned on its axis, and coated with shellac varnish, which protects the sensitized paper from the reaction of organic acids in the wood. In order to secure still further this protection, the cylinder is covered by a sheet of stout drawing paper, which is replaced by a new one from time to time. Mr. Brooke uses glass cylinders, selected from French glass shades for covering vases, &c.; and this material certainly has the advantage of not in any way reacting on the sensitive preparations, and of being most readily kept clean. But it is so difficult to obtain them of true and uniform figure that the means before mentioned have been resorted to, and with good success. Of a dozen glass cylinders sent with the instrument, but two arrived unbroken, and these are used for the vertical force register, while the wooden cylinders are used for the declination and horizontal force. Each cylinder is driven by a clock-train, regulated by a seconds pendulum with wooden rod, and revolves once in twenty-four hours. It is covered with a blackened copper case, having a narrow slit through which the pencil of light strikes the paper, as seen in figure 3.

PHOTOGRAPHIC PROCESS.

The record paper is prepared by immersing it first in a solution of *iodide of potassium*—half an ounce of *iod. potass.* to ten ounces of water. After drying it thoroughly in the air, one side of the sheet is carefully floated on a solution of *nitrate of silver*—320 grains of *fused* gray lunar caustic, (or nitrate of silver,) perfectly free from acid, in 10 ounces of water—for 1¼ minute, and after being allowed to drain a little while by being held up by one corner, it is washed in several changes of water, for which cistern water will do well enough, and hung up to dry in the dark. A number of sheets may be prepared at one time in this manner, and preserved for use in a dark place for ten or twelve days.

When required for use one of these sheets is wrapped about the cylinder, which for that purpose is taken out of its bearings and placed upon similar supports attached to a table in the laboratory. It has been customary to fasten the ends by a paste made of gum-arabic dissolved in acetic acid; but two elastic bands clasped around the sheet, one at each end, are found quite sufficient, and much more convenient. The cylinder being replaced in its bearings, and connected with the clock movement, a mark is drawn across the edge or joint of the sheet with a hard pencil, which, when the sheet is unrolled, furnishes points for drawing a line of abscissæ, on which the

time is measured, and from which the ordinates of the curve are subsequently read off. The cover is next put on, and the lens-frame put in position by stops on the base, to which the supports of cylinder and the clock are attached. Observing the place where the point of light now strikes the paper, a pencil mark is made, then the time noted, and a corresponding reading taken in the telescope on the fixed scale, which for that purpose is illuminated by a candle held in the hand.

After the lapse of 24 hours, less about 5 minutes, a similar reading is taken, the time noted, and a corresponding pencil mark made on the trace. The cylinder is then lifted out, the sheet taken off, and a new one immediately put on and started as before.

The former sheet is then floated on a saturated solution of *gallic acid*, to which a small quantity of the nitrate of silver solution has been added, and allowed to remain from 10 to 20 minutes, until the trace is distinctly brought out. After being next rinsed several times with pure water, it is soaked for 15 minutes in a solution of *hyposulphite of soda*—one ounce to ten ounces of water—to fix the trace; and, lastly, the excess of hyposulphite is thoroughly washed off, and the sheet allowed to remain in pure water until next day's operations have brought the following sheet to the same stage. The former sheet is then allowed to drain, and is thoroughly dried, exposed to the air on a flat piece of glass, to which it adheres, and is thus kept from wrinkling or warping.

TABULATING THE RECORD.

The trace is usually a well defined black line of about the twentieth part of an inch in breadth, with somewhat pale margins or edges. The middle of the line can readily be estimated to the nearest hundredth of an inch by a practised reader. Having the time and scale readings corresponding to the beginning and end of the trace, and the direction of the time-scale, it is easy to lay off points at each end of the sheet corresponding to a certain average scale-reading—600— through which points a line of abscissæ is drawn with pencil. In order to read off the ordinates or scale-reading for every half hour, or oftener, when the character of the trace makes it desirable, a scale of half hours is applied to line of abscissæ, and the corresponding ordinates read off on the edge of a square divided into spaces corresponding to minutes of arc for the declinometer, and to one ten-thousandth part of the force for the other two instruments.

The process of preparing the paper and subsequent operations are, of course, precisely the same for the three instruments. All three sheets are taken off and replaced in rapid succession before the bringing out of the traces is proceeded with.

BIFILAR MAGNETOMETER OR HORIZONTAL FORCE INSTRUMENT.

This instrument is designed to exhibit and record the *variations* in the *horizontal component* of the earth's magnetic force. To that end the magnet is constrained by a twisted suspension by two threads to

assume a position nearly at right angles to the magnetic meridian, when the horizontal force acting upon it at right angles small changes of force will produce proportional changes of direction, and the former may be measured by the latter. Figure 4 shows the instrument in outline. The framework is the same as in the declinometer above described. The magnet and attachments are suspended by a silk suspension skein attached to two hooks, and passing at the top over a glass roller, which turns on pivots in bearings supported by the torsion-circle. The compound bar above the magnet, to which the hooks are attached, effects the compensation for changes of temperature, as will be specially described below. The force of the torsion-balance depends upon the *upper* and *lower* distance between the threads, their *length l*, the *weight w*, of the suspended mass, and the angle *θ*, by which the plane of the threads is twisted. If we designate, as is customary, the earth's horizontal force by *X*, the magnetic moment of magnet by *m*, and half the upper and lower distance between the centres of the threads by *a* and *b*, respectively, we have the following equilibrium of forces:

$$m\,X = \frac{a\,b}{l}\,W,\ \sin\theta.$$

From this equation is readily derived the simple and accurate method of ascertaining the scale value of the instrument proposed by Mr. Brooke. Observing that any variations in the quantity *X* may be balanced by proportional variations in *W*, he observes the change of scale-reading produced

Fig. 4.

Horizontal force instrument or bifilar magnetometer.

by adding to the suspended mass a small weight equal to its one hundredth part, and the same change on the scale, of course, corresponds to a variation of one-hundredth part of the force. This space on the scale is divided into one hundred parts, in order to have as a convenient unit the ten-thousandth part of the whole horizontal force.

Since the magnetic force *m* of the magnet is diminished by increase of temperature, and vice versa, the indications of the instrument would be affected by this cause if some arrangement were not introduced to counteract and precisely balance its effect.

Mr. Brooke's compensating apparatus consists of a glass rod clamped

Fig. 5.　　　Fig. 6.

at its middle point to the centre of magnet, the axes of the rod and bar being parallel; the free ends of the rod are enclosed in two zinc tubes, at the inner ends of which, where they nearly meet in the centre, and to their upper surface, two hooks are attached; two loops at the ends of the suspension skein are attached to these hooks, the skein passing over a pulley at the point of suspension. Towards either end the rod and tubes are connected by a movable clamp, by which the two may be clamped together at any required distance from the centre It is evident that by elevation of temperature the free ends of the zinc tubes will be approximated to each other by a quantity equal to the difference of the expansion of the lengths of zinc and glass that intervene between the sliding clamps and the free ends of the tubes, and, consequently, that a diminution to the same extent of the distance betwe the lower ends of the suspensi skein will take place. The varia tion of the interval between the lower ends of the skein corres ponding with any given variation of temperature may be made to bear any required ratio to the whole interval, first, by a due adjustment of the upper and lower intervals of the skein, and secondly, by varying the position of the sliding clamps; that is, of the acting lengths of the expanding tubes; the former may be considered as a coarse, the latter as a fine adjustment. The glass rod rests on rollers attached to the under surface of the tubes opposite to the hooks, in order that no jerking may be occasioned by the expansion or contraction of the zinc tubes. By these means the quantity b in the preceding formula may be made to vary by change of temperature proportionably to the quantity m with any required degree of exactness; so far, at least, as the variation of m is directly proportional to the variation of temperature.

Figs. 5 and 6 show plan and elevation of the bifilar compensator two-thirds the actual size. *a a*, the magnet ; *b*, the clamp, which attaches the glass rod to the magnet; *c c*, the zinc tubes enclosing the glass rod; *d d*, the adjusting clamps, consisting of two parts; the outer encircles the zinc tube, the inner passes and nearly fills the interval between the tube and glass rod. They are capable of sliding for adjustment when the screws are loosened ; when tightened, the rod and tubes are held together; *e e*, screws for adjusting the distance between the hooks *h h;* these should be withdrawn when the clamps *d* are fixed ; *o o*, fig. 5, are the ends of the clamping pieces interposed between the tubes and the rod.

The proper dimensions of the compensator and its approximate adjustment are found by first ascertaining the temperature co-efficient of the magnet, experimentally, from its time of vibration at high and low temperatures, and calculating the corresponding proportions. The more perfect adjustment is made after the instrument is completely mounted, by enclosing it in a box with a water-jacket, in which the temperature of the water can be raised to any required temperature by heating a pipe connecting the inlet and outlet of the jackets, and comparing the variations of the instrument at different temperatures with the indications of another bifilar instrument, the temperature of which has been maintained comparatively constant.

VERTICAL FORCE INSTRUMENT OR BALANCED MAGNETOMETER.
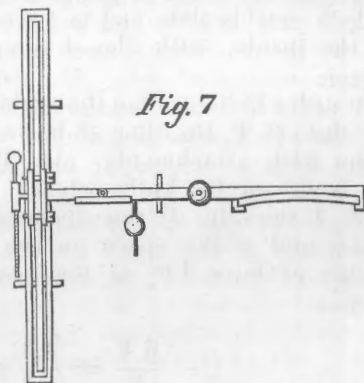
Fig. 7
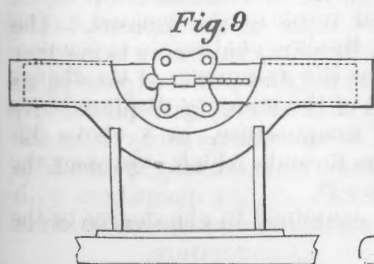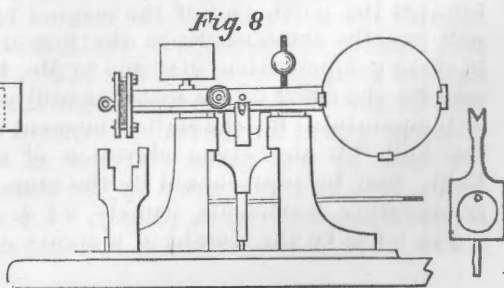
Fig. 9 *Fig. 8*

This instrument is designed to measure and record the variations in the vertical component of the earth's magnetic force, to which end a magnet is balanced horizontally in a plane at right angles to the magnetic meridian, in which position its balance is affected by the vertical part of the force only, and variations in the latter will produce corresponding proportional changes of direction in the position of the magnet. In the annexed diagram, fig. 7, is a top view, fig. 8, a side elevation, and fig. 9, an end view of the instrument, such parts being omitted in each as would confuse the representation. In fig. 7 we see the balance beam, with speculum attached on the right, and magnet on the left hand, the latter surrounded by the copper damper, which is further shown in fig. 9, but omitted in fig. 8. The beam rests with agate knife edges on agate planes, and can be lifted off or let down upon the latter by means of a vertical sliding frame, (in fig. 8, and shown endways on the right hand,) which can be moved by an eccentric, operated by means of a key from the outside of the surrounding case. Whenever the instrument is handled for the purpose of adjustment or otherwise, it is lifted off the agates, and afterwards slowly lowered. There are attached to the beam two balls on screw stems, one for adjusting the balance, the other for adjusting the height of the centre of gravity, and thereby the value of the scale divisions. The small thermometer attached to the balance frames parallel with the magnet is for the purpose of compensating the effect of temperature. The whole instrument rests on a black marble slab, and is covered by a mahogany case, blackened on the inside, with glazed apertures for the speculum and plane mirror.

In order to determine the scale value, according to Lloyd's method, we observe T_h the time of horizontal vibration of the magnet in its frame with attachments; also T_v the time of vertical vibration of the same on its knife edges. Then if F designates the vertical force, I the dip, D the distance from the mirror to the scale or paper, and x the space on the paper corresponding to a certain change of force d F, as, for instance, the one-thousandth part, we have—

$$\frac{d\,F}{F} = \frac{T_h{}^2}{T_v{}^2} \times \cot.\ I \times \frac{x}{2\,D}$$

The compensation for temperature is effected by means of a thermometer, whose bulb and stem are so adjusted to the temperature and scale coefficients that the translation of a portion of the mercury towards the north end of the magnet by an increase of temperature will exactly counterbalance the loss of force in the magnet. This mode of compensation, also due to Mr. Brooke, enables us to compensate for the effect of the *second* as well as the *first* power of the change of temperature; for the statical moment of the mercury displaced from the bulb by any given elevation of temperature, as t, above 32° Fahr., may be represented by the same formula which expresses the temperature coefficients, namely, $c\,t + e\,t^2$.

For let w be the weight of mercury contained in one degree of the

tube, and let the tube be taken such that the distance from the centre of the bulb to the point $32°$ may be $k\,c_r$ and length of one degree $2\,k\,c$, then at any temperature $32° + t°$, the statical moment of the mercury displaced by a small change of temperature, $d\,t$, will be $w\,(k\,c + 2\,k\,e\,t)\,d\,t$, and consequently the statical moment of the mercury displaced between the temperatures of $32°$ and $32° + t°$ will be $(k\,c\,t + k\,e\,t^2)\,w$. Let, now, v be the weight which, placed at an unit of distance from the axis of rotation, will represent the temperature for $1°$ above $32°$, it will only remain to obtain the bulb of such a size that $k\,w = v$.

The value of v is to be determined, experimentally, by observing the displacement of the register line, or change of scale reading, occasioned by a small weight placed on the magnet at a known distance from the axis of rotation, and comparing it with the scale value of the temperature correction obtained in the usual manner, by horizontal vibrations at different temperatures.

The slit in the screw before the burner for this instrument is, of course, horizontal, the axis of the record cylinder being vertical. The cylinder rests on a horizontal plate, which is supported on friction wheels, and driven by an arm below the stand connected with the train of a time-piece.

In several papers in the *Philosophical Transactions of the Royal Society*, from 1847 to 1852, Mr. CHARLES BROOKE, F. R. S., has described the apparatus which is the subject of the foregoing paper, in the preparation of which the former have, of course, been largely drawn from. Those wishing to pursue the subject into its practical application should, by all means, read Mr. Brooke's memoirs, and are also referred to an interesting description of the self-registering instruments of the observatory at Toronto, by Captain J. H. LEFROY, R. A., in *Silliman's Journal*, May, 1850.

NOTE.—Since the above article was prepared the instruments have been removed from Washington City to Key West, Florida, where they were put into operation in January, 1860, and where observations will be kept up for some years in connexion with the magnetic observations of the United States Coast Survey, and in direct correspondence with a system of similar observations set on foot by the British Government at various points in their possessions. Toronto, Canada West, being one of those stations, it was thought advisable to remove the Smithsonian station to a point as far south as practicable, as the long series of corresponding observations obtained at Toronto and Girard College, Philadelphia, in the years 1841 to 1845, left nothing further to be desired in the comparison of stations not any distance from each other. The observatory at Key West, and the objects of the system of magnetic observations at present in progress, will be noted in future reports.

ON THE USE OF THE GALVANOMETER AS A MEASURING INSTRUMENT.

BY J. C. POGGENDORFF.

The galvanometers in use correspond very imperfectly with their name, affording, as they do, a very uncertain and limited measure of the force of the current.   Even within the first ten or twenty degree within which the deflection of the needle is usually assumed as pro-portional to the force of the current, the accurate determination of the relation between these elements is not so easy and simple, and beyond these limits the problem becomes so complicated that even its theoretical solution is extremely difficult.

The determination is not absolutely impossible; it could be made by Ampere's formula if all the requisite data, (length and form of the coils, their position and distance from the needle, the size, shape, and magnetic condition of the latter,) were given, but the calculation would be extremely complicated and tedious.   It would, even then, hardly repay the trouble, for the result would still be unreliable, on account of the probable errors in the determination of the data; and, even if it were quite correct, it would only have a specific value, for the calculation must be repeated for each instrument, and even for each position of the needle in the same instrument.   For this reason no attempt has hitherto been made to form a theoretical scale of in-tensity for the galvanometer, but various empirical methods have been adopted, which, though they yield only special results, are preferable, because less tedious, and therefore more easily repeated, and capable of greater accuracy.

Several such methods have been contrived by Becquerel, Nobili, and Melloni.   They all require the use of a series of currents com-bined in various ways.   With reference to this they may be divided into two classes—methods by combination and by difference.

The simplest of the former is given by Becquerel.   He attached to the galvanometer a thermo-pile, of which he set in action, succes-sively, one, two, three, four, &c., pairs of plates, by heating each alternate joint as uniformly as possible.   According to the theory the force of the current must also increase in a ratio, one, two, three, four, &c., and, consequently, the deflection of the needle correspond-ing to these intensities may be read off directly.   This need only be marked on the instrument in order to establish the relation for all future cases.*

This method proceeds on the principle of passing, successively, one, two, three, four, &c., currents of the same strength from the

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* Traité d'Electricité, T. II, p. 24.

same source through the same wire ; but currents produced by one or more sources may also be made to act upon the galvanometer by several wires. This process requires a galvanometer with two or more wires. In other respects, the manipulation is the same as with the former process, but since the number of the wires, and, consequently, that of the currents which can be employed at once is limited, it requires several series of the latter to embrace the whole quadrant. It should be so arranged that the currents of one series shall produce the same effect as a certain number of currents of the next previous series, so that the several partial series may be combined into a general one. Both Becquerel and Nobili* employed this process.

Melloni based his admirable experiments on the method by differences. After satisfying himself that the deflection of his galvanometer was proportional to the force of the current within the first twenty degrees, he determined the relation between these elements beyond this limit in the following way :† He attached to the galvanometer a thermo-pile, and warmed one of its sides by bringing a spirit lamp near it until a deflection of 20° was produced. He then placed a screen before the lamp, and waited until the needle returned to 0°. He next allowed the radiant heat from a second lamp to fall upon the other side of the thermo-pile, and regulated the distance of the latter so that a deflection of 24° in the contrary direction was produced. He finally allowed both lamps to act simultaneously on the pile. He now obtained by the difference of the currents a deflection, not of 4°, but of 5.1° ; he therefore concluded that the current which, from producing a deflection of 20°, he made equal to 20, must have increased 5.1 units to produce a deflection of 24°, and must therefore be 25.1. Increasing the activity of the pile by advancing first one lamp and then the other toward the pile, he determined in the same way the increase of the force of current for the intervals 28°—24°, 32°—28°, &c. After thus fixing a sufficient number of points in his scale of intensity, he filled up the intervals by hand.

Nobili had already used the same process with a galvanometer with two wires, through which he passed currents from two batteries in opposite directions—first separately, and then together.

All of these methods are liable to considerable objections. Although not so tedious as a theoretical determination of the scale of intensity would be, they are still troublesome. They are, moreover, based on conditions which are difficult to fulfil, and for whose fulfilment we have no control. It is theoretically true that the strength of the current in a thermo-pile is proportional to the number of the pairs of plates set in action by equal differences ; but we have no certainty that the difference of temperature is the same in all the pairs, or that the same difference would produce the same effect in all.

Melloni's method is probably the best of those named. But if the observation of Becquerel,‡ that equal variations of temperature produce thermo-currents of different intensity, according to their position

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* Pogg. Annal. Bd. ix, S. 346 and xx S. 226.
† Idem, Bd. xxiv, S. 132.
‡ Idem, Bd. ix, S. 350.

in the thermometer-scale, this method can evidently be applica■
only to such slight differences of temperature and such feeble c■
rents as the Italian physicist used in his experiments.

Besides this, the assumption that the current which produc■ a
deflection of 24° must be 5.1 stronger than one which produces a
deflection of 20°, because, when acting in opposition to the latter, it
caused a deflection of 5.1°, is only an approximation to the truth,
based on the tacit assumption of the proportionality between the
force of the current and the angle of deflection. If a, b, and $b-a$,
be the forces which produce, respectively, the deflections a, β, γ,
strictly taken, the ratio $a : b = a : a + \gamma$ can only exist when $a : b =$
$a : \beta$. This method, therefore, requires that the difference betwe■
β and a and the value of γ should fall within the limits within which
this ratio is approximately true.

From what has been said, it will be clearly seen that no perfect
method of determining the scale of intensity for the galvanometer has
hitherto been given. It may be said, indeed, that none is needed,
since the best galvanometer makes only a tolerable measuring instru■
ment, and for accurate investigations we have the mirror-appar■
and the compass of sines. But these instruments are so costly a■
to be within the reach of every physicist. There are, moreove■
many experiments in which an accuracy of from one-half to a whole
degree in the deflection of the needle is quite sufficient.

I therefore believe that the description of a mode of arranging the
galvanometer scale, which seems to me to satisfy all demands, will
be welcome to many experimenters. My method is convenient; cer-
tain, and susceptible of general application. It has also a decid■
advantage over all hitherto described, in requiring but a single cur-
rent of uniform strength.* The principle of this method may be
expressed in a few words. The deflections, produced by current■ of
different strength, passing through the coils of a multiplier lying in
the magnetic meridian, can be deduced from those produced by one
and the same current, passing through the same coils, at variou■
inclinations to the magnetic meridian.

The possibility of this will be seen from the following geometric
considerations. The force with which the magnetism of the earth
tends to draw back a needle which has been deflected from the

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* Prof. Petrina, in Linz, has recently described a method (v. Holger's Zeitschrift für Physik
Bd. I, s. 171) which has also this advantage, and is easily managed, but cannot be general■
recommended, because it gives only approximate results. Prof. P. lays the ends of the wir■
of his galvanometer on the conducting wire of a constant battery, and then assumes that the
force of the branch current passing through this instrument will be exactly proportion■ to
the distance between the two points at which the conducting wire and the wires from the
galvanometer touch each other. But this is not true. If we designate by r the resistanc■ of
the portion of the conducting wire lying between the two points of contact, by r' the remain■
ing resistance of the battery, and by r'' the resistance of the galvanometer wire, we shall
have for the force of the current passing through the latter, k being the electromotive force
of the battery, the expression—

$$\frac{kr}{r\,r' + r\,r'' + r'\,r''} \quad \text{or} \quad \frac{kr}{r'\,r'' + (r' + r'')\,r}$$

from which it is evident that the force of the current is approximately proportional to the
resistance r only so long as it is very small in comparison with r' and r''.

meridian, it is well known, is the product of three factors—the intensity of the terrestrial magnetism, the magnetic force of the needle, and the sine of the angle of deflection. This force may, therefore, be represented by a curve ($M N$ page 407) whose abscissas are the arcs, and whose ordinates are the sines of the arcs, or the products proportional to these sines.

An analogous but inverted curve, *i. e.*, increasing as the cosines of of the arcs, would represent the force with which the electrical current tends to deflect the needle, in traversing a straight or circular wire lying in the magnetic meridian, at an infinite distance from the needle or the diameter of its circle, when compared with the dimensions of the needle. At the same time, the intersection of these two curves, or rather the abscissa corresponding to it, would represent the deflection of the needle under the influence of both these forces.

With the galvanometer the latter curve is much more complex, owing to the complicated form of the wire and its proximity to the needle. Its course is not known, but it is evident that it must have nearly the shape of $a R$ in the figure. The problem is first to determine the shape of this curve, and as this is theoretically impracticable, it must be done experimentally.

This is done in the following way: Suppose $a R$ to be the unknown curve. Its form could evidently be determined by moving it to the right and left along the axis of abscissas LR, and marking in each position the co-ordinates of its point of intersection with the curve $M N$, which may be considered as given. Thus for the positions $a R$ and $a' r'$, we should have the points of intersection c and c', the ordinates $c\ p$ and $c'\ p'$, and the abscissas $M p$ and $M p'$, and in order to obtain the form of the curve, the distance $w M$ would have to be added to, or subtracted from, the abscissas of the points of intersection in every position but the original one.

This simple geometrical supposition may be readily and exactly carried out, if the coils of the galvanometer wire are movable in a horizontal plane. These instruments generally have such an arrangement, and it is then only necessary to fix an index near the coils, by which the amount of rotation may be read off.*

Set the galvanometer so that the index and the zero line of the graduation shall be in the magnetic meridian, and the axis of rotation of the needle exactly in the centre of the graduated circle. Then pass a constant current through the coil so as to obtain a steady deflection of 35° to 40°. A thermo-electric current is best suited to the purpose.

This first angle of deflection represents the abscissa $m\ p$, and its

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* On my galvanometer the plate which carries the coil turns by means of a cogged wheel and an endless screw, on a metallic axis, working on metallic bearings. Such an arrangement is necessary in order to move it steadily. Strictly speaking, the support on which the needle hangs should be fixed to the plate, so as to turn with the coil, and thus eliminate the torsion of the thread; and secondly, the ends of the wire, which cannot turn, must be twisted together so as to be without influence on the needle, and pass out through a hole under the centre of the coil, as in the compass of sines.

sine the corresponding ordinate $p\,c$. Now turn the coil to the left at an angle $w\,M$. A deflection $M\,p'$ is produced; therefore we have for the point c' of the curve, the abscissa $w\,M + M\,p' = w\,p'$, and the ordinate $p'\,c' = sin\,M\,p'$. Proceed in this way until $w\,M = 90°$, the deflection will then be $0°$; and hence the abscissa $= 90°$, and the ordinate $= 0°$. Turn the coil in the same manner to the right of the meridian, and mark the corresponding angles of deflection until the angle between the magnetic needle and the coil of wire is reduced to $0°$. This completes the observations. The sine of the angle of deflection which corresponds to the last position of the coil, represents the first ordinate Ma of the curve.

In general terms the process is as follows: Place the coil at an angle m, with the magnetic meridian; the magnetic needle will then make with the coil an angle, n, and with the magnetic meridian an angle, $a = n + m$, or $n - m$, according as m lies upon the same side of the magnetic meridian as n, or the contrary. If the several values of m and n be now distinguished by accents above or below, according as they belong to the former or latter position of m, we shall have the following results:

Angle between the coil and the merid'n.	Abscissas.	Ordinates.
$+m'''\,\cdots$	0	$sin\,a''' = sin\,m'''\,\cdots$
$+m''$	n''	$sin\,a'' = sin\,(n'' + m'')$
$+m'$	n'	$sin\,a' = sin\,(n' + m')$
0	n	$sin\,a = sin\,n.$
$-m_{\prime}$	$n_{\prime}$	$sin\,a_{\prime} = sin\,(n_{\prime} - m_{\prime})$
$-m_{\prime\prime}$	$n_{\prime\prime}$	$sin\,a_{\prime\prime} = sin\,(n_{\prime\prime} - m_{\prime\prime})$
$-90°$	90	0

This determines the form of the curve which represents the effect of the coil of wire on the magnetic needle for a current of a certain strength, by means of the co-ordinates of the magnetic curve $M\,N$ whose greatest ordinate, $N\,R$, is arbitrarily assumed.

The form of the curve must next be determined for currents of any other strength. If it were necessary to repeat the process just described for every possible strength of current, the process would of course be entirely useless. But such a repetition is not necessary the form of the curve determined for one intensity, enables us to determine its form for every possible intensity.

It is evident that when the force of the current varies without any change in the relative distance and inclination of the coil and the needle, the influence of the coil upon the needle must vary in direct

ratio to the force of the current. Knowing, therefore, the form of the curve for a current of one intensity, we can obtain its form for any other intensity by lengthening or shortening all the ordinates in proportion to the force of the second current. Thus, if $a\,R$ (page 407) be the curve for the intensity one, $A\,R$ will be the curve for the intensity one and a half, all the ordinates of the latter being one-half longer than those of the former.

If we suppose the curves for all intensities of the current which can be measured by the galvanometer to be delineated in this way, the main problem is still to be solved: to deduce the values of these intensities, or their ratio to the force assumed as unity, from the points of intersection of the curves with magnetic curve $M\,N$, the co-ordinates of these points being given only by observation.

The mode of doing this may be exemplified by the two curves $a\,R$ and $A\,R$ in the figure. According to what has been said, the forces of the currents which they represent will be to each other as $P\,h : P\,C$, or, what is the same, as $p\,c : p\,k$. It is now required to deduce this ratio from $M\,p$, $p\,c$, and $M\,P$, $P\,C$, the co-ordinates of the points of intersection c and C.

Two cases must be distinguished: either the greater or the less intensity of current may be given, $i.\,e.$, the upper or the lower curve may be known. Let us consider the first case.

In this case we have to determine $P\,h$ in the ratio $P\,h : P\,C$. If we imagine the curve $a\,R$, which is known, to be moved toward the left parallel to the axis of abscissas, its intersection with the magnetic curve will evidently fall lower down, and there will be a position $a'\,r'$ in which its ordinate $p'\,c'$ will be equal to $P\,h$. But $c'\,p'$ is the sine of $M\,p'$, $i.\,e.$, the sine of a value of a for which the corresponding values of m and n have been determined by the process already described. Moreover, $P\,C$ is the sine of $M\,P;$ but $M\,P$ is equal to $w\,p'$, which is the value of n, corresponding to the given value of a.

Therefore, the currents whose effects are represented by the curves $a\,R$ and $A\,R$, and which produce the deflections $M\,p$ and $M\,P$, when the coils lie in the meridian, are to each other as one of the values of $sin\,a$ to one of the values of $sin\,n$ of the table before given; $i.\,e.$, if n_i and m_i be the special value of n and m, corresponding to the position $a'\,r'$ of the lower curve, the ratio will be $sin\,n' - m' : sin\,n'$.

In the same manner the value of $p\,k$ may be determined, if the upper curve $A\,R$ be that for which the table before given was made. We have only to imagine $A\,R$ to be moved to the right, to a position $A'\,R'$, in which $C'\,P' = p\,k$. Then $p\,k = C'\,P' = sin\,M\,P' = sin\,(n + m)$ and $p\,c = sin\,M\,p = sin\,W\,P' = sin\,n$. Now if m' and n' be the special values of m and n, corresponding to the position $A'\,R'$ of the upper curve, we shall have for the ratio of the currents $p\,c : p\,k = sin\,n' : sin\,(n' + m')$.

From this it is evident that, according as the force of the current to be measured is greater or less than that assumed as unity, the curve representing the latter must be moved to the right or left,

until the abscissa of its intersection with the magnetic curve is equal to the abscissa of the intersection of the curve of the current to be measured, in its normal position, with the same curve. The sine of the latter abscissa, divided by the sine of the former, expresses the ratio of the current to be measured to that assumed as unity.

Apart from geometric construction, this rule may be thus expressed. To measure the force of a current which is greater or less than the assumed unit, observe first the deflection produced by it when the coils lie in the magnetic meridian. Then turn the coils, in the former case backward, in the latter forward, until the angle between the needle and the coils is equal to this angle of deflection. The sine of this angle of deflection, divided by the sine of the angle of deflection produced after the rotation of the coil, is the ratio of the current to the assumed unit.

It will be seen from this that if we have a table like the one already given, containing, for a certain current assumed as unity, all the values of n, or the angle made by the needle and the coil of wire, from 0° to 80°, and the corresponding values of m, or the angle of the magnetic meridian with the coils, a second table may be constructed from this which will give the ratio of the deflection of the needle to the current producing it, in terms of the same unit, for the case where the coils lie in the magnetic meridian.

As examples, I give two such tables constructed for my galvanometer. The current for the first was furnished by a small thermopile, made of two pairs of German silver and copper wires twisted together and heated at their alternate connexions in a sand bath over a spirit lamp. During the eighteen measurements made, which required not more than half an hour, the current was as good as constant. These measurements were only undertaken to illustrate the process, and I therefore sought neither after great accuracy nor completeness. The values of n could easily have been determined for every degree.

Observed.			Observed.		
m	n	$n + m$	m	n	$n + m$
°	°	°	°	°	°
+49½	0	49½	− 8	40	32
+46½	5	51½	−19	45	26
+43½	10	53½	−28½	50	21½
+38½	15	53½	−37	55	18
+31½	20	51½	−45½	60	14½
+23½	25	48½	−54	65	11
+13	30	43	−61	70	9
+ 3	35	38	−69	75	6
0	36	36	−76	80	4

The ratio between the deflections and the forces of the current,

when the coils lie in the magnetic meridian, as calculated from this table, is as follows:

Deflection. n	Intensity. $\dfrac{\sin. n}{\sin. (n + m)}$	Deflection. n	Intensity. $\dfrac{\sin. n}{\sin. (n + m)}$
°		°	
0	0.0000	40	1.2130
5	0.1114	45	1.6130
10	0.2160	50	2.0901
15	0.3220	55	2.6508
20	0.4370	60	3.5182
25	0.5643	65	4.7499
30	0.7331	70	6.0071
35	0.9316	75	9.2408
36	1.0000	80	14.1180

These tables, in addition to what has been said, will sufficiently explain the method. I will only remark here that the unit of force, which in this table corresponds to a deflection of 36°, is, within certain limits, entirely arbitrary. Any current may be assumed which admits of n in the first table being made zero, *i. e.*, the coils being brought into parallelism with the needle. By beginning with too strong a current this would of course be rendered impossible. In the example given above a rotation of 49½ was necessary to make them parallel; the limit of this rotation would of course be 90°.

In constructing the first table, on which the second is based, it is therefore necessary to make sure by a previous experiment that the current used is not too strong to admit of the needle and the spiral being placed parallel; otherwise, the scale of intensity of the second table could not be extended down to 0° of deflection. It is also best to begin the values of m and n with this parallellism, that is, with the zero value of n, and to continue the rotation from this point until the value of n reaches 80°, or the point at which it is designed to stop.

A feeble current gives the most accurate results in the lower part of the scale of intensity, and a stronger one in the upper; two currents may therefore be used, if desired, in constructing the first table. The stronger of the two need not give $n = 0$, but its strength must be such that the smallest value of n which it gives shall coincide with the greatest value of n from the other current, so as to obtain a complete series of values of n from 0° to beyond 80°. Moreover, the force of the current, in the scale of intensity obtained in either way, can of course be reduced to the unit of either, and the values not observed supplied by interpolation.

The form of the curve which expresses the effect of the spiral on the needle for the unit of force is determined by the values of n as

abscissas, and those of $sin (n + m)$ as ordinates. In the present case, by taking $sin\ 90° = 100$, we should have—

Abscissas. n	Ordinates. $sin (n + m)$	Abscissas. n	Ordinates. $sin (n + m)$
0°	76. 04	40°	52. 99
5	78. 26	45	43. 84
10	80. 39	50	36. 65
15	80. 39	55	30. 90
20	78. 26	60	24. 62
25	74. 90	65	19. 08
30	68. 20	70	15. 64
35	61. 57	75	10. 45
36	58. 78	80	6. 98

The curve aR in the figure is drawn from these values. They show, what is evident from a glance at the figure, that the culmination of the curve is not over the zero point, but over a point between the abscissas of 10° and 15°, and that it declines from this toward zero. If drawn out completely, i. e., continued on the other side of the meridian to the abscissa of 90°, the curve would therefore have two maxima.

This circumstance, which, so far as I know, has not hitherto been observed, is not caused by an error of observation; I have satisfied myself of its correctness by repeated measurements. Nor can it be merely a peculiarity of my galvanometer, for it evidently arises from the space which is left between the coils for the purpose of introducing the needle into the interior of the spiral. All galvanometers which have such a space must exert an influence on the needle which will be represented by a curve with two maxima, and few are without this space.* It is, however, no disadvantage, provided the depression be not too great;† only, in consequence, the deflections will not be proportional to the force of the current, within the first ten degrees, as is usually assumed, but will bear a rather complicated relation to it. This is, however, a matter of indifference, for by the

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* It has already been observed by several physicists that every copper wire, even that which contains the least possible quantity of iron, is slightly magnetic, and therefore renders it impossible to place a really astatic needle parallel to the coils of the wire, when there is a space between the coils which offers two points of attraction. This observation induced Péclet, (Ann. de Chim. et de Phys. ser. III, t. II, p. 103,) to fill up this space and to hang the needle on a stirrup embracing the spiral. This arrangement would not show the above-mentioned depression, but it has other disadvantages, e. g., it lessens the amplitude of the needle very much, so that there is little probability of its general adoption.

† If the depression were too great, it might happen that at one point the tangent of the curve would form a greater angle with the line of abscissas than the tangent of the magnetic curve at the intersection of the two curves. The consequence of this would be, that this point would correspond to an instable equilibrium between the galvanic and magnetic forces, and there would be another point of intersection on each side of it, corresponding to a stable equilibrium. The existence of these three points of intersection of the curves aR and MN would make the empirical determination of the corresponding values of m and n very complicated. The space which is usually left between the coils of the galvanometers, however, not great enough to cause so deep a depression of aR.

process described, no matter how involved the relation may be, the values of both the elements can be determined as accurately as the readings of the scale will allow, and at as short intervals as may be desired, so that all necessity for uncertain interpolations is removed.

I have thus far, in common with all physicists who have investigated this subject, considered only one-half $a R$ of the resultant curve. But it is easy to imagine that the half which lies on the other side of the magnetic meridian cannot, on account of a possible want of symmetry in the form of the coils, or in the position of the needle, be unconditionally assumed as identical with the first half. If, therefore, it be desired to use the deflections of the needle on the other side of the meridian for comparative measurements of the force of the current, caution requires that the form of the second half of the curve, and the scale of intensity based on it, should be determined in the same way as the first.

It must also not be forgotten that the effect of the spiral on the needle varies with the height of the latter within the former, though in the middle, where the maximum lies, a slight change in the height has no very perceptible influence on the result. For the sake of security it is therefore advisable not to change the height of the needle after having constructed the scale of intensity, and if it should have been accidentally moved, to construct the scale anew. The fixed index may serve to detect any change of height.

The same must be done if there is any reason to fear that the magnetism of the needle has changed perceptibly. The magnetism of a simple needle may, it is true, grow stronger or weaker without any effect on the measurements, for the action of the current on the needle then increases or diminishes in the same ratio. But the distribution of magnetism in the needle must not be changed thereby, i. e., the magnetic intensity of every point of the needle must change in the same ratio, which cannot be assumed with certainty. If the galvanometer be furnished, as is usual, with two needles acting in opposite directions, its indications may be changed without any change in the magnetic distribution by a variation in the relative magnetic intensity of the two needles.

For this reason it is absolutely necessary to test the scale of intensity of the instrument occasionally, especially after the passage of powerful currents through it; and therefore the mode of testing and correcting it must be a simple one. In this respect the method described leaves nothing to wish for. Prolix as the description of it may seem, in practice it is as simple as it is convenient. Half an hour is quite sufficient to make the measurements necessary for the construction of a table like that given on page 402, which, if the instrument has a steady position, and the force of the current does not vary, are as accurate as could be desired.

Besides its convenience, this method has the great advantage of assuming nothing whose correctness is not perfectly evident. The method is certainly an empirical one, depending upon experimental data of a complicated nature; but in the use of these data it is strict

and rational. Its results, therefore, cannot be more erroneous than the measurements on which they are based.*

The principle of this method is closely allied to that on which the use of the compass of sines is based. There is, however, a difference between the two methods. In measuring with the compass of sines the magnetic needle maintains constantly the same angle with the spiral; and as the point of suspension of the needle turns with the spiral the torsion of the thread is eliminated, and at the same time the great advantage is gained of allowing the needle any eccentric position in respect to the centre of the graduated circle without injury to the results.

In the galvanometrical method described in this paper, the needle makes the same angle with the coils only while it is deflected by the currents to be measured. Before and after the action of these currents it makes a different angle with the spiral, or none at all. On this account it is necessary that its axis, the prolongation of the thread, should pass through the centre of the graduation, so that the angles read off by the needle may be really those which it makes with the spiral, or with the magnetic meridian. The small size of the graduated circle on ordinary galvanometers renders it very difficult to hit this coincidence exactly, and this method of measurement is, therefore, inferior in accuracy to that with the compass of sines.

It may be objected that, as recourse must be had to rotation it would be better to use the galvanometer at once as a compass of sines. If the former instrument were made on the same principle and as accurately as the latter, this would certainly be preferable, but even the best galvanometers are only tolerable measuring instruments, and the *small* increase in accuracy which might *perhaps* be gained by this process is no equivalent for the trouble of turning the spiral at each measurement. Besides this, there are many investigations which, without requiring great accuracy, make it very desirable to be able to follow the variations in the force of the current from instant to instant. This cannot be done more conveniently or certainly than by a previously-constructed scale of intensity.

Appendix.—In the foregoing paper the principle of the new method was illustrated, for the sake of clearness, by a geometric construction. It may be more concisely demonstrated in the analytical way, as follows: Let the successive angles between the magnetic meridian and the spiral, with the same intensity I of the current, be represented by $+ m''$, $+ m'$, 0, $- m_{\prime}$, $- m_{\prime\prime\prime}$ $\ldots$ and the corresponding angles between the magnetic needle and the spiral by n'', n', n, $n_{\prime\prime}$ $n_{\prime\prime\prime}$ $\ldots$ The needle will assume, with the different values of the first angle, such a position that I, multiplied by an unknown function f of the last angle, will be equal to a magnitude, M, proportional to the terrestrial

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* This assumes that the torsion of the thread and the influence of the ends of the wire have been eliminated in the way indicated in the note on page 399.

magnetism, multiplied by the sine of the sum of two corresponding angles of each series. We shall therefore have:

$$I f (n'') = M \sin (n'' + m'')$$
$$I f (n') = M \sin (n' + m')$$
$$I f (n) = M \sin (n + m)$$
$$I f (n_{,}) = M \sin (n_{,} + n_{,})$$
$$I f (n_{,,}) = M \sin (n_{,,} + m_{,,})$$

If, on the other hand, the coils lie in the magnetic meridian, and we pass through them successively currents of the intensity I'', I', I, I', I'', which produce the deflections n'', n', n, $n_{,}$, $n_{,,}$, we shall have

$$I''f (n'') = M \sin n''$$
$$I' f (n') = M \sin n'$$
$$I f (n) = M \sin n$$
$$I_{,}f (n_{,}) = M \sin n_{,}$$
$$I_{,,}f(n_{,,}) = M \sin n_{,,}$$

By eliminating the unknown functions, we obtain from these two series of equations the values of the intensities I'', $I' \cdots$, corresponding to the deflections n'', $n' \cdots$, referred to the normal intensity I.
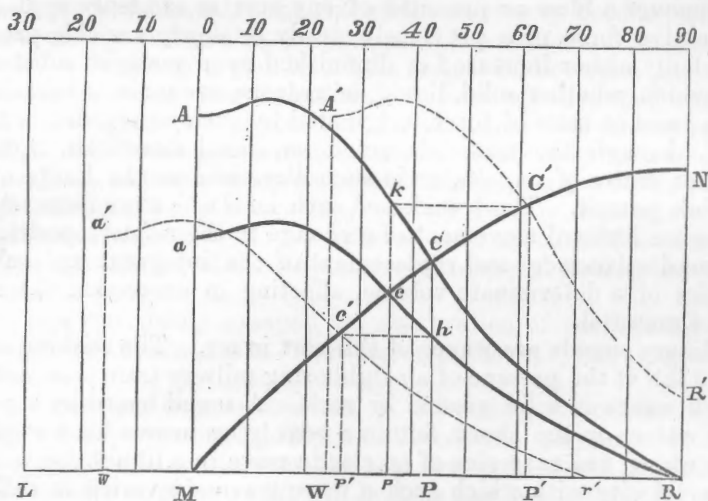
Figure to illustrate Poggendorff's method of measuring with the galvanometer.

ON OBSERVATION OF EARTHQUAKE PHENOMENA.

BY R. MALLET, ESQ.

The observation of the facts of earthquakes and the establishment of their theory constitute *Seismology*, (from σεισμος, an earthquake, a movement like the shaking of a sieve,) which has only become an exact science within the last twelve years. Its immediate and most important applications are to the discovery of the nature of the deep interior of our planet, and of the reactions of the interior upon the exterior, visible in volcanic action at the surface.

Whenever a blow or pressure of any sort is suddenly applied, or the passive force of a previously steady or slowly variable pressure is suddenly either increased or diminished upon material substances, all of which, whether solid, liquid, or gaseous, are more or less elastic, then a *pulse* or *wave* of force, originated by such an *impulse*, is transferred, through the materials acted on, in all directions, from the *origin* or *centre of impulse*, or in such directions as the limits of the materials permit. The transfer of such an *elastic wave* is merely the continuous forward movement of a change in the relative positions, a relative displacement and replacement of the integrant molecules or particles of a determinate volume, affecting in succession the whole mass of material.

Ordinary sounds are waves of this sort in air. The shaking of the ground felt at the passage of a neighboring railway train is an instance of such waves in solid ground or rock. A sound heard by a person under water, or the shock felt in a boat lying near a blast exploded under water, are examples of an elastic wave in a liquid.

The velocity with which such a wave traverses varies in different materials, and depends principally in any given one upon the degree of elasticity and upon the density. This *transit period* is constant for the same homogeneous material, and is irrespective of the amount or kind of original impulse. For example : in air its velocity is about 1,140, in water about 4,700, and in iron probably about 11,100 feet per second, all in round numbers. In crystallized or pseudo-crystalline bodies, such as laminated slate or other rocks, the transit period may vary in three different directions. A very great retardation of this period is produced in solids whose mass is shattered or broken even when the fissures appear perfectly close. Thus, if one stand upon a line of railway near the rail, and a heavy blow be delivered at

a few hundred feet distant upon the iron rail, he will almost instantly hear the wave through the iron rail; directly after he will feel another wave through the ground on which he stands; and, lastly, he will again hear another wave through the air; and if there were a deep side-drain to the railway, a person immersed in the water would hear a wave of sound through it, the rate of transit of which would be different from any of the others, all these starting from the same point at the same moment.

The size of such a wave—that is, the volume of the displaced particles of the material in motion at once—depends upon the elastic limits of the given substance, and upon the amount or power of the originating impulse. By the *elastic limit* in solids is meant the extent to which the particles may be relatively displaced without fracture or other permanent alteration; thus glass, although much more perfectly elastic than India-rubber, has a much smaller elastic limit.

Nearly all such elastic waves as we can usually observe originate in impulses so comparatively small that we are only conscious of them by sounds or vibrations of various sorts, the advancing forms of whose waves are imperceptible to the eye ; but when the originating impulse is very violent, and the mass of material suddenly acted on very great, as in an earthquake, the size of the wave may become so great as to produce a perceptible undulation of the surface of the ground, often visible to the eye, and by whose transit bodies upon the earth are disturbed, (chiefly through their own inertia,) thrown down, &c.

There is every reason to consider it established that an earthquake is simply *"the transit of a wave or waves of elastic compression in any direction, from vertically upwards to horizontally in any azimuth, through the crust and surface of the earth, from any centre of impulse, or from more than one, and which may be attended with sound and tidal waves, dependent upon the impulse and upon circumstances of position as to sea and land."*

Until this was clearly grasped the observation of earthquake phenomena, in the absence of a "guiding hypothesis," was vague and useless.

At present the objects and aim of *Seismology* are of the highest interest and importance to geology and terrestrial physics. It offers to us the only path to discover the real constitution and condition of the interior of our planet, and will become the key to open to us the true nature, depth of origin, and source of volcanic heat. In these respects one of the most primary objects of *Seismometry* is to arrive at a knowledge of the depth beneath the earth's surface from which earthquake shocks are delivered, *i. e.*, the depth of the *origin*.

The observer must form clear conceptions of the fundamental conditions of propagation of seismic waves. Fig. 1 represents a vertical section of part of the earth in the plane of a great circle, cutting the surface at $h'h$, and passing through the origin of impulse at A—Ap, being the prime vertical to that point whose depth beneath the surface is BA. The wave starts from the origin (assuming the earth's mass

homogeneous) with one normal and two transversal vibrations.   Neglecting the latter for the present, the wave may be imagined trans-

Fig. 1.
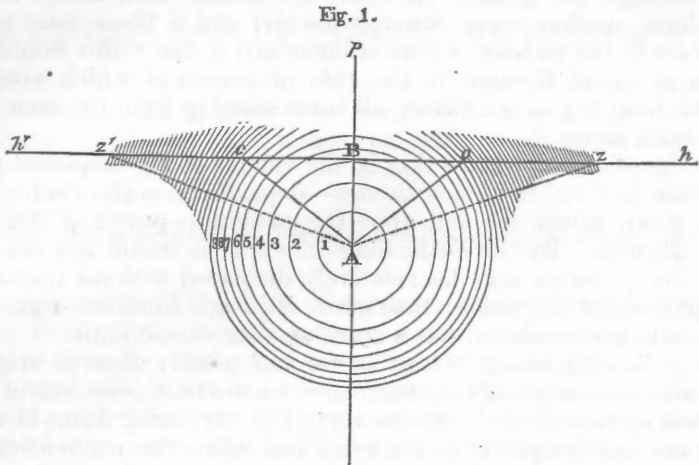
ferred outwards, in all directions in concentric spherical shells, whose volume at the same phase of the wave is constant.   The interval between any two such shells, therefore, diminishes as r^2, r being the mean radius, and the overthrowing energy of the shock in the direction of r varies inversely as the square of the distance from the origin.

The shock reaches the surface at B directly above the origin vertically, but for all points around that it emerges with angles getting more and more nearly horizontal as the distance measured on the surface increases.   The intersecting circle of any one shell with the surface, which is that of simultaneous shock, is the *coseismal line*, or crest of the earth wave, circular (like the circles on a pond into which a stone has been dropped) if in a homogeneous medium, more or less distorted if in a heterogeneous one, such as constitute the various formations of the earth, but always a closed curve.   The transverse vibration is transmitted outwards in the normal direction (Ac) more slowly than the normal one, which is one cause of the small jarring impulses often felt after the great shock.   (For more complete information as to the physical and mathematical conditions, see "Jamin, Cour de Physique, 1858," 9th and 10th chapters; Rankine's "Applied Mechanics," chap. 3, sec. 1, and chap. 5, sec. 4, and passim; Dr. Young's Lectures, "Nat. Phil.," passim, but especially lecture 8, 13, 31, 49, and 50; Herschel, Art. Sound, "Encyc. Metrop.;" Hopkins, Report, "Brit. Ass. Trans.," 1847–'48; Mallet's Fourth Report, Facts and Theory of Earthquakes, "Brit. Ass. Trans," 1857–'58.)

Observers in earthquake countries should make themselves familiar with the usual features, succession of events, and concomitant which, with a certain sort of regularity, apply to all earthquakes. Mr. R. Mallet's "First Report upon the Facts of Earthquake" Trans. Brit. Ass. for 1850, gives these in a condensed and sys-

tematic form. The greatest shocks are not the most instructive, except as to secondary effects; but every great shock is usually followed by several smaller; the first should therefore be viewed as a "notice to observe" the latter carefully. Earthquakes must not be confounded, either with the forces producing *permanent* elevations of the land, or with these elevations themselves. "An earthquake, however great, is incapable of producing any permanent elevation or depression of the land whatever, (unless as secondary effects;) its functions of elevation and depression are limited solely to the sudden rise, and as immediate fall, of that limited portion of the surface through which the great wave is actually passing momentarily." The one class of phenomena must be held as distinct from the other as the rise and fall of the tide is distinct from the momentary and local change of sea-level produced by the waves of its surface.

The phenomena of every earthquake may be divided into—1st. *Primary*, or those which properly belong to the transit of the wave or waves through the solid or watery crust of the earth, the air, &c.; 2d. *Secondary*, or the effects produced by this transit; and both must be kept distinct from co-existent forces, such as those of volcanic eruption, permanent elevation or depression of land, &c., which, however closely they may be connected with the originating impulse of the earthquake, form no true part of it, though they usually complicate its phenomena.

The *centre of impulse*, or *origin* of earthquakes, is generally conceived to be at and due to a sudden volcanic outburst, or sudden upheaval or depression of a limited area, or sudden fracture of bent and strained strata, or probably to the sudden formation of steam from water previously in a state of repulsion from highly heated surfaces, (spheroidal state,) and which may or may not be again suddenly condensed under pressure of sea-water, or possibly to the evolution of steam through fissures and its irregular and *per saltum* condensation under pressure of sea-water. This origin should be carefully sought for as to its nature and position.

An earthquake may have its *origin* either *inland* or *at sea;* and as this may be, a different set of phenomena will present themselves. In the former case we may expect, in the following order : 1st. The *Great Earth-wave*, or *true shock*, a real roll or undulation of the surface travelling with immense velocity outwards in every direction from the point vertically above the centre of impulse. If this be at a small depth below the surface, the shock will be felt principally horizontally; but if the origin be profound, the shock will be felt more or less vertically; and in this case also we *may* be able to notice two distinct waves, a greater and a less, following each other very rapidly: the first due to the originating normal wave; the second to the transversal waves vibrating at right angles to it. If we can find the point of the surface vertically over the origin, and the direction of emergence of the shock at a distant point, or the angles of emergence at two distant points, neither of which is vertically over the origin—*i. e.*, in one coseismal line—we can find the depth of the origin from the surface by methods pointed out in

Mr. Mallet's Fourth Report on Facts and Theory, &c., "Brit. Ass. Trans.," 1857–'58, but which space will not permit of transcript here.

An erroneous notion of the dimensions of the great earth-wave must not be formed from its being called an undulation; its *velocity* of translation upon the earth's surface is great occasionally in hard, elastic, and unshattered formations, probably as much as thirty miles per minute, and the wave or shock moving at this rate has been recorded to have taken some seconds to pass a given point; if so, its *length* or *amplitude* is often several miles. Its *altitude*, however, is not great, and, as may be seen from Fig. 1, continually diminishes as the wave passes outwards from the origin.

Before, during, or immediately after the passage of the great earth-wave or main undulation, a continuous violent tremor or short quick undulation (like a short chopping sea) is often felt. This may arise from secondary elastic waves accompanying the great earth-wave, (like the small curling or capillary waves on the surface of the ocean swell,) produced probably by comparatively minute secondary impulses, due to the discontinuous and heterogeneous nature of the formations through which the normal wave has been propagated. Sometimes, however, a number of shocks occur so rapidly as to convey the impression of a continuous jar or tremor, and may be succeeded by one or more *great* shocks; this is probably the source of "tremor observed before the shock," as the subsequent arrival of the transversal waves is of the tremors after it. (For other complications of the phenomena, see Mallet's 1st, 2d, and 4th Reports, Brit. Ass.) It is very desirable that the interval in time between these minor oscillations should be observed by a seconds watch, and also their total duration at each epoch of motion. Former narrators often confound the whole of each epoch of such rapidly recurrent shocks with one shock supposed to last a considerable time.

2d. When the superficial undulation of the earth-wave, coming from inland, reaches the shores of the sea, (unless these be precipitous, with deep water,) it may lift the water of the sea up and carry it along on its back, as it were, as it goes out into deep water; for the rate of transit is so great that the elongated heap of water lifted up has not time to subside laterally. This may be called the *forced sea-wave;* its elevation will be comparatively small, and a little less than the altitude of the earth-wave, when close to the shore on a sloping beach; and where the water is still, any observations that can be made as to the height of this fluid ridge will afford rude indications of the altitude of the earth-wave or shock.

Earthquakes, whether at sea or on land, seem to be only accompanied with subterranean noises when strata are fractured or masses of matter rent or blown away at volcanic origins. Where such is not the case, the two preceding are the only waves to be expected from an earthquake of inland origin; but when fracture occurs, then at the moment of the shock, or very slightly before or after it, we shall

hear, 3d, the *Sound-wave through the earth;* and at an interval longer or shorter after this, 4th, the *Sound-wave through the air.*

Again, when the origin of the earthquake is under the sea, (and such seems to be the case with many great earthquakes,) we may expect in the following order: 1. The great earth-wave or shock; 2. The forced sea-wave, which is formed as soon as the true shock or coseismal undulation of the bottom of the sea gets into shallow water, and forces up a ridge of water directly above itself, which accompanies it to shore, and which seems to be the cause of that slight disturbance of the margin of the sea often noticed as occurring at the moment of the shock being felt; 3. The sound-wave through the earth, (as in the former case;) 4. The sound-wave through the sea, which arrives after that through the earth, but prior to, 5. The sound-wave through the air. Where the originating force is not a single impulse, but a quick succession of these, or a single impulse extending along a considerable line of disturbance, passing away from the observer, the sound-waves will be rumbling noises, and may be confounded in each medium more or less; and where no fractures or explosions occur, the sound-waves may be wholly wanting.

Lastly, and usually a considerable time after the shock, the *great sea-wave* rolls in to land. This is a *wave of translation;* a heap of sea-water is thrown up at or over the origin of the earthquake by the actual disturbance of the sea-bottom, or in the direction, and by the emergence, of the earth-wave beneath the sea at a large angle to the horizon, and begins to move off in waves like the circles on a pond into which a pebble is dropped; and its phenomena depend upon laws different from any of the other (elastic) waves of earthquakes.

The original altitude (above the plane of repose of the fluid) and volume of this *liquid wave* depend upon the suddenness and extent of the originating disturbance, and upon the depth of water above its origin. Its velocity of translation on the surface of the sea varies with the depth of the water at any given point, and its form and dimensions depend upon this also, as well as upon the sort of sea-room it has to move in. In deep-ocean water one of these waves may be so long and low as to pass under a ship without being observed; but as it approaches a sloping shore its advancing slope becomes steeper, and when the depth of water becomes less than the altitude of the wave, it topples over, and comes ashore as a great breaker. Sometimes, however, its volume, height, and velocity, are so great that it comes ashore bodily and breaks far inland. The direction from which it arrives at any given point of land does not necessarily infer that in which the origin may be; as this wave may change its direction of motion greatly, or become broken up into several minor waves in passing over water varying much and suddenly in depth, or in following the lines of a highly-indented or island-girt shore.— (See Airy on Tides, Encyc. Metrop.; Russel, Report on Waves, Brit. Ass., 1844; Bache, Great Sea Waves in Pacific, Amer. Jour. Science, vol. xxi, 1856; Mallet, 4th Report, 1857–'58; Darwin, Voyage of the Beagle.)

Observations of each of these classes of waves which we have thus briefly described may be made either directly by the aid of instruments, specially provided or extemporaneously formed, or indirectly by proper notice of certain effects which they produce on objects upon the earth's surface.

Direct observations by complete self-registering seismometers do not come within our present scope. They will be found treated of at large in Mallet's 4th Report, Brit. Ass., 1857-'58, where the principles, construction, methods of observation, and applications of the best known instruments are described.

Whatever instruments be employed, however, it is found that perturbations in the main directions of emergence at the surface, of the normal earth-wave, due principally to heterogeneity of structure in depth, and to inequality of surface, are such as to render a special choice of district necessary, in attempting any seismometrical researches (even with perfect instruments) having in view the determination of the position of the origin or focus of disturbance. This choice, according to our present knowledge, must be determined by the following conditions:

1. The whole surface-area of observation must be, as far as possible, uniform in geological structure, and so to as great a depth as possible. If of stratified rock, not greatly shattered and overthrow but (viewed largely) level or rolling only. The harder and more dense and elastic the formation the better, but neither intersected by long and great dikes or igneous protrusions of magnitude, nor suddenly bounded by such formations.

2. The surface must not be broken up into deep gorges, and rocky ranges, and valleys. Seismometry, in a high and shattered mountainous country, can scarcely lead to any result but perplexity. If the surface be deeply alluvial all over, it is less objectionable than valley-basins and pans of deep alluvium, with rocky ribs between them.

3. The size of the area chosen for observation must bear a relation to the force of the shocks experienced in it. *Moderate shocks* are always best for observation, and in *large areas* of the *most uniform character of formation and surface* will give the most trustworthy indications.

4. If several seismometers are set up in the area they should be all placed on corresponding formations, either all on rock, or all on deep alluvium. The rock, when attainable, is always to be preferred. Three seismometers, at as many distant stations, will be generally found sufficient, if the object be chiefly to seek the focal situation and depth.

We, therefore, proceed to observations with extemporaneous instruments on the earth-wave or shock. The elements necessary to be recorded are such as will enable us to calculate: 1. The direction in azimuth of the wave's motion upon the earth's surface; and also its direction of emergence at the points of observation. 2. Its velocity of transit upon the surface. 3. Its dimensions and form—i. e., its amplitude and altitude.

If a common barometer be moved a few inches up and down by the hand the column of mercury will be found to oscillate up and down in the tube in directions opposite to the motions of the instrument, the range of the mercury depending upon the velocity and range of motion of the whole instrument. A barometer fixed to the earth, therefore, if we could unceasingly watch it, would give the means of measuring the vertical element of the shock-wave; and if we could lay it down horizontally, it would do the same for the amplitude or horizontal element. This we cannot do; but the same principle may be put into use by having a few pounds of mercury, and some glass tubes bent into the form of **L**, sealed close at one end, and open at the other; the bore being under two-tenths of an inch in diameter, and each limb about fifteen inches long. We shall also require some common barometer tubes of the same calibre: the open end being turned up like an inverted syphon, and equal in bore to the rest of the tube.—(See *Fig.* 5.) The **L** tubes are used for the horizontal, the others for the vertical elements.

To fit the **L** tubes for use fill each partly with mercury, and so adjust it that a column of five inches in length shall be in each limb of each tube, when held as in *Fig.* 2; the limb *a b* horizontal, and the vertical column being supported as in a barometer. Tie four of these tubes so prepared together, back to back, so that if one horizontal limb face the north, the others shall face east, south, and west, respectively, as in *Fig.* 3. In this position secure them all down upon a broad, stout board, that can be itself fixed to a surface of rock, or other fixed surface of the earth.

Sealed end.

Fig. 2.

An index or marker must now be prepared for each tube; for one of these, cut a common piece of card two inches long by rather less than two-tenths of an inch wide, nick it partly through along a centre longitudinal line, and double it down the long way, so that the two segments shall stand at rather less than right angles to each other; cut a cylindrical slice of cork one-eighth of an inch thick, of a diameter such that it will go easily into the tubes; attach the bit of cork with glue or sealing-wax to the end of *one* wing or segment of the folded card, leaving the other free, and thrust the whole into the horizontal limb of the tube until the cork just touches the mercury, and so for the others. This marker is shown at rather more than full size in

Sealed ends.

Fig. 3.

W   E

N

S

Cork.   Free side.

Fig. 4.

Attached side.
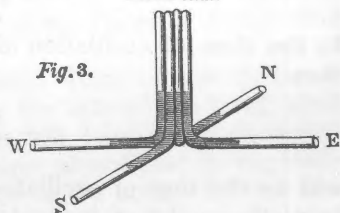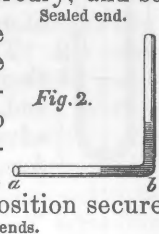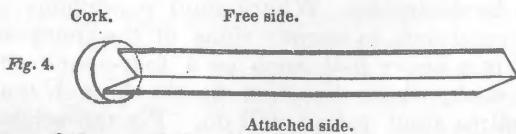
The edges of the card, having a certain amount of elastic extension, must slightly grip the inside of the tube.

It will now be found, if horizontal motion be given to the system

of four tubes—say, from south to north—that the marker in the southern tube will be pushed southward a certain space by the movement of the mercury, and will remain to point out the space when the mercury has returned to rest. If the motion be in some direction between two adjacent tubes—say, from southeast to north-west—the markers in the south and east tubes will both show a certain motion, equal in this case, but in others with a certain ratio to each other, by which the direction between the cardinal points may be calculated.

For the vertical element: let the barometer tube, *Fig.* 5, be filled with mercury so that about six inches shall stand in the open end *a*, into which thrust a marker, as in *Fig.* 4, and about twelve inches in the sealed limb; place this vertically, and secure it to a fixed mass of rock, a heavy low building or large tree; from the amount to which the marker is found moved up in the tube the altitude of the wave may be found; and it is obvious that, by the conjoint indications of the four horizontal tube-markers, and this vertical one, the direction of emergence of the wave is determinable.
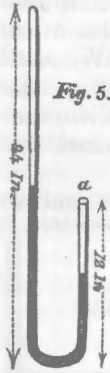
These instruments are of the nature of fluid pendulum; their use assumes the velocity of the earth-wave constant, and, in common with all pendulums, they have certain disadvantages as seismometers.—(See Mallet, 4th Report Brit. Ass.) If

$$T = \pi \sqrt{\frac{l}{g}}$$

be the time of oscillation of any solid pendulum whose length is *l*, then

$$T = \pi \sqrt{\frac{l}{g\,(\sin. a + \sin. a')}}$$

will be the time of oscillation of any such fluid pendulum, *a* and *a'* being the angles of inclination of the limbs of the tube to the horizon. Where these are parallel and vertical, sin. $a =$ sin. $a' = 1$ and

$$T = \pi \sqrt{\frac{0\cdot5 \times l}{g}}$$

They are much superior to common solid pendulums, where the dimensions of the shocks are small; but where these are great and very violent, heavy solid suspended pendulums will be found more applicable; the length of the seconds pendulum for latitude Greenwich will always be desirable. Where fluid pendulums are not attainable, a solid pendulum to answer some of the purposes may be thus prepared: Fix a heavy ball, such as a four-pound shot, at one end of an elastic stick, whose direction passes through the centre of gravity of the ball; a stout rattan will do. Fix the stick vertically in a socket in a heavy block of wood or stone, and adjust the length above the block as near as may be to that of the seconds pendulum for Greenwich. Prepare a hoop of wood, or other convenient mate-

rial, of about eight inches diameter; bore four smooth holes through the hoop in the plane of its circle, and at points ninety degrees distant from each other; adjust through each of these a smooth round rod of wood, (an uncut pencil will do well,) and make them, by greasing, &c., slide freely, but with slight friction, through the holes. Secure the hoop horizontally at the level of the centre of the ball by struts from the block, and the ball being in the middle of the hoop, slide in the four rods through the hoop until just in contact with the ball.

It is now obvious that a shock, causing the ball to oscillate in any direction, will move one or more of the rods through the holes in the hoop, and that they will remain to mark the amount of oscillation.

A similar apparatus, with the pendulum rod secured horizontally, (wedged into the face of a stout low wall, for example,) will give the vertical element of the wave. Two of these should be arranged—one north and south, the other east and west. One objection to this and all apparatus upon the same principle is, that as the centre of elastic effort of the pendulum rod never can be insured perfectly in the plane passing through the centre of gravity of the ball for every possible plane of vibration, so an impulse in a single plane produces a conical vibration of the pendulum, and hence the ball deranges the position more or less of the index rods which are out of the true direction of shock. Moving the apparatus by hand, and a little practice in observation of its action, will, however, soon enable a pretty accurate conclusion as to the true line of shock to be deduced from it.

It will be manifest that the observer must record minutely the dimensions and other conditions of such apparatus, where not permanently kept, to enable calculations as to the wave of scientific value to be made from his observations of the range of either fluid or solid pendulums.

A common bowl partly filled with a viscid fluid, such as molasses, which, on being thrown by oscillation up the side of the bowl, shall leave a trace of the outline of its surface, has been often proposed as a seismometer. This method has many objections; it can only give a rude approximation to the direction of the horizontal element; but as it is easily used, should never be neglected as a check on other instruments. A common cylindrical wooden tub, with the sides rubbed with dry chalk, and then carefully half filled with water or dye-stuff, would probably be the best modification.

Another extemporaneous instrument for measurement of vertical motion in the wave may be sometimes useful. Make a spiral spring of eighteen inches or so in length by twisting an iron wire of one-eighth of an inch diameter round a rod of about $1\frac{1}{4}$ inch diameter, (the staff of a boarding-pike;) suspend it by one end vertically from a fixed point, and fix a weight (a twelve-pound shot will do) to the lower end, and below and in a line passing vertically through the centre of gravity of the weight fix the stem of a common tobacco pipe; let the lower end of this stem just dip into a deep cup filled with pretty thick common ink or other colored fluid; the action of this needs no description.

The preceding instruments suffice at once to give the direction of transit of the earth-wave and its dimensions; its rate of progress or

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transit over the shaken country remains to be observed, and wherever it may be possible to connect three or more such instruments as have been described at moderately distant stations, say 15 to 30 miles apart, by galvanic wires, so as to register at one point the moment of time at which each instrument was affected, the best and most complete ascertainment of transit rate may be expected.   Galvano-telegraphic arrangements of the simple character required are become familiar, and are easily set to work.   The best seismometer to which they can be applied (for voyagers) is that. described in Mallet's 4th Report, &c., page 87, and plate xv; and no surveying ship proceeding to earthquake regions should be unprovided with three such seismometers and the requisite time-recording apparatus.

A still simpler form of rough seismometer suited to the resources of distant and isolated observers remains to be described.   It depends upon principles altogether different from those already mentioned, and is most applicable to seismic districts where the angle of wave emergence is not steep—*i. e.*, where shocks are usually nearly horizontal.

Every body overthrown by an earthquake shock is upset by its own inertia causing it to move in the opposite direction to that in which the ground has moved under it.   Thus a wall falls towards the south if the shock passes across its length from south to north, and if any such homogeneous parallelopiped or right rectangular prism, standing on end upon a level surface, be so upset by its own inertia, the supporting surface being suddenly moved beneath it, in the direction of its own plane, (as by the horizontal component of an earthquake shock,) it may be shown that the velocity of the surface must be

$$V^2 = \frac{4}{3} g \sqrt{a^2 + b^2} \times \left(\frac{1 - \cos. \theta}{\cos^2 \theta} \right)$$

where a is the altitude of the solid, b its diameter of base or thickness, and θ the angle formed by the side, and a line drawn through the centre of gravity to the extremity of the base and $V = 2 g$. This velocity is independent of the density or material of the solid, because the oversetting force, being its own inertia, is always proportional to the density.

This is the foundation of all accurate and useful observation of dislocated and overthrown buildings in countries that have suffered by earthquake, and by which not only may the direction of (the horizontal component of) the earthquake shock be obtained, but a close approximation made to its velocity.

With a given velocity, V, therefore, it is possible to assign the dimensions a and b, such that the solid shall *just overset*, and with this velocity a similar solid, but having θ greater, shall remain unmoved; assuming always, that friction against the supporting surface gives sufficient adhesion to prevent sliding.

If, in place of a square prism, such as a wall, the solid be a right cylinder like a column, the diameter of its base being b, then

$$V^2 = \frac{15 \, b^2 + 16 \, a^2}{12 \, a^2} \times g \sqrt{a^2 + b^2} \, (1 - \cos \theta).$$

This gives the means of constructing a seismometer of great simpli-

city, that (in the absence of better means) shall give the horizontal velocity of shock within a norrow limit of error.

Let there be constructed two similar sets of right cylinders—say, each set six to twelve in number—all of equal height (a) and of the same sort of material, but varying in diameter in each set, with a uniform decrement from the greatest to the least.

Convenient dimensions for earthquake observations of *mean* intensity will be such that the cylinder of largest diameter shall have its altitude equal to three diameters, or $b = \dfrac{a}{3}$; and that the cylinder of least diameter shall have its diameter one-third of that of the greatest one, or $b = \dfrac{a}{9}$. Any number of cylinders of intermediate diameters

Fig. 6.

Sand Bed.
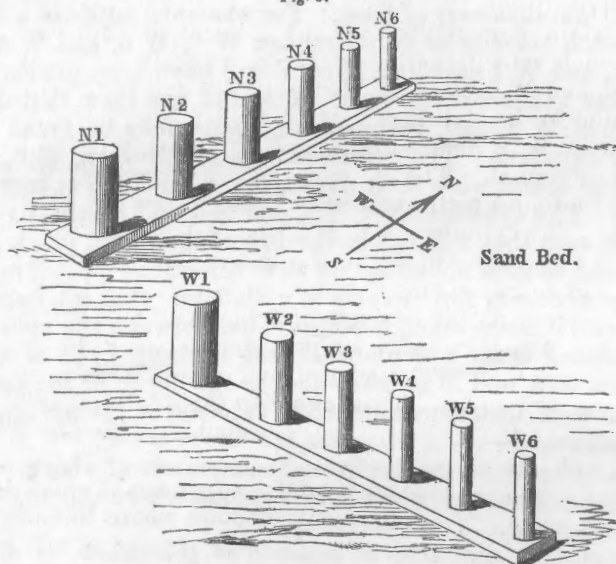
may be interpolated between, and the greater the number the more accurate the instrument becomes. A series of six to ten in each set will, however, be sufficient for any purpose. For observation of shocks of extreme violence, larger diameters in proportion to altitude should be chosen for all the cylinders

The material of the cylinders is not important—cast-iron, stone, pottery, or other substances at hand—whose *arrisses* will not crumble away by being overthrown—may be used; but no material will be found more convenient than some hard heavy seasoned wood, of uniform substance, straight grain, and equable specific gravity, from which the cylinders can be formed in the lathe, and their bases brought perfectly square to the axes with facility.

Upon any horizontal and solid floor let two planks be placed, as in fig. 6, with their directions in length respectively lying N. and S. and E. and W., each plank to be about three inches in thickness, and in width equal to the diameter of the largest cylinder, and its length

such that the set of cylinders when placed upright and equidistant thereon shall have a space greater than the altitude between each. Thus, if the cylinder of largest diameter have $b = 0.5$ of a foot, the length of plank will. for a set of six, as in the figure, be about 12 feet. These base planks being *fixed* level and solid, the floor is to be levelled up with dry sand to their upper surfaces, and the two sets of cylinders adjusted to their places, one set running in an E. and W., the other in a N. and S. direction, so that in whatever direction the horizontal component of shock may move, the overthrown cylinders of one or the other set shall fall transversely to the lengths of the plank bases and *lodging on the sand-bed, remain exactly in the position as to azimut in which they were overthrown.* If now a shock of any horizontal velocity, capable of overthrowing some of the cylinders, but not all of them, arrive, it will throw down at once all the narrower ones, and up to a certain diameter of base. For example, suppose a N. and S. shock of such velocity as to overthrow W 6, W 5, and W 4, leaving W 3, W 2, and W 1 standing, then V will have been *greater* than the velocity due to the overthrow of W 4, and *less* than that due to the overthrow of W 3, and, within those limits, may be found from the preceding equation. The cylinders here overthrown, W 6, W 5, and W 4, will be found with their axes lying N. and S., at rest upon the sand-bed. The cylinders N 6, N 5, and N 4 will be also overthrown; but in this case they will fall in the line of their own plank basis, and *may* roll, and so give no indication as to direction of shock in azimuth Hence the necessity for two sets of cylinders. One set, however, will be sufficient, if space enough be provided between the cylinders, and each be placed upon a cylindrical and separate basis of a diamet equal to its own, and in height equal to the depth of the sand-bed.

This form of instrument, then, is capable of giving approxima determinations of—

1. The velocity of the horizontal component of shock, neglecting the vertical component, which may be done where the angle of emergence is not great.

2. The surface direction in azimuth of the shock, or direction of horizontal component of the seismic wave.

3. Its absolute direction of primary movement, viz: *the direction of translation of the wave, which always coincides with the direction of molecular displacement of the wave itself in the first half of its complete phase—e. g.*, if a shock in N.S. azimuth throw the cylinders to the southward, then the wave has traversed from S. to N.

4. The exact time of the transit of the shock at the instrument may be also indicated, if either the narrowest cylinders, N. 6 and W. 6, (which by hypothesis must be *always* overthrown,) be connected with a house-clock in the way about to be described, so as to stop it at the moment of overthrow, or, still better, if a separate cylinder of even less stability be appropriated to this purpose.

Three such sets of instruments at distant stations may of course be easily connected by galvanic wires, so as to give the transit time at each accurately, and hence the transit rate.

Three or more distant observers, with chronometers, may of course

observe this, but such observations can seldom be very numerous or extend over a large tract of country, and without automatic instruments shocks are almost certain to be missed at one or more stations; yet it is most desirable that a network of such observing points should be stretched over the shaken country. For this purpose common house-clocks, situated at several distant points, may be easily arranged, so that the pendulum shall be brought to rest and the clock stopped at the moment that the shock passes.

Fig. 7 shows part of the case and pendulum of a common clock. To fit it for this purpose, bore two holes of a quarter of an inch diameter, one through either side of the clock case, at *a b*, at the level of the lowest point of the pendulum-bob, and in the plane of its vibration; round off the edges of these holes, and grease them.

Fig. 7.
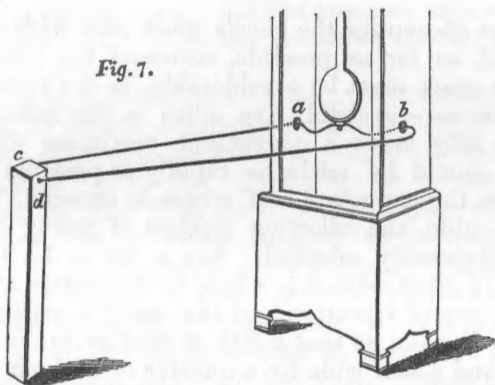
In the centre of a piece of fishing-line or stretched whip-cord make a loop and pass it round the screw or other lower projection of the pendulum-bob; pass the two free ends of the cord out, one through each of the holes in the sides of the clock-case; provide a squared log of heavy wood of about five or six inches thick each way, and from four to five feet in height; cut both ends off square, and stand the log upright on one end directly opposite the dial of the clock.

Measure off equal lengths of the cord at each side of the pendulum, and make fast their extremities to the two opposite sides of the upright log, *c d*, close to the top; bring the log backwards from the clock now, until the pendulum being at rest, both cords are drawn tight; and then advance it two or three inches towards the clock, so that the cords may be slacked down into a festoon or bend at each side of the pendulum; and *within the clock-case*, so that the pendulum may have room to swing freely; and *very slightly* wedge the cord to keep it so, through the holes in the clock-case, and from the outside; see that the log rests firmly and upright upon a firm floor; and now set the clock going. The length of the cords, or the distance of the log from the clock in relation to its height, must be such that if it fall *towards* the clock it shall bring the cords up tight before the upper part of the log touches the ground. It is now obvious that, in whatever direction the log may fall, it will arrest the motion of

the pendulum and stop the clock within less than a second of the true time of transit of the wave at the spot.

If the adjustments are similar for all the clocks this error will be constant for them all; and if the true time be noted at the principal station it can be got for the rest.

Clocks with seconds pendulums only, should be chosen for this use. They should be all set by one chronometer, and their errors afterwards taken.

Where convenient, the pendulums should be all placed to swing north and south, or east and west; and in this case the sides of the logs will face the cardinal points, and the directions of their fall (where not entangled) be a rude index of that of the wave. It will be also desirable to place a tub of fluid to mark direction with each clock.

The positions chosen for the clocks must vary with circumstance but they should, as far as possible, surround the principal station; their distances apart must be considerable, as the speed of the wave or shock is immense—probably five miles is the ordinary minimum and thirty to fifty miles a convenient maximum distance. Such arrangements should be made as rapidly as possible after the first shock has given the expectation of others to succeed.

When practicable, the following method of fitting common clocks may be advantageously adopted. Let a, fig. 8, be the pendulum bob; fix a pin of stout wire into a hole in the centre of it, b, at right angles to the plane of vibration; cut two small mortices through the sides of the clock-case, so that a lath of deal or other light wood, of about an inch and a half wide by a quarter of an inch thick, may be passed through from c to d, just in front of the bob and clear of it.

Mark the length of the arc of vibration on the lower edge of the lath, and cut this length into nicks or teeth like a rack, of about three-eighths of an inch in depth and breadth each. Place the lower edge of the lath horizontally, and just above and clear of the pin b,

Fig. 8.
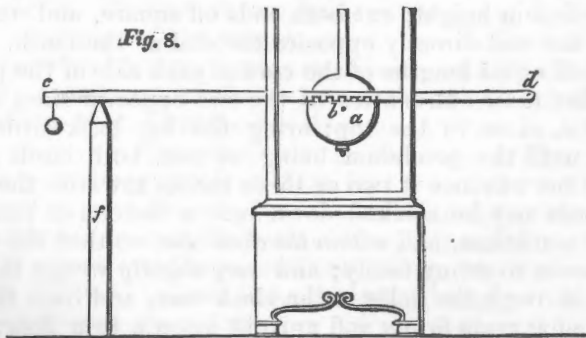
secure the end of the lath d by a wire pin or stud, as a fixed point, so that the end c is free to move in an arc of a few inches up and down round d as a centre. Prepare a vertical log of wood f, of the size and form already described, but cut its upper end to a square pyramid, the flat surface at the top being reduced to about a quarter

of an inch square; adjust the length and position of the log, so that it shall form a support for the end of the lath c, as in the figure.

It is obvious that the moment the log f is overthrown by a shock the lath will drop at the end c, (which should be slightly weighted,) and the teeth or rack nicks catching the pin b of the pendulum-bob will stop the clock; on examining which, the dial will show the time to a second when the shock took place, and the tooth in the rack will show at what part of the arc of vibration the pendulum was arrested, which will obviously give the time of the shock to a fraction of a second.

This method may be applied to any form of clock, and with any length of pendulum. Observation should be accurately made by a seconds watch, or still better, with a Breguet chronoscope, which readily reads to $\frac{1}{16}$ of a second, of the total duration of the shock in passing the observer's station; and the observer should endeavor to record the number of small, rapidly recurrent shocks, and their total duration at each epoch.

Returning now to observations to be made upon the earth-wave, indirectly or by its effects, consisting principally of—1. Observations on buildings and other objects, fissured, dislocated, or thrown down. 2. On bodies bent, projected, displaced, or inverted. 3. Bodies twisted on a vertical axis, with more or less diplacement. Some of the most precious data are to be obtained, by the observation, after the earthquake, of the fissures and dislocations of *buildings*. Choice should be made of buildings rectangular in plan, of tolerably good masonry, and but one-story in height, such as churches, &c.; and as often as possible such should be chosen as have their principal walls running north and south and east and west. These may be advantageously described as *Cardinal buildings*. With a given force of shock, and in buildings of generally similar form, the extent of fissure depends chiefly upon the character and "bond" of the masonry. The direction of fissures is nearly vertical when due to nearly horizontal shocks; but those of steep emergence produce highly-inclined fissures, often crossing each other. Cardinal buildings exposed to shocks, the horizontal component of which is either N.S. or E.W., are fissured chiefly near the quoins, and through the walls whose planes are in the line of shock. But irregularities in the mass of the walls, due to apertures, the brittleness of masonry, and slight deviations from cardinal direction in the shock itself, frequently produce subordinate fissures in the walls transverse to its line of movement, when these are not overthrown.

When the direction of shock is diagonal to the plan of the walls, a triangular mass is dislodged from the upper part of each of the adjacent walls, at the quoin *from which* the wave comes. With steep emergence such masses may be dislodged from both quoins at the same end of a rectangular building, which is that *towards which* the wave moves. Heavy roofs and tiled or arched floors suffer most from shocks of steep emergence. Buildings situated near about vertically over the centre of disturbance present evidence of dislocation in every direction, *i. e.*, by the vertical, or nearly vertical, emergence

of the normal vibration, and by the nearly horizontal movements of the two transversal vibrations in orthogonal planes.

The observer must bear in mind that all these motions are due to the inertia of the bodies at the moment of the wave transit. The first tendency, therefore, of every body is to fall in a direction contrary to that of the wave's motion; but this is often perplexed by mutually supporting bodies, as cross walls—by the direction of the wave being one in which a fall is impossible, as when passing very diagonally through a long line of wall—by disintegration from the first wave, so altering the conditions of the bodies (walls, towers, &c.,) though short of producing a fall, as that the dislocation and fall produced by a succeeding one is not contrary, but in the same direction as the wave motion. When the shock emerges at a large angle to the horizon bodies are often projected, as stones out of or from the coping of walls: the size, weight, form, cement, sort of stone, distance thrown, and all other conditions of projection should then be carefully noticed. Isolated bodies, such as bells from belfries, balls or vases of stone, statues, &c., are often thrown from elevated points on buildings, and reach the ground after describing a trajectory path. The vertical height fallen through, and the horizontal distance thrown from the original position, with the form, dimensions, substance, weight, and mode of attachment of the body, being noted, afford elements for calculating the velocity of the wave transit if its direction if emergence be otherwise known, or *vice versa*.

Fissured or overthrown walls of buildings usually give approximations to the horizontal azimuth of shock, but may or may not give any decided response as to the direction of transit, *e. g.*, with a N. S. azimuth it may remain uncertain whether the transit was N. to S. or S. to N. Objects overthrown, such as images, altar candlesticks, pilaster slabs, pictures, that *can* fall only in one direction, may generally be found, such as will decide the question. Space will not permit of this part of the subject being treated systematically or fully. The observer should train his mind, by solving for himself various cases of the effects of shock on different sorts of buildings, &c., and he will see from the hints here given how much the value of his observations, in a recently shaken country, will depend upon the "nous" and adroitness with which he seizes upon the fit objects to afford him the best data. The note and sketch-books should be in perpetual use—no conditions essential for after calculation must be omitted—and the azimuths, directions, and emergence of the shock at every observed point marked upon the best maps as soon as possible. Azimuths must usually be taken with the prismatic compass, or pocket sextant, but should be plotted to the true meridian; and the magnetic variation should be determined at frequent intervals, especially in volcanic countries.

Bodies twisted on a vertical axis (such as the Calabrian obelisk see Lyell, "Geology") were formerly supposed to be due to a vorticose motion of the earth. This movement arises from the centre of gravity of the body lying to one side of a vertical plane in the line of shock, which passes through that point in the base on which the body

rests, in which the whole adherence of the body to its support, by friction or cement, may be supposed to unite, and which may be called the *centre of adhesion.*—(See Mallet, "Dynamics of Earthquakes," Trans. Royal Irish Acad., 1846.) The observer who fully masters these mechanical conditions of motion will see what elements he must collect, so that the motion impressed on bodies thus twisted may be used to calculate the velocity, &c., of the wave. All observations of this class, to be of scientific value, must comprise the materials, size, form, weight, sort of cement, base or foundation of the bodies disturbed, and measurements of the amount, &c., of disturbance, with any other special conditions which occur; and these will always be numerous, and demand the utmost alertness and scrutiny of the observer. The arc and azimuth of oscillation, with weight and length of chain or cord of suspended lamp set swinging by shock, often afford valuable information. The length to centre of oscillation is got by setting the lamp swinging, and noting the vibrations made in a minute, knowing the latitude; also iron crosses, or lamp irons *bent* by shock. The height, form, weight, exact section at the bend, and direction of deflection, to be noted.

Whatever difference in destructive effect may be due to formation or accident, it must be borne in mind that in every shock transmitted from a deep centre of impulse, and passing outwards in all directions in spherical shells, there will be a coseismal circle upon the earth's surface at some determinate horizontal distance from the central point vertically over the centre of impulse, in which the horizontal *upsetting* or *overturning* power will be a maximum, greater, cæteris paribus, than at any point within or without this circle: within, because there the direction of shock is more vertical, and therefore less calculated to overturn buildings; and without, because, though more horizontal, the power of the shock has become weakened by distance of transmission. This may be called the *Meizoseismal Circle* or *Zone*, having the radius B *c*, fig. 1. It may be proved that the angle of emergence for this zone of maximum overthrow is constant, and makes with the horizon an angle equal to 54° 44′ 9″ nearly, assuming the energy of shock in the normal to vary as the inverse square of the distance from the origin. If, therefore, the centre of the circle, or point-plumb over the origin be given, or three points can be fixed by observation in the meizoseismal circle, the *depth of the origin below the earth's surface* can be calculated by the following rule:

"Find the mean diameter of the meizoseismal circle. Then the depth of the origin or centre of impulse beneath the surface is equal to the diagonal of the square whose side is equal to the radius of that circle."

If the energy in the normal be assumed to vary simply as the distance from the origin inversely, then the constant angle of emergence for maximum overthrow is 45°, and the depth of centre of impulse is equal to the radius of the meizoseismal circle.

This gives us one method of approximating seismometrically to the depth below the surface of the volcanic "couche" beneath. The general horizontal direction of shock (radial from a point on the surface-

plumb above the centre of impulse) is subject to great and often very
perplexing and abrupt changes in azimuth and direction in very moun-
tainous or shattered country, or even in perfect planes of deep allu-
vium (like the basin of the Ganges) resting upon a highly uneven
skeleton of rock, or where the formations vary suddenly and much,
or are very discontinuous. The change often amounts in direction
to total inversion, and in azimuth to 90°.

Great perturbation of direction is also produced by the abutting of
one mountain chain upon another, which usually alters the apparent
angle of emergence also. The methods of disentangling these larger
and complex phenomena exceed the limits here imposed.—(See 4th
Report Brit. Ass. Trans., 1857–'58.)

Amongst doubtful phenomena on record are *inversions* of bodies,
such as parts of pavements turned upside down, &c.; such cases, or
any strange and unaccounted for phenomena, deserve special atten-
tion.—(See 1st Report, "Facts of Earthquakes," section 6, Secondary
Effects; "Cosmos," vol. iv., Sabine's Translation.)

In traversing an extensive city, or thickly-built-over country, to
observe the shattered buildings—having first ascertained generally
the line of motion of the wave—the observer should remark where
its directon of motion has appeared to change as it passed along, and
note all the conditions that seem to have there affected it. He shoul
also obtain decisive evidence of its actual transit, for sometimes the
wave seems to emerge all but simultaneously over a vast tract of
country, where the origin is deep-seated, and nearly vertically below.
Changes in the rate of transit horizontally, or in the energy of the
wave, should be noted by its effects on *similar* objects at *distant*
spots. These changes may be expected at the lines of junction of
different rocks or other formations. Evidence should also, if possi-
ble, be got of any breaking up of the primary wave into secondary
waves, as of several shocks being felt where only one has occured
further back.

All evidence should, as far as possible, be *circumstantial*. Nature
rightly questioned never lies; men are prone to exaggerate, at least
where novel and startling events are in question.

Various *local conditions* must be recorded: the great features of the
geological formations of the region, not only the successive under
lying rocks, with the general directions of bedding, lamination
joints, &c., but *the topographical character* of surface, the direction
and altitudes of the chief mountain ranges and of the main river
courses, the depth and description of its loose materials, their varia-
tions and extent, and the same for the surrounding districts, from
whence and towards which the earth-wave travels especially. The
deeper a knowledge can be got by exposed sections, &c., of the rock
of the shaken district the better; the proximity or otherwise to vol-
canic vents, active or passive, the lithological character of material
of the country shaken, whether broken, solid, or fissured; if the latter,
their general directions, dip, &c., whether dry or flooded, and the
effects on the transit of the wave, of changes in any or all of these
conditions; places least and most affected by the shock, and whether

there be some free from any, and their local conditions, to be par-
ticularly noted.

Referring now to *secondary phenomena*, or effects resulting from the
transit of the earth-wave, (other than merely measures of it,) we
should observe falls of rock, or land-slips, to which most of the con-
ditions of shattered buildings apply. Land-slips change their initial
directions frequently, in consequence of moving over curved or
twisted surface of rocks; thus the previously straight furrows of a
field may be found twisted after an earthquake. Scratches or furrows
engraven on rocky surfaces by such land-slips should be looked for.

Sometimes great sea-waves are produced by the fall into the sea of
rock or land-slips, which need to be carefully distinguished from the
true great sea-wave produced by an original impulse of the sea-
bottom. Land-slips often dam rivers, fill up lakes; and various
changes of surface again produce basins for new lakes, to be filled
by the changed river-courses. The circumstances, as far as possible,
should be accurately observed, and the causation of the events
unwound, and all such phenomena cautiously separated from actual
ejections of water, (temperature to be ascertained,) which are *said*
sometimes to have happened on an immense scale.—(Humboldt, Per-
sonal Narrative; "Cosmos," vol. iv.)

Fissures containing water often spout it up at the moment of shock.
Wells alter their water-level, and sometimes the nature of their con-
tents; springs become altered in the volume of water they deliver.
The directions of the fissures, and the relations of such directions to
that of the shock, should be ascertained, and any changes in the
temperature of wells noted. Ejections from holes or fissures of
strange liquid or solid matters, sometimes of dry ashes or dust, are
recorded, and occasionally fiery eructations or smoke *are said* to have
occurred, especially near volcanic centres, and blasts of steam vapors
or gases, whose chemical characters should be in all the above cases
observed as far as possible. The dust of overthrown buildings, or
that produced by the rending of rocky or other masses, must not be
confounded with these. Fissures, sometimes of profound depth, open
and remain so, or close again; their directions, dimensions, time and
order of production, and closing up, and the formations in which they
occur, to be noted; bodies engulfed to be detailed as future organic
remains. Fissures in solid rock arise either from the effects of inertia
or from the range of molecular displacement of the passing wave
exceeding the elastic limit of the materials disturbed; but fissures in
earth or other discontinuous and very imperfectly elastic masses seem
due only to the secondary effects of the shock, producing land-slips,
subsidations, &c.—(See "First Report on Facts of Earthquakes,"
sec. 6, Secondary Effects.) *Permanent elevations and depressions* of
the land usually accompany earthquakes, and are of much importance
to science, but, as already remarked, must be viewed as clearly
distinct, from the earthquake itself. Such elevations or depressions
have a common cause with the earthquake; both are due to the
volcanic efforts beneath, but are not the less absolutely distinct
phenomena, to confound which is to lose sight of all true science in

both. The observation of these should never be neglected, though rather belonging to geology proper. The half-tide level must in all cases be taken as the datum-plane for all questions of level, and opportunities diligently sought for along beaches, quays, wharfs or inland along mill-streams or irrigating channels, &c., where alterations of level may be trustworthily evinced by changes of depth or run of water. Occasionally local, but widely-extended, permanent elevations or depressions accompany earthquakes, which seem to result from lateral compression, and not from direct elevatory forces. These should be distinguished from the preceding.

Rivers are stated to have sometimes run dry during earthquakes, and again begun to flow after the shock. This is presumed to arise either from the transit of an earth-wave along their courses up stream, thus damming off their sources, or from sudden elevation of the land, and as sudden depression. Where well observed, however, it has nearly always been found due to sudden damming up by falls of rock or earth at narrow points of their courses, the débris being soon afterwards swept away.

Observations of the *forced sea-wave*, whether produced by the earth-wave going out to sea or coming in from it, will be nearly the same. It is desirable to find its height above the surface of repose referred to half-tide level, and its length or amplitude; but from the extreme rapidity of its production and cessation, or conversion into small oscillatory waves lapping on the beach, and its generally small altitude, observations are extremely difficult; they are only possible when the surface of the sea is perfectly calm, and then must be left to the skill of the observer in taking advantage of local circumstances, and of evidence as to the visible circumstances of this wave, which occurs at the instant the shock is felt.

Observations of the *waves of sound* through the earth, the sea of fresh water, and the air, are indicated pretty fully by the description of these waves already given. The sound-wave through the earth travels probably at the same rate as the shock or earth-wave; it is, in fact, *the shock (or its fractures) heard.* Notice if any and what sound is heard before, along with, or after the shock is felt. An observer, putting one ear in close contact with the earth, and closing the other, will hear the sound-wave through the earth separate from that through the air, and thus hear sounds otherwise inaudible. So, also, an observer immersed in the sea will hear the sound-wave through it, sometimes without any complication of that through the earth.

An exact description of the character and loudness of the sounds heard, and the places in an extensive district where each was heard loudest and faintest, with the nature of the rock formations at these spots, should be noted. The duration of the sound from first to last, through either medium, accompanying each shock, is important. Circumstances of a character analogous to those upon which the rumbling and reverberation of thunder depend, may affect these sounds transmitted through the earth and thence to the air.

Observations on the *great sea-wave* should embrace, for each wave,

its height, its amplitude or length, its velocity, and direction of translation. The height to be taken above the plane of repose of the fluid, and referred to half-tide level. These waves, when on their grandest scale, defy any methods of direct admeasurement; but observations of their results, such as the height to which they have reached on mural faces of rock, or on such buildings, &c., as may have withstood them, or eye-sight observations made at the moment of transit of the crest of the wave cutting distant objects, should not be omitted. When of a manageable size the height of the crest may be pretty closely obtained by the traces on wharfs, buildings, &c., or on posts or piles driven into the littoral bottom. It may be taken from any convenient fixed points of level, and all ultimately referred to half-tide as the datum for all earthquake observations as to level.

The sextant may be occasionally used to get the elevation of the crest of the passing wave, several observers making a simultaneous observation of an expected wave. The velocity of the wave may be got by noticing from a suitable position, by a seconds watch, the time of its transit inwards between two distant points having water between them whose depth is or may be known. Islands off the land are advantageous posts for this purpose. Where tide-gauges can be established they afford the best means of recording all the conditions of these waves when of a manageable height. The state of the tide at the time of their occurrence, and the general nature of the local establishment, with the in and off shore currents, should be ascertained.

The length of the wave (while entire) should be sought for by a similar method; a knowledge of its length and of the depth of water infers its height. There are two indirect methods by which the dimensions of the great sea-wave may be pretty accurately determined: First. The distance to which solid bodies before at rest are translated by the passage of the wave over them is about equal to its length or amplitude; so that when we can obtain evidence of the distance to which a large loose rock, for example, whose precise position was before known, has been carried, we approximate to one dimension of the wave. Secondly. The depth of water at the point where the wave is first observed to break, when capable of being accurately found, gives the height of the wave, which is here equal to the depth of the soundings. This breaking point and depth should always be anxiously tried for. Besides the dimensions of the wave, observations should be made, on the interval of time, after the great earth-wave, or shock, and before the great sea-wave comes in, reckoning from the *commencement* of the shock. When more than one great sea-wave comes in, the precise number of successive waves and the intervals in time of their recurrence should be noted; also, what are their relative dimensions; what changes are observable in the directions whence they arrive at the same point of coast, and what are the several in-coming directions at various points along a great stretch of coast, (the latter must be had usually from collected testimony;) what reflux from the beach before or after the coming in of the wave; after the wave has come in and broken, what oscillatory waves are pro-

duced, their character and dimensions; whether the level of the sur-
face of the sea is, in repose, the same before and after the subsidence
of the great sea-wave and its secondary or oscillatory waves; whether
any subsequent irregularity of tide occurs after the shock or great
sea-waves, or any permanent change of establishment should be ascer-
tained.

As accurate a section as possible of the form of the littoral bottom,
beach, offing, and out to deep water, should be got by soundings in
the line of the coming in of the wave, and laid down on paper.   It
should be noticed whether the great wave comes in of muddy or dis-
colored water, or clear and like the sea it traversed; and, where
possible, a cruise should be made out to sea in the direction whence
the waves came, to look for pumice, dead fish, volcanic ashes, or other
indications of the distant origin or centre of disturbance.   The cotidal
lines of the great waves should be laid down in direction upon a map
of the coast.

The secondary effects of the great sea-wave, most worthy of re-
mark, are the materials, if any, carried in from deep sea, such as
loose mineral matter, new animal or vegetable forms, or the substances
swept from off the land and sunk in the depths of the sea.   As the
range of transferring power of a great sea-wave (wave of translation) is
only equal to the wave itself, but little matter will be carried inland
from the sea bottom, unless where the depth is great close to shore.

If fish or testacea are thrown inland into fresh water, the effects on
them should be noticed.   Lastly, the effects of the passage of the
wave over the land and all that stands upon it are to be observed.
In recording the transporting power of the wave, (i. e., its absolute
transferring power, without reference to distance,) the size, form,
specific gravity, and lithological character of rocks or boulders moved,
the distance moved and height lifted are to be given; the base on
which moved, and if rock, the scratches or furrows produced; the
mode of motion, and if swept or rolled along, the obstacles overcome
in their progress.   Where gravel or loose materials are moved there
should be given an estimate of the mass moved, and to what distance;
the character, external and internal, of its deposition; the mutual
relations or sorting of its fine and coarse parts.   The effects on build-
ings variously exposed; on vertical and sloping sea-walls; on steep
faces of cliffs, and on the caverns excavated in them.   The denuding
effects of the wave in sweeping off sand, gravel, trees, animals, &c.
The disruption and lifting of masses and abrasion of stratified rocks,
especially of nearly level and nearly vertical beds.   Effects of vertical
sea-walls or cliffs in the reflection or extinction of the wave.

Specimens should be taken of the rock of which very remarkable
boulders or architectural fragments moved by the wave consist; of
any new or strange matters cast up, or gases or vapors evolved from
the sea, or ejected from fissures, cavities, wells, &c., on land; of min-
eralized or suddenly fouled water found in fissures or wells.   Of these,
where possible, immediate chemical qualitative examination should be
made.

Such specimens in particular should be brought home of the rocks

or other mineral masses through which the speed of transit of the earth-wave has has been carefully observed, as will enable the mean *modulus of elasticity of the mass* to be determined. Where this is rock, three specimens should be taken of maximum, minimum, and average hardness, density, and compactness, as representatives of the whole, noticing especially in stratified rock the depth from surface of ground and from top of the formation at which taken; each specimen to be of a size enabling a block to be sawn out of it of at least three feet in length by four inches square. Where convenient, this operation may be done on the spot. An iron wire stretched like a bowspring, with some sharp sand and water, makes an excellent stone saw; still better where continuous motion can be given by a band to the wire wheel and winch handle. Where the district is a deep detrital or alluvial one, the depth and characters of the loose materials should be carefully observed, and illustrative specimens, as far as possible, brought home. It is in the highest degree important that the degree of shatteryness or compactness of the rock formations, the nature, directions, closeness, or openness, and contiguity of the fissures be remarked, as these conditions of comparative discontinuity most materially affect the transit period of the shock in every formation.

Collateral conditions to be observed are: Barometer before, during, and after the earthquake; thermometer and rain guage; hygrometer and electrical state of the air during the phenomena; magnetometrical observations to be made where these are practicable; all unusual meteorological appearances to be noted, and all changes or perturbations of climate or season observable for a year before and after the shock are desirable to be ascertained. Also, whether epidemic or other diseases follow, and have a distinct connexion of cause and effect with the earthquake, as by change of season, failure of crop or food, injury to arterial drainage, the presence of fogs or exhalations, or like events.

The effects of the shock itself on man and the lower animals to be noticed. Nausea is undoubtedly a frequent effect upon human beings at the instant of shock; but the nature of its production is uninvestigated. Is it due to nervous perturbation, or to the movement, as in the case of sea-sickness? Some animals appear to predict the shock before men are conscious of its approach. Birds are often killed by being thrown off their roosts while asleep at night. Flat fish on the sea bottom are often killed by the direct blow of the steeply emergent wave. All such modes of death should be noted. Active volcanic phenomena occurring before, during, or after the earthquake, in adjacent or distant regions, will, of course, be recorded.

Records or trustworthy traditions are to be sought for in new or little explored volcanic countries, or those neighboring to them, as to the state of activity or repose of these vents for a long period prior to and during the earthquake; also as to their state before and during any previous earthquakes—all remarkable facts as to which should be collected. Where meteorological or tidal tables exist they should be transcribed for the times correlative to the above records. The opinions of old observers as to changes of climate or season; the occurrence

of pestilences, failure of crops, &c., in relation to earthquakes, while they must be received with caution, should not be disregarded.

Any changes of permanent level of sea and land that accompanied former earthquakes that are on record should be obtained, with their particulars; whether the same points have been affected in successive earthquakes and by successive upheavals; whether the same or different volcanoes were in action during successive earthquakes; and whether the area of disturbance in habitual earthquake regions seems to enlarge in successive shocks.—(Humboldt, "Cosmos.")

Upon maps of the country in which the shock was felt, coseismal and meizoseismal curves may be finally laid down, upon which also the cotidal lines of the great sea-waves on a long coast-line may be marked. Maps of fissures formed in relation to the coseismal lines, and generally sketches of all visible remarkable effects of the earthquake on natural or artificial objects, should be made. Photography affords precious facilities for preserving the appearances of shattered buildings and the relations or alterations of natural features, &c. The effects of earthquakes on the lives of men and animals; statistics of mortality; modes of entombment by the convulsion, as bearing on future organic remains; burying of objects of human art—are all worthy of notice.

It sometimes happens that a shock of earthquake is felt at sea at great distances from land, and over profound depths; a sudden blow is felt as if the ship had struck a rock.—(See "Comptes Rendus," vol. vi, pp. 302 and 512, 1853.)

The earth-wave coming from an origin probably in most cases nearly vertically beneath is here transferred to the ocean, through which it passes upwards as an elastic wave, with the same speed as the sound-wave through the sea. When such an event occurs in a smooth sea, and circumstances are favorable, we should look out for and note the direction of the passage almost immediately in form of a single, low swell, of the great sea-wave, which may be formed directly over the origin, at no very great distance off. Immediate attention should be given to the particulars of any objects that may have been displaced on board. Compasses are thrown out of the gimbals, shot dislodged from their seats round the hatchway coamings, or other places; a mast has even been unstepped. The relation observed between the extent of lateral and of vertical displacement will give some notion of the deviation of the line of shock from the vertical, and of its slope in azimuth. This found, a cruise about may be made in search of pumice, discoloration, or other indications upon the surface of the sea, &c., of the orgin under the sea bottom. Where the depth of water is great it is improbable that any indications of the convulsion below will reach the surface. Efforts, however, should be made to reach the bottom with the armed lead line and to obtain two lines of soundings at equal intervals for some miles, running both in latitude and longitude, and to bring up specimens of the bottom at each throw of the line. The origin may be found to be a newly-emerging volcano, an object always of great interest; the observation when in deep water

is capable of adding much to our knowledge of chemical and physical geology.

Perhaps no branch of terrestrial physics will so richly repay to the observer, who is so fortunate as to be able to reach the greater seats of volcanic and seismic action of our globe, the labor that will be necessary beforehand to enable him effectually to grasp his subject, as seismology; but observations undertaken without such preliminary knowledge will, for the most part, be valueless.

Besides the study of the several works already mentioned in the text, Lyell's "Geology," *passim,* should be studied, and a few of the best narratives of earthquakes perused. Such are Hamilton's and Dolomieu's "Accounts of the Great Calabrian Earthquake," (neglecting their theoretic views;) Humboldt's, Admiral FitzRoy's, and C. Darwin's Accounts of the South American, and Sir Stamford Raffles's Account of those of Java; with several others.

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METEOROLOGICAL INSTRUMENTS.

[Mr. Cassella, of London, has furnished us with a series of wood cuts to illustrate some late forms of instruments constructed by himself; and, as they may be interesting to meteorological observers, we have concluded to insert them in this Appendix to the Annual Report.—J. H]

Fig. 1.
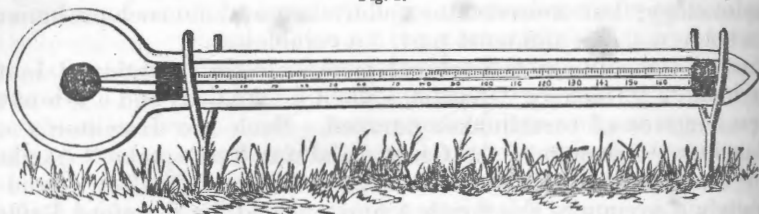
Fig. 1 represents a solar radiation thermometer, with blackened bulb, in a stout glass tube exhausted of air within one-tenth of an inch of the mercurial gauge, constructed agreeably to the suggestion of Sir John Herschel.

The instrument being thus protected from all external influence gives uniformity of readings for comparison of solar radiation, which surpasses those obtained by the naked bulb exposed to contact with the air.

Fig. 2.
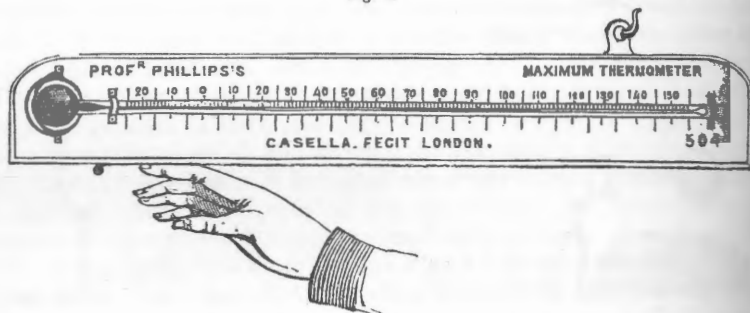
Fig. 2 represents a maximum thermometer constructed on the principle of Professor Phillips. The maximum point of temperature attained during the interval between two observations is indicated by a separation in the mercurial column. The end of a portion of the extremity of the column is left at the point of maximum, while the contraction takes place in the remainder of the column, as shown in the figure. To insure this separation, the bore of the tube is exceedingly fine, and a minute portion of air is left by the maker at the point where the separation takes place. This instrument is suspended horizontally on a hook at one end and on a pin at the other. In order to bring back the index to its proper place after the observation has been made, the pin is removed, the instrument is brought to a perpendicular position with the bulb downwards, when the detached mercury descends into near contact with the remaining

portion of the column. The instrument is again brought back to its horizontal position and the pin restored to its place.

Thermometers constructed after this plan were first exhibited by Professor Phillips, accompanied by a description, at the Oxford meeting of the British Association for the advancement of science, in 1832. The principle of the instrument is, as we have stated, the employment of a certain portion of the column of mercury detached as a marker. The length of this is capable of a great range of adaptation to suit the objects of experiment; the instrument is independent of change by time or chemical action, and as delicate in operation as the best ordinary thermometer. Mr. Phillips constructed a number twenty-five years ago, some of which remain in an excellent state to the present time. The length of the marker may be varied at pleasure by means of a second hollow ball blown at the extremity opposite the ball containing the mercury. The longer this marker is left the more moveable it becomes. With *a certain small length* depending on the diameter of the tube it will remain, without moving, in any position, and requires strong shaking to change its place. Among the samples presented to the Association was one planned by Professor Phillips for special researches on limited sources, or areas of heat, with small bulb, fine bore, and *short detached marking column.* Thus constructed, the thermometer may be used in any position—vertical, inclined, or horizontal—and the short detached marking column will retain its place with such firmness that the instrument may be carried to a distance, or even agitated, without disturbing the registration.

Fig. 3.
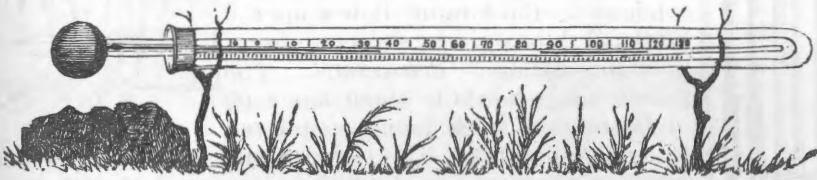
Fig. 3 represents an instrument of the same kind with black bulb for solar radiation.

Fig. 4.
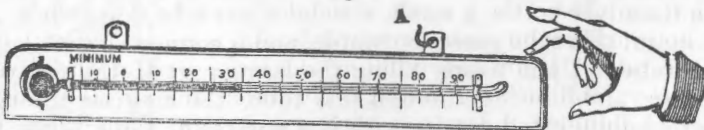
Fig. 4 represents the ordinary minimum thermometer in which the index is a small piece of enamel.

Fig. 5.
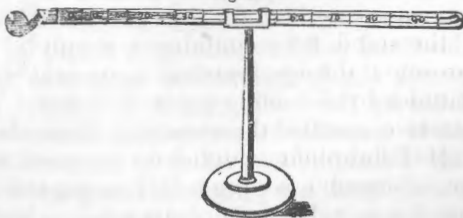
Fig. 5 represents a convenient form for mounting a thermometer for determining the temperature of grass due to radiation.

Fig. 6.

Fig. 7.
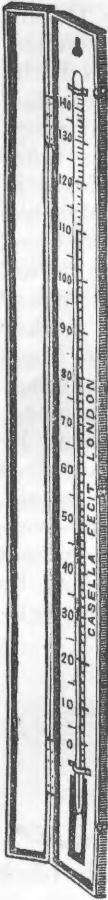
Fig. 6 represents one of a series of standard thermometers, extra sensitive, about 20 inches in length, each degree three-fourths of an inch, divided into tenths or twentieths.

Sensitive thermometers for extremely low temperatures are also constructed of the same pattern, thirty-five inches long, with a range from 60° below to 80° above zero, filled with pure alcohol of the specific gravity of 720.

Fig. 7 represents Regnault's condensing dew-point hygrometer.

This instrument consists essentially of two sensitive thermometers, as shown in the figure, the lower exposed to the action of the atmosphere, the upper to the influence of a current of air passing through ether contained in a well-polished silver bottle, from the mouth of which the stem of the thermometer projects. This thermometer marks the exact temperature at which the aqueous vapor at the time in the atmosphere is condensed in the form of dew upon the bottle, and thus gives by direct observation the existing "*dew-point.*" The polished silver bottle is about one inch in diameter, the neck being contracted to about five-eighths. The thermometer inserted into this bottle is a sensitive one, divided on its stem to *half degrees,* the stem passing through an ivory stopper fitted with a cork which renders the bottle air-tight at the neck. On one side, and within the silver bottle, a small, slender silver tube descends to nearly the bottom; this tube passes outwards, and is connected with an India-rubber tube. Upon nearly filling the large part of the silver bottle with *ether,* and blowing through this tube, the air rises through the ether in bubbles and carries with it a portion of the ether in vapor. This evaporation of the ether causes such a degree of cold that the surface of the silver bottle is so reduced in temperature as to cause a precipitation of dew. The supporting stem of the instrument being *hollow* a ready means is provided for the egress of the air. The bottle at the foot of the stand is for containing a supply of ether.
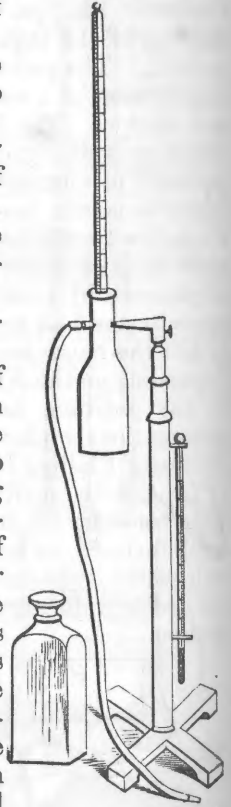
Fig. 8 represents the hygrometrical apparatus or instrument for measuring altitudes by the boiling point of water. It consists first, of a strong sensitive enamelled thermometer, the scale of which ranges from 180° to 214° Fahrenheit, divided on the stem so as to show the tenth of a degree. Second, a copper boiler supported on a small tripod and surmounted by a telescopic draw-tube, which is again sur-

Fig. 8.
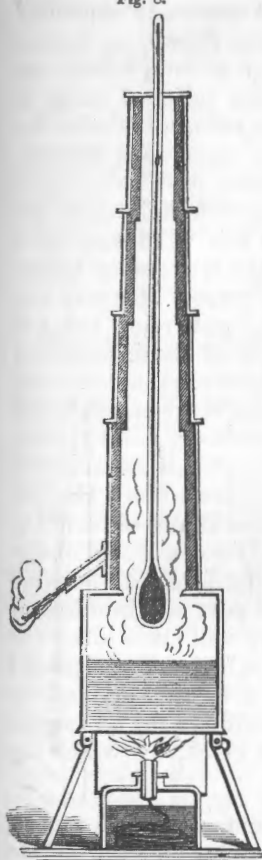
rounded by a second tube or steam jacket. The inner tube has perforations near the top which allow the steam readily to fill the intermediate space and freely to escape by a side tube, as shown in the figure. The thermometer is supported, at about one inch above the surface of the water in the boiler, by means of a cork or India-rubber washer on the upper part of the stem, and can be immersed in the steam to any required amount by sliding the telescope tube to any required height. Distilled water is used in this instrument, which is made to boil by means of a spirit lamp. The whole is packed in a leathern sling case, shown in Fig. 9.

Fig. 10.

Fig. 9.
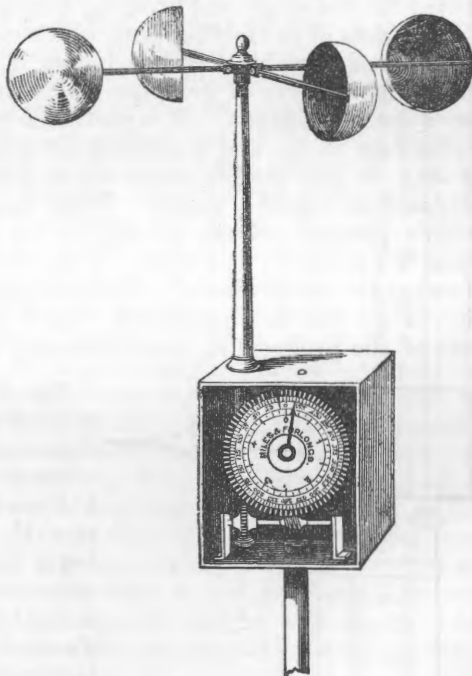
Fig. 10 represents a portable anemometer for registering the velocity of the wind in miles and furlongs.

This instrument is a modification of the anemometer devised by Dr. Robinson, of Armagh, which consists essentially of four hemispherical cups, having their diametrical planes exposed to a passing current of air; they are carried by four folding horizontal arms attached to a vertical shaft or axis, which is caused to rotate by the motion of the wind. Dr. Robinson found that the cups, and consequently the axis to which they are attached, revolve with one-third of the wind's

velocity. A simple arrangement of wheels and screws is appended to the instrument, which, by means of two indices, shows, on inspection, the space traversed by the wind. The outer or front wheel, one revolution of which is equal to the transit of five miles of wind, is furnished with two graduated circles, the interior being divided to the eighth part of a mile, so that each division is equal to a furlong, while the exterior is divided into one hundred parts, each being equal to five miles. The stationary index at the top of the dial marks the number of miles (under five) and furlongs that the wind may have traversed, in addition to the miles shown by the traversing index, which revolves with the dial and indicates the transit of every five miles. The graduation is to five hundred. The traversing index is furnished with a milled-headed screw at the back of the instrument, which is employed for bringing its extremity to the zero point when the instrument is set, which consists in merely turning it by means of the milled-headed screw and bringing the end of the index to point to zero.

By means of the folding arms which carry the cups this anemometer is rendered portable. When in use it may be screwed on a shaft or the ordinary piece of gas-pipe which accompanies it and elevated to any desirable altitude. It is particularly adapted for occasional observations on shore, and is suitable for measuring the force of the wind at sea. It may readily be set up on the highest part of a building or elevated on board a vessel. When inspected it will show alike the wind's present velocity as well as the rate at which it was passed since it was set or last read. This instrument may also be used for showing the ventilation of public buildings or dwellings, by an inspection of its dial in combination with a watch or clock, by which the rate of the progress of ventilation may be seen.

Fig. 11.
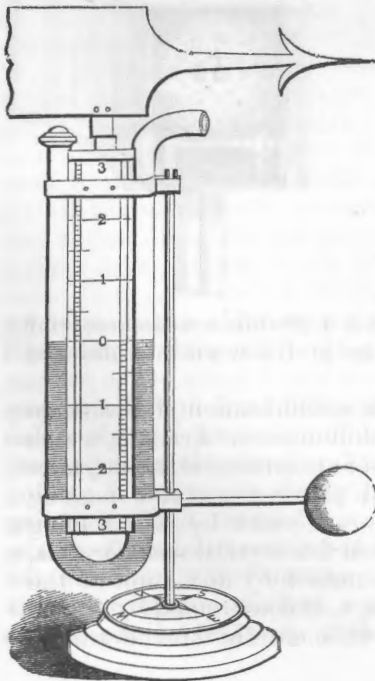
Fig. 11 represents a convenient form of Lind's anemometer for showing the direction and force of the wind. This consists essentially of a glass tube of half an inch bore, bent into the form of a U, as shown in the figure, the lower half of which is filled with mercury; the upper end of one of the legs is bent horizontally, and when this is directed toward the wind the mercury is driven down by the pressure in one leg and caused to rise in the other, the difference in level gives the pressure of the wind in inches of mercury from which the velocity may be calculated. For observing very high winds the straight leg may be closed at the top, in which case the pressure on the open end will be indicated by the condensation of the air in the other leg, combined with the difference of level of the mercury in the two legs.

ON FILLING BAROMETER TUBES.

[Having been frequently called upon by our correspondents to give information relative to filling barometer tubes, we requested Mr. James Green, of New York, and Mr. W. Wurdemann, of Washington, to furnish us with an account of the methods employed by them. The following are their answers to our request, with additional information from the Transactions of the Royal Society.—J. H.]

I.—BY JAMES GREEN, OF NEW YORK.

One of the greatest difficulties with the inexperienced is to get the tube itself clean and free of moisture. If the tube is foul, the common way is to clean it with a covered copper wire, wrapped with additional cotton at the end to fit the tube, and moistened with alcohol and whiting at first, afterward with dry cotton. If the tube can be heated and air blown in dry, so much the better.

The mercury and tube should be heated as much as will be allowable to handle them, to keep all the water in a state of vapor. The mercury is filtered into the tube in a long paper funnel, in a fine stream, until within a quarter of an inch of the top. The tube will now be found covered with small air bubbles. Stop the end of the tube with the finger, and run a large air bubble up and down the tube. This will collect the small ones together. Provided the tube be clean and dry, and mercury pure, a pretty good result is obtained.

To boil the mercury in the tube, fill within three inches of the top. Then, with a clear charcoal fire or long spirit lamp, warm the whole tube as much as you can without inconvenience. The tube being held by a cloth, (with woollen gloves on hands is well,) then commence at the top or open end and hold the tube over the fire until the mercury boils, moving the tube a little in all directions all the time to equalize the distribution of the heat from the fire. Continue the boiling downwards until you reach the end, and then return boiling up to the top again. Some begin at the closed end, (for economy of risk and labor,) and boil up only. This may answer the purpose, but not so well as the other, particularly if the tube is not perfectly dry and clean. The part of the tube unoccupied will be well-prepared by the boiling mercury bubbling up; so that to complete the filling, filter hot mercury to the top.

The more perfect methods of boiling are impracticable out of the workshop and hands of the glass-blower.

One of the best tests for the purity of the mercury is, that after once filtering in a long paper funnel to get it clean, in filtering again slowly no lines or marks are left on the paper by the receding surface, and in motion no strings or tails are made, but the mercury will be rounded at its edges.

The best method ordinarily practicable for purifying mercury is to put it in a large bottle with some very dilute nitric acid, and shake it

frequently.   It should then be left for some days, and shaken occasionally; then well washed with pure water and dried.   I distil first, and then wash with acid, and this will take out the metals likely to be found in it.

II.—BY W. WURDEMANN, OF WASHINGTON.

In compliance with your request, as contained in your note received this day, I will give some notes in regard to my usual method of filling barometers.

First, let me premise that I have so far filled only such as have a straight tube, without bend or contraction, and to such alone the method below explained is applicable; nor ought the tubes to be of a less bore than $\frac{3}{16}$ of an inch.

Besides the requisites stated, those of a clean tube and perfectly pure mercury are equally indispensably necessary with this method as well as any other, where a perfect instrument is desired.   The purification of mercury is best accomplished by means of perchloride of iron, with which it is shaken in a diluted state; then carefully washed with pure water, and again freed from moisture by heating. The glass tube must have its open end ground straight and smooth, so that it can be closed air-tight with the finger, or better, with hard caoutchouc, as the former is liable to introduce moisture or grease. Warm well both mercury and glass tube, and fill in through a clean paper funnel with a very small hole (about $\frac{1}{30}$ of an inch) below, to within about one-fourth of an inch of the top.   Shut up the end and turn the tube horizontal, when the mercury left will form a bubble that can be made to run from one end to the other by change of inclination, which will gather all the small air bubbles visible that adhered to the inside of the glass tube during filling.   Now let that bubble, which has grown somewhat larger, pass to the open end. Fill up this time with mercury entirely, and shut up tightly.   Then reverse tube over a basin, when, by slightly relieving the pressure against the end, the weight of the column of mercury will force some out, forming a vacuum above, which ought not to exceed one-half an inch.   Closing up again tightly, let this vacuum bubble traverse the length of the tube on the several sides, when it will absorb those minute portions of air, now greatly expanded from removed atmospheric pressure, that were not drawn at the first gathering.

The perfect freedom from air is easily recognized by the sharp concussion with which the column beats against the sealed end, when, with a large vacuum bubble, the horizontally held tube is slightly moved.

ACCOUNT OF THE CONSTRUCTION OF A STANDARD BAROMETER, AND DESCRIPTION OF THE APPARATUS AND PROCESSES EMPLOYED IN THE VERIFICATION OF BAROMETERS AT THE KEW OBSERVATORY.

BY JOHN WELSH.

Communicated to the Royal Society by J. P. Gassiot, esq.

I.—STANDARD BAROMETER.

In the course of the years 1853–'54 several attempts were made, under the superintendence of the Kew committee, to prepare, by the usual method of boiling, a barometer tube of large dimensions. Mr. Negretti, to whom was entrusted the preparation of the tube, succeeded repeatedly in boiling, apparently satisfactorily, tubes of fully one inch internal diameter. Many of these, however, broke spontaneously before they could be mounted, some of them within a few hours and others after an interval of several days. Two or three tubes were ultimately erected, but their condition was not satisfactory. The adhesion of the mercury to the glass was so great, that in a falling barometer the convexity of the top of the column was destroyed, and the surface of the mercury assumed even a concave form. After a few days rings of dirt or other impurity were formed on the glass near the top of the column, which soon increased to such a degree as entirely to interfere with the observation. The mercury employed in filling the tubes had been previously treated for some weeks with dilute nitric acid, and afterwards kept in bottles under strong sulphuric acid being well washed with water and dried by repeated filtering before use. Dr. W. A. Miller examined specimens of the mercury, and could detect no impurity in it.

Suspecting that some injurious effect might have been produced upon the mercury or upon the glass by the great heat to which the tube was necessarily exposed in boiling so large a mass of mercury, it occurred to me that the difficulty might be removed by another method of filling the tube, which I shall now describe:

The tube was, in the first place, prepared as follows: To its upper end was attached a capillary tube bent thrice at right angles, having its bore much contracted at the middle point of its length, with a small bulb blown at another part of its length, being finally drawn out to a fine point and there hermetically sealed. To the lower end of the large tube was attached ten inches of a smaller tube, having a bore of three-tenths of an inch, and to that again was added about six inches of capillary tube. A bulb of three-fourths of an inch was blown at the end of the smaller tube, which, at its junction with the

larger tube, was finally bent into a syphon. The end of the lower capillary tube was now connected with a good air pump, and the air very slowly extracted at the same time that the whole tube was strongly heated by passing a large spirit lamp along it. When the air had been as well as possible extracted, and whilst the air pump was still in action and the heat still applied, the lower capillary tube was sealed by a blow-pipe flame. When the tube had cooled, it was placed at a small inclination with the end of the upper capillary tube in a vessel containing mercury which had been previously boiled. The point of this tube was broken off under the mercury, which then rose in the tube by atmospheric pressure. The mercury continued to rise until the bulb at the other end was more than half filled, the remaining space being occupied by the air which the pump had failed to extract. It was estimated from the amount of space thus left unoccupied by the mercury that the pressure of the residual air in the tube when cold must have been less than five hundredths of an inch. The basin of mercury was then withdrawn from beneath, leaving the point of the capillary tube exposed, the bore of which remaining quite filled with mercury. The blow-pipe was then applied to the point, and the opening sealed. When the glass had cooled, the large tube was placed erect, the mercury separating at the contracted part of the capillary tube, leaving the remainder filled, or very nearly so, and the part between the point of contraction and the large tube a vacuum. The upper capillary tube was now sealed at about the middle of the vacuum, and the remaining portion removed. Finally the syphon tube at the lower end of the large tube was broken under mercury, leaving about an inch of the syphon remaining.

The earlier tubes filled by this process were not satisfactory, there being, as in those previously prepared by boiling, a considerable adhesion of the mercury to the glass, with the formation, after a few days, of rings of dirt; so similar, indeed, was the appearance of these tubes to that of the boiled tubes, that I was led to believe that the evil in both cases was due to the same cause. Being satisfied that there was no impurity in the mercury, which, besides having been cleaned with nitric acid, had before these last experiments been redistilled, and suspecting that the evil might have been owing to imperfect cleaning of the tubes, which had only been *wiped* out by the glassblower in the usual way, I had fresh tubes made under my own inspection, and sealed at the glass-works immediately after being drawn. Great care was also taken by the glass-blower to prevent the entrance of moisture during the subsequent operations with the blow-pipe. These tubes, however, still showed the same imperfection, though in a less degree. About this time I had the advantage of consulting Mr. John Adie, of Edinburg, who informed me that he had also experienced the same inconvenience, and that he had removed it by thoroughly cleaning the tubes by sponging with whiting and spirits of wine. Following his directions, 1 had the satisfaction of finding the tubes when filled almost wholly free from the imperfections mentioned. A tube of 1.1 inch internal diameter, prepared in July, 1855, by the process above described, is at this time in as good condition as when first

erected. The top of the column presents a good convexity in all states of the barometer, with only a very slight trace of dirt. No appearance of air-specks can be detected, except a few very minute ones near the lower end of the tube, which have existed since the commencement, and were produced by the temporary entanglement of a small air-bubble at the shoulder-bent part of the syphon tube in the operation of filling. These specks have not increased in number nor shown any tendency to rise. A portion of the syphon being retained at the lower end of the tube, it is highly improbable that any air can now enter, the mouth of the syphon being cut off from communication with the external air by the mercury in the cistern. The tube extends to about nine inches above the mean height of the mercury.

The tube is supported over a glass cistern in a strong brass frame secured by brackets to the wall of the old mural quadrant of the observatory, the height of the mercury being measured by a catheto-meter * fixed to the same wall at a distance of five feet. A conical point, at the lower end of a short rod of steel, is adjusted by a screw to the surface of the mercury in the cistern. At the upper end of the steel rod, and above the level of the glass cistern, is a fine mark, whose distance from the conical point has been found by comparison with the Kew standard scale to be 3.515 inches. When an observation is made, the lower point is adjusted to exact contact with the mercury in the cistern; the telescope of the cathetometer is then levelled, and its horizontal wire made to bisect the mark on the upper end of the steel rod, the scale reading of the cathetometer being noted. The telescope is then raised, again levelled, and the wire made a tangent to the surface of the mercury in the tube, the cathetometer scale reading being again observed. The difference between the two readings of the cathetometer scale added to the length of the steel rod is the height of the column of mercury. Besides the rod terminating in the conical point, a second adjusting rod is provided, whose lower extremity is a straight edge. No difference could be detected between the results from the two methods of adjustment. In order to avoid the inconvenience of light being reflected into the telescope from the surface of the mercury in the tube, a movable screen is provided, the upper part of which is black and the lower part oiled paper, which is so adjusted as to shut off all light which comes from a higher level than the top of the mercurial column. The surface of the mercury thus presents in the telescope a well-defined dark outline. A window behind the barometer gives a good illumination to the paper screen; a lamp being required at night. A thermometer whose bulb is within the mercury of the cistern gives its temperature, and the scale of the cathetometer being of brass, the usual tables can be employed for the temperature correction, the difference in the expansion of steel and brass being insignificant for the length of the short adjusting rod. The variations of the temperature of the room

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* A small telescope, with a horizontal wire in the focus of the eye-piece, sliding on a vertical graduated measuring rod.

are not rapid, so that no sensible error arises from assuming the temperature of the cathetometer to be the same as that of the mercury. The cistern of the standard barometer is 33.9 feet above the mean level of the sea, being 9.1 feet above the ordnance bench-mark on the northeast corner of the observatory, whose elevation is stated by Lieutenant Colonel James to be 24.83 feet.

Observations of this barometer being too troublesome when an extensive series is required, a standard by Newman, (No. 34,) having a tube of 0.55 inch, which has been recently compared with the great Kew standard, is employed for ordinary use, its index correction (which, inclusive of capillary action, is $= + 0.003$ inch) being first applied to the observed readings.

Comparisons, by means of two portable barometers by Adie, London, were made during last summer between the Kew standard and that of the observatory at Paris. The result of these comparison was, that the Kew standard reads higher than the Paris standard by 0.001 inch, no correction being applied to either instrument on account of capillary action.

II.—VERIFICATION OF BAROMETERS.

In the best barometers of the present day a provision is made for adjusting the surface of the mercury in the cistern to the zero of the scale at each observation. Supposing the tube to be in good order, which is easily ascertained by mere inspection, the only source of error in such instruments is to be looked for in the scale. The graduation of the scales of all carefully made barometers is performed by means of a dividing engine, and it is not likely to be inaccurate to any sensible extent within the ordinary range of the mercury. If, however, the barometer is intended to be used at considerable elevations, or if it should otherwise be considered desirable to examine the graduation, the error of the divisions can be readily obtained by measurement with the cathetometer. It frequently happens, however, that the point to which the level of the mercury is adjusted is not the true zero of the scale. The error arising from this source is, of course, constant for all heights of the barometer. As the capillary action of the tube is also supposed to be constant for the same barometer, and as it is seldom possible to determine its true amount, it is better to consider it in connexion with the zero error. This is the more advisable, since a reference to the zero point in a complete barometer to any point of the scale is rendered difficult and uncertain by the circumstance that it can only be viewed through the glass of the cistern, which, from its irregularity, may considerably affect its apparent position. It is therefore the practice to suspend the barometer to be examined beside the standard, to make a sufficient number of simultaneous observations of the two instruments, and to adopt the mean difference of their indications as a single constant correction for the combined effects of zero error and capillary action.

In many portable barometers, and in nearly all marine barometer there is no means of adjusting the mercury to a constant level. It

becomes therefore necessary to determine the correction for "capacity," or the variation in the zero point corresponding to different heights of the column of mercury. The amount of this correction may be determined during the construction of the instrument; or, by reducing in the required proportion the lengths of the divisions, it may be allowed for in graduating the scale, as has been done in the marine barometers made under the supervision of the Kew committee by Mr. P. Adie, of London. In order to test the accuracy of this correction, it is necessary to compare the barometer at two considerable different pressures with a standard instrument, that is, with one in which the mercury is adjustable at each observation to a constant zero point. This is done by placing the barometer and a standard within a receiver provided with the means of altering at pleasure the pressure of the inclosed air.

The receiver is of cast iron, its horizontal section being rectangular. It is 39 inches high, 12 inches by 6½ at its lower end, and tapering to 10 inches by 4½ at the upper end; there being room for three marine barometers besides the standard. Windows of strong plate glass, each 11½ inches high and 9½ inches wide, let into both sides of the receiver, admit of the barometers being observed by a cathetometer. Smaller windows below, each three inches square, show the cistern of the standard barometer, the mercury in which is adjusted to a constant level by a screw passing through a stuffing-box in the base of the receiver. The barometers to be verified are suspended by a gimbal arrangement from the upper end of the receiver, a massive lid closing the opening at the top, by which they are introduced. An opening in the base, furnished with a stop-cock, is connected by a flexible tube with a pump which regulates the pressure of the inclosed air. The pump consists of a single barrel and piston. There being openings at both ends of the barrel, the valves are so arranged that when the flexible tube is attached to the lower opening, air is extracted from the receiver, and when with the upper air is forced in. The receiver is supported by an iron bracket, securely fixed to the quadrant wall, about 10 feet from the standard barometer. The cathetometer being between the receiver and standard barometer, can be used at pleasure for either. The adjustable barometer used in the receiver for comparison with the marine barometers has a tube 0.35 in diameter; there being a contraction in the tube of the same kind and to about the same degree as in the ordinary marine barometers made by Mr. Adie. This apparatus for the verification of marine barometers has (with the exception of the adjustable barometer, which is by Mr. Adie) been entirely constructed in the observatory by Mr. Robert Beckley, the mechanical assistant, who has executed the work in a most satisfactory manner, and who has shown much ingenuity in arranging the mechanical details so as to afford the utmost exactness in observation and convenience in manipulation.

The mode of observation is the following: supposing air to have been extracted from the receiver until the barometers stand at about 27 inches, sufficient time having elapsed to allow the mercury to come

to a state of rest, and the zero of the standard having been adjusted, the height of the mercury in each of the barometers is observed by the cathetometer. Air is then admitted till the mercury stands at about 31 inches, when the same operation is repeated. The length of the graduated scale of the barometer under comparison is then measured by the cathetometer. If A a be the cathetometer readings at the higher pressure of the standard and marine barometers, respectively, B b those for the higher pressure, and if L be the measured length of one inch of the scale of the marine barometer, then the correction for capacity for one inch $= L - \dfrac{a - b}{A - B}$. In order to avoid the error which might otherwise arise from the different capillary actions of the standard tube and that of the marine barometer, it is the practice to make these comparisons only in the forenoon, when the temperature of the room, and consequently the pressure of the air within the receiver, is slowly increasing.

Besides the determination of the capacity correction, a series of simultaneous observations are made of the marine barometer and the standard, "Newman 34," for the purpose of obtaining the zero error. From twenty to thirty comparisons are usually made, care being taken that there shall be, as nearly as possible, an equal number of observations with the barometer rising and falling; this being necessary in order to eliminate the retardation produced in the movements of the mercury by the contraction of the tube combined with the capillary action. The final corrections at different heights of the mercury are thus deduced from the data now obtained. Let H be the height (corrected for zero error) of Newman 34; h the corresponding height of the marine barometer; T the temperature of Newman 34; and t that of the marine barometer; K being the "capacity" correction; the correction corresponding to any height h_0 of the marine barometer is —

$$H - h + K \left(h_0 - h \right) + \left(t - T \right) \times 0.0027.$$

Each barometer, when it leaves the observatory, is accompanied by a statement of its corrections, of which the following is a specimen:

Corrections to the scale readings of marine barometer, B. T., No. 231, by Adie, London.

Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
At 27.5	At 28.0	At 28.5	At 29.0	At 29.5	At 30.0	At 30.5	At 31.0
+ 0.001	0.000	− 0.001	− 0.001	− 0.002	− 0.003	− 0.004	− 0.005

When the sign of correction is +, the quantity is to be added to the observed reading; and when —, to be subtracted from it. The corrections given above include those for index error, capacity, and capillarity.

III.—CATHETOMETER.

The cathetometer hitherto employed was made by Mr. Oertling, of London, on the plan of that used in the experiments of M. Regnault.

It was originally mounted on an independent support; but this was found to be too unsteady for exact observation. It was accordingly removed from its support and mounted between brackets attached to the quadrant wall. The scale of this instrument has been compared with the Kew standard scale, both in the horizontal and vertical positions; in the former by observation of both scales by fixed micrometer microscopes, and in the latter position in measuring by the cathetometer the divisions of the standard scale, placed vertically at a distance of five feet. In the horizontal position there appeared to be no appreciable error in the graduation of the cathetometer; but when vertical, its scale was found to be somewhat too long, the measurement of a length of 30 inches requiring a correction of + 0.003 inch. Besides this discrepancy, which is probably due to irregular flexure of the bar and to imperfect fitting of the sliding frame which carries the telescope and level, the manipulation of the instrument is exceedingly inconvenient and troublesome, and requires much care and patience. It is believed, however, that when the requisite care and time are bestowed, the measurements, after allowing for the correction mentioned, are accurate.

A new cathetometer is at present being constructed by Mr. Beckley, at the observatory, which promises greater accuracy and convenience. This instrument is very nearly completed, and will be described in a subsequent communication.

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