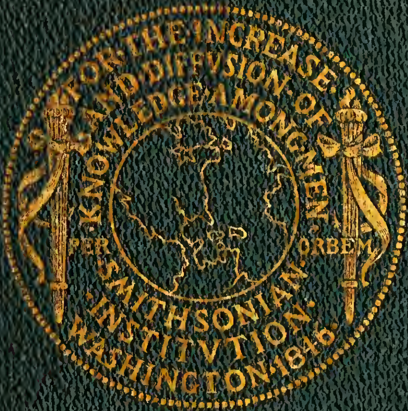
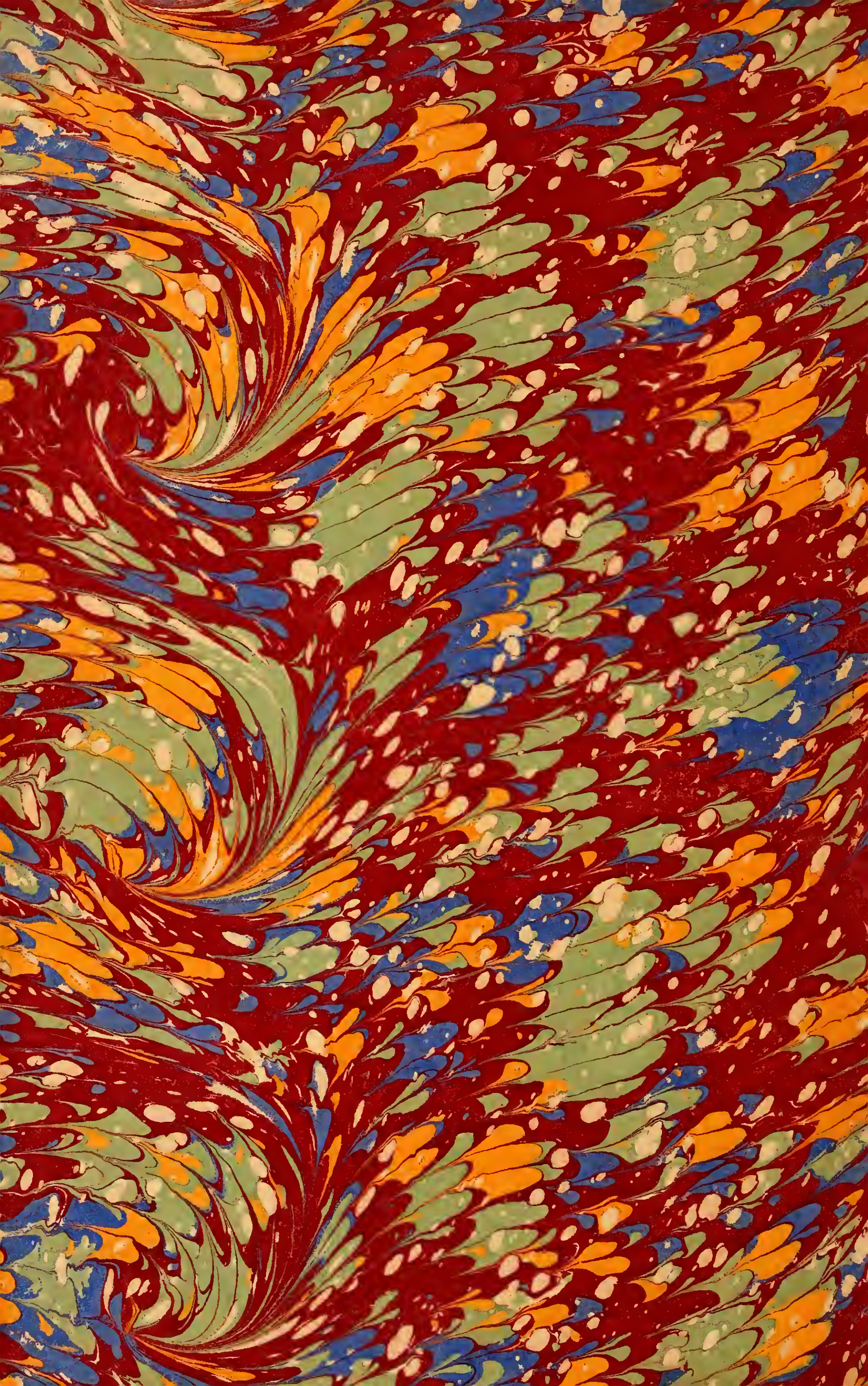

FOR THE INCREASE AND DIFFUSION OF KNOWLEDGE AMONG MEN
PER ORBEM
SMITHSONIAN INSTITUTION
WASHINGTON 1846



ANNUAL REPORT

OF

THE BOARD OF REGENTS

OF THE

SMITHSONIAN INSTITUTION,

SHOWING THE

OPERATIONS, EXPENDITURES, AND CONDITION OF
THE INSTITUTION FOR THE YEAR 1862.

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1863.

IN THE SENATE OF THE UNITED STATES, *February* 28, 1863.

Resolved, That five thousand additional copies of the Report of the Smithsonian Institution for 1862 be printed—two thousand for the use of the Smithsonian Institution, and three thousand for the use of the Senate; *Provided*, That the aggregate number of pages contained in said report shall not exceed four hundred and fifty, without wood-cuts or plates, except those furnished by the Institution; and that the Superintendent of the Public Printing be authorized, if consistent with the public service, to allow the Smithsonian Institution to stereotype the report at its own expense, or to otherwise print at its own expense such additional copies as may be desired from the type set in the Government Printing Establishment.

LETTER

OF THE

SECRETARY OF THE SMITHSONIAN INSTITUTION,

TRANSMITTING

ANNUAL REPORT OF THE BOARD OF REGENTS.

Smithsonian Institution,
Washington, February 19, 1863.

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 1862.

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

JOSEPH HENRY,
Secretary Smithsonian Institution.

Hon. Hannibal Hamlin,
Vice President of the United States and President of the Senate.

ANNUAL REPORT OF THE BOARD OF REGENTS

OF THE

SMITHSONIAN INSTITUTION,

SHOWING

THE OPERATIONS, EXPENDITURES, AND CONDITION OF THE INSTITUTION UP TO JANUARY, 1863, AND THE PROCEEDINGS OF THE BOARD UP TO FEBRUARY, 1863.

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 1862.
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 1862.
3. Proceedings of the Board of Regents up to February 4, 1863.
4. Appendix.

Respectfully submitted.

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

OFFICERS OF THE SMITHSONIAN INSTITUTION.

ABRAHAM LINCOLN, *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.

A. D. BACHE,
JOSEPH G. TOTTEN, } Executive Committee.
R. WALLACH.

REGENTS OF THE INSTITUTION.

H. HAMLIN, Vice-President of the United States.
ROGER B. TANEY, Chief Justice of the United States.
R. WALLACH, Mayor of the City of Washington.
W. P. FESSENDEN, member of the Senate of the United States.
L. TRUMBULL, member of the Senate of the United States.
GARRETT DAVIS, member of the Senate of the United States.
S. COLFAX, member of the House of Representatives.
E. McPHERSON, member of the House of Representatives.
S. S. COX, member of the House of Representatives.
W. B. ASTOR, citizen of New York.
W. L. DAYTON, citizen of New Jersey.
T. D. WOOLSEY, citizen of Connecticut.
ALEXANDER D. BACHE, citizen of Washington.
JOSEPH G. TOTTEN, citizen of Washington.
LOUIS AGASSIZ, citizen of Massachusetts.

MEMBERS EX OFFICIO OF THE INSTITUTION.

ABRAHAM LINCOLN, President of the United States.
HANNIBAL HAMLIN, Vice-President of the United States.
W. H. SEWARD, Secretary of State.
S. P. CHASE, Secretary of the Treasury.
E. M. STANTON, Secretary of War.
G. WELLES, Secretary of the Navy.
M. BLAIR, Postmaster General.
E. BATES, Attorney General.
ROGER B. TANEY, Chief Justice of the United States.
D. P. HOLLOWAY, Commissioner of Patents.
RICHARD WALLACH, Mayor of the City of Washington.

HONORARY MEMBERS.

BENJAMIN SILLIMAN, of Connecticut.
A. B. LONGSTREET, of Mississippi.
CALEB B. SMITH, Secretary of the Interior, (*ex officio.*)

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 diffusion 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, 1st, to increase, and 2d, 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 increased, 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 extensively 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 provisionally; 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 to be 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 be 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 art.

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, first, of a complete collection of the transactions and proceedings of all the learned societies in the world; second, 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.

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

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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

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.

To the Board of Regents:

Gentlemen: I have the honor again, at the commencement of your annual session, to present the report for another year of the operations of the Institution intrusted by the General Government of the United States to your special care.

So much public attention has been absorbed during the last year by the exciting events of the war that we might at first suppose that little or no thought could be bestowed upon purely scientific subjects, such as fall within the province of this Institution to cultivate, or indeed upon any kind of knowledge which has not an immediate bearing on the special requirements of the times. But even in the most sanguinary and gigantic warfare the responsibility for the important plans which are to determine the result of the conflict devolves upon the few, and leaves the many to fall into a condition of comparative mental inactivity.

As a relief from the tedium of this condition, or a prevention from falling into it, a large number of subordinate officers, and even privates, of the army have devoted themselves to pursuits connected with natural history, or to the solution of problems of a theoretical or practical character.

Although the immediate object of war is the destruction of life and property, yet a state of modern warfare is not a condition of evil unmingled with good. Independent of the political results which may flow from it, scientific truths are frequently developed during its existence of much theoretical as well as of practical importance. The art of destroying life, as well as that of preserving it, calls for the application of scientific principles, and the institution of scientific experiments on a scale of magnitude which would never be attempted in time of peace. New investigations as to the strength of materials, the laws of projectiles, the resistance of fluids, the applications of electricity, light, heat, and chemical action, as well as of aerostation, are all required.

The collection of immense armies of individuals of different ages and nations affords the means of obtaining data of much interest to the ethnologist, while the facts which are gathered from the unusual experience of the battle-field and hospital afford materials for the advance of physiology, surgery, and medicine, which a century of ordinary observation would fail to furnish.

In illustration of what has been done in the line we have mentioned, I would refer to the extended labors of the Sanitary Commission and of the department under the direction of the Surgeon General. The one, besides aiding in the improvement of the health and comfort of the soldiers, has collected a large number of interesting facts relative to the moral and economical condition of the army; while the other, in addition to its immense labors in the care of the sick and the wounded, has recorded the statistics of every part of its varied operations, and formed a collection of illustrations of surgical anatomy which is perhaps unrivalled in any part of the world.

In reference to all the inquiries to which I have alluded, the Smithsonian Institution has been called upon for aid and counsel, and has continually rendered active co-operation and assistance. Its labors, however, in this line, as well as in several other branches of its ordinary operations, are not attended with results which can be given to the world through its publications.

During the continuance of the war we must expect to find that more attention is given to the collection of facts than to the deduction from them of general principles ; the latter must be deferred to a period of more tranquillity, when the mind is in a better condition for continued application to the development of a single idea; consequently the number of papers which have been presented to the Institution since the date of the last report is less than that of any previous year.

The meteorological system which had been established, and was in successful operation for several years before the commencement of the war, has been much deranged, few records of observations having been received from the middle States and none from the southern; still, as I have before intimated, the labors of the Institution have been industriously pursued in lines more in accordance with the peculiar condition of the country. A large portion of all the time of the Secretary has been devoted to inquiries referred to the Institution by the different departments of government. The work of preparing the duplicate specimens of the Institution for distribution has been continued. The library has been thoroughly overhauled, partially rearranged, and a new catalogue of the transactions and proceedings of learned societies prepared for publication. Extensive repairs and improvements have been made in the building, by which several rooms, previously occupied by tanks for receiving rain water, now rendered useless by the introduction of Potomac water, have been reclaimed for other uses.

The efficient income of the Institution has been very essentially impaired during the past year. 1st. By the increased price of all the materials of printing and other articles used in the operations of the establishment. 2d. On account of the high premium on gold required to pay the agents of the Institution in Europe and to meet the incidental expenses of exchanges, and the cost of serials and other works necessary for the use of the collaborators and other persons engaged in researches for the Institution. 3d. In consequence of the non-payment of the interest on the southern State stocks forming part of the extra fund.

Still it will be seen by the report of the Executive Committee that the expenditures of the Institution during the past year have been kept within the receipts, and a large balance retained in the treasury to meet the contingencies which may arise, particularly in the present unstable condition of public affairs.

The interest on the Indiana stock for the first half of the year was paid in specie, which was deposited with Messrs. Riggs & Co., with the understanding that it could be withdrawn in the same currency at any time it might be required. In the settlement of the accounts for the year this specie was sold and the premium added to the receipts.

A power of attorney has been forwarded from the President of the United States to Messrs. Fladgate, Clarke & Finch, of London, authorizing them to collect the remainder of the Smithsonian fund, which was left, by the Honorable Mr. Rush, as the principal of an annuity to the mother of the nephew of Smithson. The power of attorney was forwarded to the care of Honorable Charles F. Adams, American minister to England, and the money, when collected, will be deposited with George Peabody & Co., bankers, London, subject to the order of the Institution.

As a fact connected with the history of this establishment, I may mention that the charter of a society in this city known as the National Institute expired in July last, and that in accordance with the law of

Congress incorporating that society, the Secretary of the Interior delivered the remainder of its library and museum to this Institution.

The National Institute was founded twenty years ago, for the cultivation of science, by a number of gentlemen of the city of Washington, most of them connected with the government. It at first found favor with the public, and the hope was entertained that an endowment, in the form of a donation of land, or otherwise, would be granted by Congress, by which it would be enabled to support a museum and a library, and publish a series of transactions ; but this hope was not realized, and as the field it proposed to occupy fell within the province of the Smithsonian Institution, the society gradually declined in activity, and finally allowed its charter to expire by its own limitation. Before the organization of the Smithsonian Institution, the personal effects of Smithson, and the large collection of specimens procured by the exploring expedition, were placed in charge of the National Institute, and from the similarity of names the two establishments were at first frequently confounded with one another.

A large number of valuable books and specimens were presented to the Institute by various societies and individuals; but as there was not sufficient means to constantly employ a curator, or even to supply appropriate rooms for their preservation, these collections have been rendered comparatively of little importance. The specimens in ornithology and entomology were almost entirely destroyed by insects, and the library reduced principally to broken sets of periodicals and transactions of learned societies, duplicates of those already in the library of the Institution. In some cases, however, we have been enabled to supply deficiencies, and the books so incorporated into the Smithsonian collection have been properly designated by an appropriate mark in the manuscript catalogue of the library. The most valuable part of these collections was that which related to mineralogy and ethnology.

Publications. The publications of the Institution, as stated in previous reports, consist of three series: 1st. The Contributions to Knowledge. 2d. The Miscellaneous Collections. 3d. The Annual Reports.

The Contributions include memoirs embracing the records of original investigations and researches, resulting in new truths, such as are considered interesting additions to the sum of human knowledge. Twelve volumes in quarto of this series have been published—the thirteenth is still in the press.

The Miscellaneous Collections include works intended to facilitate the study of the various branches of natural history, to give instruction as to the method of observing natural phenomena, and to furnish a variety of other matter connected with the progress of science. Of this series four large octavo volumes have been issued, and a fifth has been commenced.

The Annual Reports to Congress consist, each, of an octavo volume of 450 pages. They contain the report of the Secretary as to the operations and condition of the Institution, the acts of the Regents, and an appendix, giving a synopsis of the lectures delivered at the Institution, extracts from correspondence, and articles of a character suited to meteorological observers, to teachers, and other persons especially interested in the promotion of knowledge.

Contributions to knowledge.—The following papers have been accepted for publication in the 13th volume of Contributions:

1. The concluding paper of Dr. Kane's Series of Observations in the Arctic Regions.
2. The reductions of the observations of McClintock while in search of Sir John Franklin.
3. Parts II to VI of the Reduction of the Girard College Observations, by Professor Bache.
4. Ancient Mining on the Shores of Lake Superior, by Charles Whittlesey.
5. On Respiration in the *Chelonia*, by Drs. Mitchell and Morehouse.
6. On Magnetic Observations in Pennsylvania and adjacent States,. by Professor Bache.

In the reports for 1859 and 1860 accounts are given of two parts (which were subsequently divided into three parts) of a series of reductions of the magnetic observations made at Girard College, Philadelphia, from 1840 to 1845, inclusive, by Professor Bache. The first two of these papers relate to the investigation of what is called the eleven-year period of the variations of the needle, which coincides with the recurrence in frequency of the spots on the sun. The third paper relates to the influence of the moon on the variation of the magnetic needle as shown by the Girard observations.

Of the same series there have been accepted and printed parts 4, 5, and 6. The fourth refers to the change of horizontal force coinciding with the eleven-year period of the spots on the sun. In explanation of this it may be stated that the whole magnetic force of the earth upon a needle freely suspended causes it to take a direction inclined to the horizon, at all places north and south of the magnetic equator, which direction is called the magnetic dip or inclination. The force with which the needle is drawn into this position may be resolved into two forces—one in the plane of the horizon, and the other perpendicular to this plane. By knowing the horizontal component, and the angle of the dip, the total force may be readily calculated, and as this element can be much more accurately determined than that in the line of the dip, it is the one which is generally made the object of observation.

In the case of the observations made by Professor Bache, the instrument employed was one of Gauss's large bifilar magnetometers, which consisted of a magnetic bar weighing twenty-five pounds, upwards of thirty-six inches long, and suspended horizontally by two long fine wires slightly diverging from parallelism. This magnetic bar instead of being allowed to take a north and south position, or to settle in the direction of the magnetic meridian, was forced to turn nearly at right angles to this position by twisting the pair of suspension wires so as to overcome the directive force of the magnetism of the earth. In this position the bar was in a state of equilibrium between two forces, viz: the torsion of the wires tending to turn the north end of the needle round towards the west, and the horizontal magnetic force of the earth which tended to draw it back into the meridian. In this condition, if the magnetism of the earth diminishes, the force of torsion will prevail, and the bar will move from the meridian. If the magnetism of the earth increases, the torsion will be relatively weaker, and the bar will move in an opposite direction. Attached to this bar was a mirror which, reflecting the image of the divisions of a scale into the object glass of a telescope, enabled the observer to perceive a variation in the intensity of the force, equivalent to a ten-thousandth part of the whole force.

The indications of this instrument were corrected for variations in magnetism produced by changes of temperature, and for a constant small diminution in the intensity of the bar, from an actual loss of its magnetic force. But besides these changes in the magnetism of the bar itself, there is a progressive change in the horizontal component of the earth's magnetism, which may be due either to a change in the direction of the force, or to a change of its intensity. After allowing for these, from observations made in various parts of the earth on changes of direction and intensity, the next step was to separate the large disturbances, which have been called magnetic storms, from the series of ordinary records. For this purpose deviations from the normal position of the bar amounting to thirty-three scale divisions were considered as due to abnormal disturbances of this kind. This number was ascertained by a theorem founded on the doctrine of probabilities, for which we are indebted to Professor Pierce, of Cambridge. Out of 24,231 observations, 1,698, according to this criterion, were considered as abnormal disturbances. After all the larger disturbances were excluded, new monthly means were taken, and all deviations from these of thirty-three divisions were again set apart.

The result of this elaborate investigation is, that the variation in intensity of the horizontal component of the earth's force is subject to a change coincident with that found from observations in other parts of the world, which has been called the eleven-year period, or that corresponding with the frequency of the appearance of the spots on the sun. The observations extended only through five years, which was less than half the whole period ; the data were not therefore sufficient to determine the movement through its complete cycle, although they served to mark the minimum point and the ascending and descending parts of the curves of illustration between two maxima.

The fifth part relates to investigations on the effect of the sun in producing daily and annual variations of the horizontal component of the magnetic force. These investigations were made upon the quantities which remained after removing from the tables the larger or fitful disturbances previously described, leaving the normal effect due to the sun from a change in its distance from the earth, and perhaps from a change in the relative position of its magnetic poles on account of the revolution of the earth in its orbit. Blanks in the tables of observations were filled by interpolation. From these investigations it appears that the horizontal magnetic force of the earth, or the intensity with which a deflected magnetic needle is drawn back toward the north, varies with the different hours of the day. The variation is greatest in summer and least in winter; the force is most intense at a little after 6 o'clock in the morning, and diminishes rapidly until a little after 10 o'clock a. m., when it reaches its minimum, and again commences to increase, and continues to do so until about 3 o'clock p. m., when it again declines until about 11 o'clock at night. The amount of change is much greater in the day time than at night, the whole indicating that this daily variation depends on the varying heat derived from the sun.

The 6th and last part of this interesting series relates to the lunar influence on the horizontal magnetic force.

Each observation, after being corrected for temperature and for the progressive changes in the earth's magnetism, was marked with the corresponding lunar hours, reckoning from the passage of the moon over the meridian. The difference was then taken between the position of the magnetic bar, as calculated from all the hours and that found by observation at each lunar hour, and the difference was considered as the effect of the magnetism of the moon on the earth.

From these and similar observations, it appears that the moon exerts a magnetic influence on the earth. The principal magnetic effect takes place 2 h. 52 m. after the upper passage of the moon over the meridian; secondary effect, 1 h. 7 m.; lower passage, principal minimum, at 6 h. 41″; lower passage, secondary minimum, at 8 h. 19 m. after upper.

Another paper contains an abstract of the observations and results, with the discussion of a detailed magnetic survey of Pennsylvania and part of the adjacent States of New York, Ohio, and Maryland, originally made by Professor Bache, and resurveyed by Mr. Charles A. Schott.

In the study of terrestrial magnetism, two objects are essentially necessary: first, the direction and intensity of the magnetic force of the earth at a given epoch; and secondly, the variation in these elements after a known interval.

In 1840, 1841, and 1843, Professor Bache made a series of observations to determine the magnetic elements at a number of prominent places in Pennsylvania, New York, Ohio, Maryland, and Canada. The whole number of stations at which he ascertained the declination or variation was 16, and of those where the dip and intensity was observed 48.

Last summer Mr. C. A. Schott, an assistant in the coast survey, visited six of the stations at which Professor Bache had previously observed the magnetic elements, and by carefully redetermining them obtained the data for calculating the secular changes which had taken place during an interval of about twenty-years. The observations originally made by Professor Bache, and also the late ones by Mr. Schott, were obtained with instruments of great precision, and will always furnish the elements of future comparison for the study of the changes which the magnetic force of the earth undergoes in long periods. The paper commences with an abstract of those on the declination. To these are added the latitudes and longitudes of the places observed.

The observed magnetic variations, at the different places, have been reduced to the epoch of January, 1842, by adding the annual increase, namely 2′ 7, found by comparing the observations of 1840 with those of 1862. An elaborate discussion of the horizontal force furnishes a comparison of European and American results. The deductions from the observations on the dip of the needle are given in terms of geographical latitude and longitude, and the isoclinal lines or lines of equal dip are protracted on a map which also contains the lines of equal magnetic force, both in regard to its horizontal and total intensity. Three of the stations visited by Mr. Schott fell within the scope of the coast survey; and the expense of the redetermination of the magnetic elements, at these points, was defrayed by the superintendent of that work; the determinations at the remaining stations were at the expense of the Smithsonian Institution.

An account of the other papers in this volume has been given in the previous report.

Miscellaneous Collections.—Several series of articles forming parts of the series of Miscellaneous Collections, as stated in previous reports, have been undertaken, of which some have been completed, some are still in hand, and others have been printed during the past year.

The first of these series is that relating to the shells of North America, and will consist of the following works :

1. Check lists of North American shells, by P. P. Carpenter, &c.
2. Circular relative to collecting shells.
3. Elementary introduction to the study of conchology, by P. P. Carpenter.
4. List of the species of shells collected by the United States exploring expedition, by the same author.
5. Descriptive catalogue of the shells of the west coast of the United States, Mexico, and Central America, by the same author.
6. Descriptive catalogue of the air-breathing shells of North America, by W. G. Binney.
7. Descriptive catalogue of several genera of water-breathing fresh-water univalves, by the same author.
8. Descriptive catalogue of the *Melaniadae*, or the remainder of the water-breathing fresh water univalves, by George W. Tryon.

9. Descriptive catalogue of the *Corbiculidae* or *Cyclodidae*, a group of bivalves principally inhabiting fresh-water, by Temple Prime.

10. Descriptive catalogue of the *Unionidae*, or fresh-water mussels.

11. Descriptive catalogue of the shells of the eastern coast of the United States, by W. Stimpson.

12. Bibliography of North American conchology, by W. G. Binney.

The first and second articles of the foregoing list were printed in 1860, and described in the report for that year, and since then in their wide distribution have done good service in facilitating the collecting, labelling, and exchanging of specimens of conchology.

The third article was published in 1861, as a part of the annual report for 1860, and a new edition will be incorporated in the Miscellaneous Collections as soon as we can procure the long-promised wood-cuts from the British Museum, to illustrate the work.

The fourth and fifth articles are still in the hands of Mr. Carpenter, and will, it is expected, be ready for the press the ensuing year.

With reference to the sixth article, it may be mentioned that for many years before his death, the late Dr. Amos Binney, of Boston, was engaged in collecting and arranging materials for a general work upon the land shells of the United States. The result of his labors was published after his decease, in three large volumes, giving copious descriptions and accurate figures of all the species. His collections are now in the hands of his son, Mr. W. G. Binney, of Burlington, New Jersey, who has since greatly extended them, and has brought up the subject of his father's work to the present day, by various supplements, memoirs, &c. He has also lately rearranged all the materials in his possession, and prepared from them, for this Institution, the synopsis which is given as the sixth article in the above enumeration.

The seventh and eighth articles include an account of fresh-water univalves of the United States. Within the last twenty years the number of described species of this class of animals known in this country has greatly increased. The descriptions of these previously made were from specimens collected in isolated situations, oftentimes by persons who had not had the opportunity of studying large collections or of comparing typical specimens, and, indeed, in some cases without access to descriptions which had been previously made by others. The shells belonging to this class are characterized, perhaps above most others, by a remarkable range of variation in form and size arising from local causes, or different stages of growth, &c.

Many of them are also furnished with so few positive specific external characters, that for their determination an anatomical difference in their soft parts can alone be depended upon. To study, therefore, satisfactorily single species of fluviatile shells, one must have before him, says Mr. Binney, a very large suite of specimens of all ages, from every portion of the district which it inhabits, as well as authentic specimens of every allied described species, with an equally complete set of individuals. From these facts it is evident that among the descriptions of these shells which have been made by different authors, and published in different works, there must be many which refer to the same species; or, in other words, that the bibliography of this branch of natural history must abound in synonyms exceedingly perplexing to the student, as well as exhibit imperfections in the systematic exposition of the subject which ought not to exist. Such a condition must occur from time to time in every branch of natural history, and it therefore becomes important that some person competent to the task should go over the whole field and reduce to what is called a monograph, or single sketch, all that has been previously done in regard to it.

Mr. Binney in his portion of the catalogue does not attempt to present a complete monograph, but rather a report on our present state of knowledge relative to fluviatile univalves. He has given an English translation of all the original descriptions and a *fac-simile* of the outline of each original figure. The portion of this work which relates to the fresh-water univalves of North America intrusted to Mr. Binney has been completed, and is now in the press. The remainder on the *Melaniadae*, undertaken by Mr. Tryon, has been commenced; but since the family contains a large number of species exhibiting many of the variations above-mentioned, a year or more will be required before it can be completed.

The ninth article, or that relating to the *Cycladidae*, is nearly ready for the press. The recent species of this group are generally small and inhabit the fresh waters of various parts of the world, although some belong to brackish or even marine localities. Mr. Prime has paid particular attention to this group, and is said to be one of the first living authorities in regard to the subject.

The eleventh article of the above-mentioned series will include detailed descriptions by Mr. Stimpson of all the marine shells of the eastern coast of the United States, with original notices of the internal structure.

The twelfth article in the series above enumerated is the bibliography of conchology, which has been prepared to avoid the necessity of repeating bibliographical references in the different manuals just enumerated. It is intended to give, first, an account of all the articles or works of American authors, relative to conchology in general, and secondly, those of foreign writers relative to the conchology of North America.

All the species referred to by authors are enumerated after the title of each article, and such references to the page and date of description are given as will enable any species to be quoted from the bibliography itself without the necessity of referring to the original. A complete index of authors, and of all the species mentioned, placed at the end of the work, will greatly facilitate its use. The first part of this work, embracing the writings of American authors, forms an entire volume of the Miscellaneous Collections of upward of 700 pages, of which 400 are already stereotyped.

Another series of works belonging to the Miscellaneous Collections is intended to facilitate the study and the advancement of the science of entomology, of which the several articles are the following:

1. Instructions for collecting and preserving insects.
2. Catalogue of the described Diptera (flies, mosquitoes, &c.) of North America, by Baron Osten Sacken.
3. Catalogue of the described Lepidoptera (butterflies, moths, &c.) of North America, by Dr. Jno. G. Morris.
4. Classification of the Coleoptera (beetles, &c.) of North America, by Dr. Jno. L. LeConte.
5. Synopsis of the described Neuroptera (dragon flies, &c.) of North America, with a list of the South American species, by Hermann Hagen.
6. Synopsis of the described Lepidoptera of North America. Pt. 1, Diurnal & Crepuscular Lepidoptera, by Dr. Jno. G. Morris.
7. List of the Coleoptera of North America, with descriptions of new species, by Dr. John L. LeConte.
8. Monographs of the Diptera of North America, by H. Loew, with additions by Osten Sacken.
9. Catalogues of Homoptera and Hemiptera (chinches, roaches, &c.) of North America, by P. R. Uhler.
10. Descriptive catalogue of the Hymenoptera (bees, wasps, &c.) of North America, by Dr. Henri DeSaussure.

These have all been described in previous reports, and have all been printed excepting the 9th and 10th, which are still in preparation, and a part of the 7th, which has been delayed on account of the call on Dr. LeConte to a position in the medical department of the army of the United States.

The instructions for collecting and preserving insects were published in the appendix to the report for 1858. A new and enlarged edition will be published during the coming year as a part of the next volume of the Miscellaneous Collections.

Another part of the Miscellaneous Collections is a series of tables furnished by Professor Guyot in addition to the set of physical tables by the same author, previously published by the Institution. The first of these is intended for converting the klafter and feet of Vienna into measures of length of other countries. They are to be substituted for those contained in the former edition, because they are derived from a new comparison of the *klafter* of Vienna and French *toise*, by Von Struve and Von Littrow. This comparison has furnished the standard adopted in the great trigonometrical survey of Austria. The second set of tables is for converting the Spanish or Mexican and Bolivian varas and feet into English and French measures.

The preceding tables have been printed, and are inserted in volume I of the Miscellaneous Collections.

The third set of tables gives the value of a degree of the meridian in every degree of latitude from the equator to the poles, in metres, kilometres, German miles, nautical leagues, French leagues, geographical or nautical miles, and English statute miles.

The fourth set of tables gives the value of each degree of longitude on each parallel of latitude, from 0° to 90°, in the same measures as those mentioned above.

The two preceding sets of tables, which are based on Bessel's terrestrial elements, have been computed under the direction of Professor Bache at the office of the Coast Survey, and have been kindly furnished by him for this volume. The columns of the German miles and nautical and French leagues have been added to make the table more complete.

The fifth set of tables is for converting the most usual measures of length into each other, such as myriametres and kilometres, Austrian, Prussian, and German miles, nautical leagues, French, Spanish, and Mexican leagues, geographical or nautical and statute miles, and Russian wersts.

The sixth set of tables is for the conversion of the corresponding surface or square measures of the preceding table into each other.

These measures, which are also founded on the late terrestrial elements of Bessel, have been selected with reference to the frequency of use in the consultations of the official publications of different European nations. Each of the above tables furnishes the value of every full number from 1 to 100, in six figures for the measures of length, and seven for those of surface.

The seventh set of tables gives the velocity of rotation of the earth for each degree of latitude, expressed in the measures of length most frequently used, and will facilitate computations in regard to the velocity of the wind.

The whole series comprises over 17,000 separate numbers, filling 70 large octavo pages.

It is proposed to add to these tables one giving the length of time of sunshine in different latitudes for each day in the year, and, to complete the series, a number of tables for calculating the resultant direction of the winds.

It is believed that these tables will be received as a welcome addition to the physical tables previously published by the Institution, and which are now well known and used in every part of the civilized world. They will be of value to the meteorologist and physical geographer, as well as to the statistician and political economist. The first set is indispensable for easily computing distances on globes, maps, &c.; the second will greatly facilitate the use of the numerous data furnished by travellers, scientific publications, and especially the official statistical information contained in the various documents of the states of Europe. The selection of the various measures is made with special reference to the latter—as the myriametre and kilometre for the study of later French publications, and the common French league for the older ones; the Austrian and Prussian miles, the Russian werst, and English statute mile for publications in the countries where those measures are used. In the preparation of these tables a question has arisen whether an additional series should be constructed for the conversion of the American foot into the English, between which there is, unfortunately, a slight difference.

Reports.—The annual reports to Congress are printed at the expense of the government, with the exception of the cost of the wood-cuts, which is at the expense of the Institution. The report for 1861 contains, besides the report of the Secretary and the acts of the Regents, a Eulogy on Prof. Felton, by Dr. Woolsey, President of Yale College; a Eulogy on Hon. Stephen A. Douglas, by Hon. S. S. Cox; synopsis of lectures on the Construction of Bridges, by Fairman Rogers, of the University of Pennsylvania; lecture on the relation of Time and Space, by Prof. S. Alexander, of the College of New Jersey; lecture on Arctic Explorations, by Dr. I. I. Hayes; a memoir of Geoffroy St. Hilaire, by M. Flourens; the chemical analysis of the Sun by means of the Solar Spectrum; the small planets between Mars and Jupiter; studies and experiments on Metamorphism, by M. Daubrée, and a number of articles on Archæology, all translated for the Institution; also a report on Nitrification, prepared for the Institution by B. F. Craig, M. D.; the history of Petroleum, by T. Sterry Hunt; and the explosibility of Coal Oil, by Z. Allen; list of birds inhabiting the District of Columbia, by E. Coues and D. W. Prentiss; and a series of prize questions of scientific societies, together with a number of minor articles.

The reports for a number of years past have contained a series of memoirs of distinguished men of science, members of the French Academy, translated for the Institution by C. A. Alexander, Esq., of this city. It is intended to continue the translation and publication of similar memoirs, and when the number is sufficient to form an ordinary sized volume, to collect and publish them in a separate form.

Of the last report, 10,000 extra copies were ordered by Congress, of which 4,000 were presented to the Institution for distribution among its special correspondents. The requests for copies of this work have been constantly increasing from year to year, and it is to be regretted that the pages were not stereotyed, since there is now a large demand for back numbers, to complete sets, which cannot be obtained.

Previous to last year we were allowed to have extra copies of certain articles of the report struck off for separate distribution, but under the new rules for the regulation of the public printing this privilege was denied us in the case of the report for 1861. It is thought, however, that if a proper statement were made to Congress, a clause would be added to the acts relative to the government printing which would give all the facilities required.

The following general rules for the distribution of the reports have been adopted:

1st. They are presented to all the meteorological observers who send records of the weather to the Institution.

2d. To the collaborators of the Institution.

3d. To donors to the museum or library.

4th. To colleges and educational establishments.

5th. To public libraries and literary and scientific societies.

6th. To teachers, or individuals who are engaged in special studies, and who make direct application for them.

It is proper to remark that, owing to the many changes which have taken place since the commencement of the war in the list of correspondents of the Institution, it has not been thought advisable to send the report, as heretofore, to all whose names are on the record of distribution, but in most cases to wait until direct application is made by letter for a copy of the work. Whenever a report is sent to any address, a separate announcement is made through the mail by enclosing a blank receipt, to be signed and returned to the Institution.

In view of the great cost of paper at the present time and the space required for storage of a large edition of each volume of the publications of the Institution, it has been thought advisable to stereotype the text and strike off only as many copies as are required for immediate distribution, printing a new supply from time to time to satisfy the demands as they arise. By the adoption of this plan, should the cost of paper return to its normal rate, the expense of the stereotype plates would be saved.

Meteorology.—The meteorological system has continued to be carried on as in former years, though necessarily very much diminished in extent on account of the absence of records from the southern States. The volume of meteorological reductions from 1854 to 1859, an account of which was given in the last report, has been published and distributed to the meteorological observers, to public libraries, and foreign institutions. Many letters have been received expressing the new interest in the system that has been awakened by the appearance of this volume, and the records which before existed only in the archives of the Institution are now in the hands of a large number of the students of science, whose various tastes and abilities will enable them to draw from them important general results. The second volume is still in press at the Government office, the printing of it having been delayed by that of documents of more immediate importance. It is expected, however, that it will be finished during the year 1863.

The daily telegraphic bulletin of the state of the weather, which was entirely discontinued for some time on account of the demands of public business, has been partially resumed, and we are indebted to the courtesy of Mr. Anson Stager for occasional despatches since the early part of December last, from stations in the Rocky mountains, and even from points as far west as California. These telegrams, however are not sufficient to enable us to predict, with much probability, the changes of weather, without the additional information from the south and southwest. The telegraphic bulletin giving the daily condition of the weather at various important positions on the continent of Europe continues to be lithographed by the Imperial Observatory at Paris, and is sent to the Institution by every steamer.

In May last a circular was distributed asking for information on the subject of tornadoes, the principal design of which is to direct attention to a full and definite system of observations, so that on the recurrence of tornadoes precise and uniform reports may be obtained as to all the features of the phenomenon. In addition to the special replies to the circular, a number of general answers have been received, which are of interest in helping to define the regions of our country where these destructive visitants frequently appear, and those in which they are happily almost unknown. From Steuben, Maine, Mr. J. D. Parker writes: "There was never a tornado seen hereabouts by any one, so far as I can learn;" and Mr. R. H. Gardiner, at Gardiner, in the same State, says: "Tornadoes are of very rare occurence in this part of the country. I have no knowledge of any within the last fifty years." From Vermont, Mr. Hiram A. Cutting, of Lunenburg, informs us that "there never was but one tornado, in this section, in my remembrance, and that was at Victory, about twenty miles northwest of this place, in the summer of 1842." Mr. Levi Packard, residing in the State of New York, at Spencertown, Columbia county, between the Hudson river and Massachusetts, tells us there are no tornadoes in that tract of country. On the other hand, we are informed that over the swampy region of the great bend of the Mississippi tornadoes are very frequent, and the paths of many of them are marked upon the ground for miles by prostrate trees and other indications of a violent disturbance of the atmosphere.

Correct general information of this kind would furnish material for the preparation of an interesting map, showing at a glance the regions where tornadoes annually prevail, and those where they occur only occasionally, or not at all. In meteorological investigation we always make a step in advance when the region to which phenomena are limited can be accurately defined.

Since the latter part of October, a series of daily observations has been made for the Institution, by Mr. Force, on the temperature of the hydrant water in Washington. The water flows some six or seven miles through iron pipes, under ground, before reaching the place of delivery, and acquires, approximately, the temperature of the soil at the depth at which the pipes are laid. The observations were made at a hydrant out of doors, about 7 o'clock every morning, the water being allowed to flow two or three minutes before noting the temperature. When the curves for the water and the air are plotted on the same diagram and the same scale, they exhibit to the eye, in a very striking manner, the extreme fluctuations of the temperature of the air, compared with that of the ground, at the depth of only two or three feet. They also show the slowness with which variations of temperature penetrate the earth. The change produced by a decided increase or decrease of temperature being generally indicated by the water two or three days after it occurred in the open air.

Among the contributions relative to meteorology which have been received at the Institution, is a series of tri-daily observations made at Brunswick, Maine, by the late Professor Parker Cleaveland, from 1808 to 1859. The records include observations on the ordinary thermometer, the maximum and minimum thermometer, the barometer, rain-gauge, wind-vane, &c. It is proposed to publish these in full, as a part of the second volume of the "Meteorological Results," but for this purpose it is desirable that the means for months, years, and the whole period, should be calculated, and this work is now in progress, and will probably be finished before the printer will call for the copy. The publication of these observations is in accordance with the plan adopted to print in full a number of long series of observations such as those we have already published, viz: for Providence, Rhode Island, and Washington, Arkansas. They will be of much interest for solving various meteorological problems, such as the recurrence of particular phenomena, changes of seasons, verification or disproval of the many empirical rules which have been proposed and are used for predicting the weather, &c. The extensive series of observations made by Dr. S. P. Hildreth, of Marietta, Ohio, mentioned in a previous report, belong to the same class, and will also be inserted in the volume above mentioned.

We have already referred to the additions to the meteorological tables of Professor Guyot, and stated that it is proposed to still further increase these tables by a series intended to facilitate the calculation of the mean direction of the wind.

The directions and blanks for making meteorological observations which have been prepared and published at the expense of the Institution have been extensively distributed in this country, and translated and reprinted abroad. The geographical and statistical society of Mexico has recently adopted them, and signified the intention of co-operating with the Institution in extending its system of observations more widely over this continent. Were not our general system of meteorology interrupted by the discontinuance of the reception of observations from the southern States, this would be an important addition to our means of tracing the extent of disturbance of the atmosphere which accompanies our winter storms.

Professor Guyot availed himself of a visit which he made to Europe last year to establish by his own observation the relation of the standard barometers used by the Institution to the most important standards of the European observatories. The comparisons were made by means of two Fortin barometers, with cistern of constant level, as modified by Ernst, of Paris, and constructed several years ago by that skilful artist for the Institution. Both instruments had been previously tested by long usage, and just before leaving this country their relation to a large standard barometer by Newman, of London, and a large sized barometer by Ernst, was ascertained by a series of numerous comparisons made with great care. These two instruments were directly compared in Europe with a standard barometer at Kew observatory, now acknowledged as the normal barometer among English meteorologists, and with the standard instruments at the Brussels, Berlin, and Geneva observatories, by the kind permission and assistance of the directors of these several institutions, Professors Stanley, Quetelet, Enke, and Plantamour. The last set of comparisons at Geneva, Switzerland, was found particularly useful from the fact that by the untiring care of Professor Plantamour the relation of his barometer has been fully established with that of Regnault in the College de France, and that of the observatory of Paris. On his return Professor Guyot made a new series of comparisons with the same Smithsonian standards as before, the results of which proved that no change had taken place during his absence in the two barometers used abroad. It is believed that these comparisons establish a correspondence of the European and American standard barometers within the narrow limit of one or two-thousandths of an inch. A large standard is about to be prepared under the direction of Professor Guyot, to which these determinations will be referred.

The meteorological branch of the operations of the Institution still continues under the charge of Mr. William Q. Force, to whose habits of order and scrupulous accuracy the system is much indebted for whatever value it possesses.

Among the contributions to the meteorological materials of the Institution, presented during the last year, is a series of continuous records of the changes of atmospheric temperature made by a machine invented by Dr. James Lewis, of Mohawk, New York. This instrument consists of an arrangement of a number of brass and iron wires, whose relative contraction and expansion give motion to a metallic point, the several positions of which are marked by a puncture in a fillet of paper, produced by a blow of a hammer moved by clockwork, repeated at regular intervals of fifteen minutes. Although instruments of this kind have been frequently constructed, they have generally not possessed sufficient sensibility to indicate the fitful changes of the atmosphere. Dr. Lewis appears, however, to have been more successful in his invention, and from the results which he has presented to us it would appear that the registers of his self-recording instrument are of considerable value in determining the general law of changes of temperature, especially during the day, and thus furnishing corrections by which the mean temperature of places in the same latitude can be obtained from observations made at only one or more hours of the day. It may be mentioned here that an idea has been very prevalent in this country among observers that to obtain the average temperature of a place, the best times of observation are about sunrise and sunset, and at 2 or 3 o'clock in the afternoon; but as the rising and setting of the sun occurs at different times in different seasons of the year, and as the maximum temperature occurs at different hours in different latitudes, it is best always to make the observations as nearly as possible at fixed hours, as, for instance, at those which have been adopted by this Institution, viz: 7, 2, and 9; since, by observing this rule, corrections derived from such observations as those made by Mr. Lewis can be applied so as to give with more precision the average temperature of the place of observation.

The present Surgeon General, Dr. Hammond, takes a lively interest in meteorology, as one of the branches of science intimately connected with his department; and as soon as the posts and cantonments are again permanently established will reorganize the system of army observations on a more extended scale, and furnish it with the instruments and instructions prepared under the direction of this Institution.

Laboratory.—The operations of the laboratory during the past year have principally consisted in the preparation of a large quantity of Laborraque's disinfecting liquid, and the continuance of the examination of minerals preparatory to a distribution of duplicates. More than a thousand bottles of the disinfecting liquid have been prepared for the use of the hospitals in the city of Washington. The efficacy of this substance has been demonstrated by abundant experience. Simply sprinkling it on the floor, or wetting with it a cloth placed near the source of unpleasant effluvia, at once renders the air of the apartment entirely inodorous. The preparation of this substance is still going on and will be continued as long as the article may be required for the hospitals.

The examination of the minerals preparatory to a distribution of the duplicates has not been carried on as rapidly as could be wished, on account of the absence of the person to whom this duty was assigned. We have, however, ready for distribution, upwards of two hundred sets of specimens, properly labelled, of the stones used in erecting the public buildings in the city of Washington.

In consideration of the high rate of exchange no purchases of foreign apparatus have been made during the past year. Occasion however, has been taken, in the interval of other business, to remodel the cases of the apparatus room, in order to a better disposition and display of the instruments. The room itself is fifty feet square with a ceiling of twenty-five feet in height. Around the four sides of this room, cases of about six feet deep and ten feet high have been constructed. The upper floorings of these cases extend about two feet beyond their front, and thus form a projecting gallery entirely around the room, which serves for the display of larger instruments as well as to increase the available capacity of the apartment. The whole arrangement produces a pleasing architectural effect, while from the size of the cases instruments may frequently be used in the way of experiment without bringing them out into the room, or exposing them to the handling of visitors.

Collections of Specimens of Natural History, &c.—In the last two reports a distinction was drawn between the large collections of specimens of natural history, &c., which have been made through the agency of this Institution and what is called the Smithsonian Museum. This distinction has become necessary in order to separate more clearly in the public mind two objects, which, although they are generally confounded, are in the case of this Institution essentially different. The object of making large collections of duplicate specimens is twofold, first, to advance science by furnishing to original investigators, wherever they may reside, new materials for critical study; and second, to diffuse knowledge by providing colleges, academies, and other educational establishments with the labelled specimens necessary to give definite ideas of the relations and diversities of the various productions of nature. The principal end attained by the public museum of the Institution has been the gratification and incidental instruction of the visitors to the city of Washington. It is true that there are preserved in the museum the type specimens of the species and genera which have been described, and of which accounts have been published at the expense of the Smithson fund, or by other means; but for the preservation of these there is required no costly building nor corps of attendants, and, indeed, the charge of them might well be assumed by other establishments.

From the foregoing exposition it will readily be seen that while the collecting and distributing of large numbers of specimens is an important means of increasing and diffusing knowledge, and as such is in strict accordance with the will of the founder of this Institution, the support of a public museum, the effects of which must of necessity be in a great degree local, is not so consistent with the liberal intention of the bequest.

It should not be inferred from the foregoing remarks that I mean to disparage the establishment of a general collection of objects of nature and art, like that of the British Museum or the Garden of Plants, which includes in its design the encouragement of original study as well as of popular instruction and amusement. On the contrary, I have always advocated this measure as one of national importance, while I have endeavored to show that it ought not to be attempted by means of the Smithsonian fund, and that it could only be properly carried out by a liberal annual appropriation from the public treasury.

I have thought it necessary frequently to urge the importance of guarding against the tendency to increase the expenditure on local objects, and against accepting presents on condition that they shall be perpetually preserved and exhibited to the public at the expense of the Smithsonian fund. If this propensity were indulged in and donations solicited on the terms mentioned, which are those usually agreed to in similar cases, the whole income of the bequest would be ultimately absorbed in providing house-room and accommodations for the collections; and the active operations, as they are called, which have given so much celebrity to the name of Smithson, and have constituted the distinctive feature of the establishment, would cease.

The collections of specimens which have formed the prominent subject of the preceding remarks may be divided into two classes, namely, those which have been studied and an account of them published in the reports of the government expeditions, or in the transactions of the Smithsonian and other institutions, and those which have not been described, and which consequently are considered of much interest to the naturalist, who may be anxious to make new explorations in the domain of natural history. Of both classes the Institution possesses a large number, for the disposition of which the following general rules have been adopted:

First. To advance original science, the duplicate type specimens are distributed as widely as possible to scientific institutions in this and other countries, to be used in identifying the species and genera which have been described.

Second. For the purposes of education, duplicate sets of specimens, properly labelled, are presented to colleges and other institutions of learning in this country.

Third. These donations are made on condition that due credit is to be given the Institution in the labelling of the specimens, and in all accounts which may be published of them.

Fourth. Specimens are presented to foreign institutions, on condition that if type specimens are wanted for comparison or other use in this country they will be furnished when required.

Fifth. In return for specimens which may be presented to colleges and other institutions, collections from localities in their vicinity shall be furnished when wanted.

In the disposition of the undescribed specimens of the collection, the following considerations have been observed as governing principles:

First. The original specimens are not to be intrusted for description to inexperienced persons, but to those only who have given evidence of an ability properly to accomplish the task undertaken.

Second. Preference is to be given to those who have been engaged in the laborious and difficult enterprise of making complete monographs.

Third. The investigator may be allowed, in certain cases, to take the specimens to his place of residence, and to retain them for study a reasonable time.

Fourth. The use of the specimens is only to be given on condition that a series of types for the Smithsonian Museum will be selected and properly labelled, and the whole returned in good condition.

Fifth. In any publications which may be made of the results from an investigation of the materials from the Smithsonian collection, full credit must be accorded to the Institution for the facilities which have been afforded.

During the past year, the labelling of specimens for colleges and other educational institutions has been continued, but the work has not advanced with as much rapidity as was expected, owing to the call upon many of our co-laborers to join the army. Under the most favorable circumstances the labelling requires much labor, and cannot be properly done except by persons specially trained in particular branches of natural history.

The assorting and labelling of the principal part of the shells is still in progress under Mr. Philip P. Carpenter, of Warrington, England, assisted by Dr. Alcock. Other shells have been named, or are in the process of being named, by Prof. Agassiz and Dr. Stimpson, of Cambridge; Mr. Isaac Lea and Mr. G. W. Tryon, of Philadelphia; Mr. W. G. Binney, of New Jersey; Mr. Prime, of New York; Mr. Busk, of England; and Dr. Steenstrup, of Copenhagen.

The botanical collections, to which several additions have been made during the past year, are still in charge of Dr. Torrey, of New York, and Dr. Gray, of Cambridge. The assorting of the rocks and minerals is carried on in the Institution; and as an auxiliary work, Mr. Egleston has prepared a general list of mineral species to facilitate the labelling and exchange of specimens. This will be printed and distributed to correspondents during the present year.

The additions to the collections of insects, during the year, have been referred for identification, as usual, to the collaborators in the line of entomology, viz: To Baron Osten Sacken, Dr. Le Conte, Dr. Loew, Dr. Hagen. Dr. Morris, Dr. Clemens, Mr. Edwards, Mr. Norton, Mr. Scudder, Mr. Ulke, and Mr. Uhler.

The whole number of entries on the record book of the Smithsonian collection, up to the end of 1862, is, according to the statement of Professor Baird, 74,775, and when it is understood that each entry is that of a lot which in most cases contains many specimens, some idea may be formed of the whole number of specimens which have been collected through the agency of the Institution, and the service which will be rendered when all these are made available for the advancement and diffusion of knowledge among men.

The Museum of the Institution consists principally of the type specimens of the various collections of objects of natural history and ethnology obtained by the different exploring and surveying expeditions sent out by the government of the United States, as well as by various special expeditions instituted at the expense of the Smithsonian fund. These specimens have generally been described, and in many cases figured in the reports published by Congress, or in the Smithsonian or other transactions, and have thus rendered their chief service in the way of advancing knowledge. Yet, in view of the future progress of science, it is important, irrespective of their use for the purposes of education, that these specimens should be carefully preserved, in order that they may be referred to as the original objects from which the descriptions were drawn. It often happens that in the subsequent study of similar specimens from other localities doubts arise as to some points of the published descriptions, which can only be solved by a reference to the original materials, and it is also frequently desirable to re-examine the specimens in relation to some new point of interest which may have been developed in the course of more extended investigation.

The additions to the museum should be confined principally to the type specimens collected and described at the expense of the general government, or under the immediate auspices of the Institution. Even thus restricted the specimens will increase in number as rapidly as that part of the Smithsonian fund, which is taxed for their support, will permit, and in time they will form of themselves a valuable collection of authentic illustrations of the natural history of America. It is true, as is often urged, that the value of these specimens would be enhanced by the addition for comparison of corresponding specimens from other parts of the world ; but the full adoption of this extension of the plan would involve the maintenance of a general museum, which, as has been repeatedly stated, is incompatible with the means and design of the Institution.

The only museum at present in this country, expressly established for the threefold object of popular instruction, systematic study, and original research, is that at Cambridge, under the direction of Prof. Agassiz. For the purposes of such a museum specimens of all kinds, from every part of the earth, are necessary, and in accordance with the liberal policy by which it has always been governed, the Smithsonian Institution has actively co-operated in assisting this exemplary enterprise of the State of Massachusetts.

During the past year the type specimens of a number of series of collections, of which the duplicates have been separated for distribution, have been transferred to the museum. The work of labelling the specimens, so that the common as well as the scientific name of each article may be distinctly exhibited, has been continued, and will probably be completed before the end of the present year. But for an account in detail of what has been done in regard to the museum and the collections, I must refer to the report of Prof. Baird, herewith submitted.

As a matter of interest, and, in some cases, of importance, a record book is kept at the principal entrance of the building, in which the names of all the visitors are entered. Since the date of the last report the Institution has been visited by thousands of persons, who have been called by business or pleasure to the national capital. The specimens of natural history and ethnology have excited much popular interest, particularly among those who come from the more distant western portions of the country. The museum and grounds are a favorite resort for the convalescent soldiers. The trees and shrubbery of the latter are growing finely, and the whole park, under the care of the Commissioner of Public Buildings, B. B. French, Esq., forms no unworthy memento of the talents of the lamented Downing, by whom its plan was designed.

I am sorry, in this connection, to consider it my duty to refer to the existence of an evil over which, though you have no official control, yet as legislators and prominent citizens you may exert a beneficial influence. I allude to the city canal, which forms the boundary of the Smithsonian grounds on the north. The basin or widest part of this canal, across which most of the visitors to the Institution have to pass, has become, since the introduction of the Potomac water, the receptacle of the sewage of the city, and is now an immense cesspool, constantly emitting noxious effluvia prejudicial to the health and offensive to the senses of all who approach the locality. This nuisance, which will continue to increase with the increasing use of the Potomac water, may perhaps be mitigated by placing a gate at each end of the wider part of the canal, to be closed after high tide and opened occasionally at low water, so as to discharge the contents with a force which would remove, in part, at least, the deleterious deposit. But the only effectual remedy, as it appears to me, is to fill up a part of the width of the canal, and convert the remainder into a sewer by covering it with an arch of masonry. This sewer may perhaps be cleared out by flood-gates, as before mentioned, or by anchoring flat-boats at the mouths of the drains, to be removed and emptied when filled. But whatever plan may be adopted, the character and prosperity of the city, as well as the interests of the Institution, are involved in a speedy and efficient effort to remove the evil. The small pecuniary benefit which may result from the canal to the city or to individuals ought not have any weight in the decision of this matter.

Explorations.—A part of the large collections which have just been described was gathered through officers and other persons attached to the surveying and exploring expeditions sent out by the government, and another part by expeditions expressly organized for the purpose, under the immediate auspices of the Institution. Among the latter is the expedition mentioned in the last two reports as having been undertaken by Mr. Robert Kennicott, of Chicago.

This enterprise has terminated very favorably, the explorer having returned richly laden with specimens, after making a series of observations on the physical geography, ethnology, and the habits of animals of the regions visited, which cannot fail to furnish materials of much interest to science.

The route traversed by Mr. Kennicott was from Lake Superior, along Kamenistiquoy river, and Rainy and Winnipeg lakes, up the Saskatchewan river to Cumberland House; thence nearly north through a series of rivers and lakes to Fort Churchill on English river, up the latter to Methy Portage, at which point he first reached the headwaters of the streams flowing into the Arctic ocean; thence along the Clear Water river and Athabasca lake, down Peace river into Great Slave lake, and along the Mackenzie river to Fort Simpson. At this place Mr. Kennicott spent a part of the first winter with the officers of the Hudson's Bay Company, making excursions up the Liard river to Fort Liard in autumn, and again on snow shoes in January. Before the close of the same winter he went up the Mackenzie to Big island, and thence northwest to Fort Rae, near the site of old Fort Providence. From this point he travelled on the ice across Great Slave lake to Fort Resolution, at the mouth of Peace river, where he spent the summer of 1860. He next descended the Mackenzie to Peel's river, and thence proceeded westward across the Rocky mountains, and down the Porcupine river to the Youkon, in the vicinity of which he spent the winter of 1860–'61, and the summer of 1861. The winter of 1861 and '62 was spent at Peel's river, and La Pierre's house in the Rocky mountains, and in travelling from this point up to Fort Simpson and back to Fort Good Hope on the Mackenzie. He left the last-mentioned place on the 1st of June, 1862, and reached home in October.

During the whole exploration he was the guest of the Hudson's Bay Company, the officers of which not only furnished him with free transportation for the materials he collected, but also extended to him in the most liberal manner the hospitalities of their several posts, and facilitated in every way in their power the objects of his perilous enterprise.

The principal object of the exploration was to collect materials for investigating the Zoology of the region visited. Mr. Kennicott, however, also collected specimens of plants and minerals, and gave considerable attention to the ethnology of the country, in observing the peculiarities of the various Indian tribes, and forming vocabularies of the languages. He carried with him a number of thermometers, and succeeded in enlisting a number of persons as meteorological observers, as well as in exciting an interest in natural history, and in physical phenomena, which cannot fail to be productive of important information respecting a region of the globe but little known.

The contributors to this exploration, besides the Smithsonian Institution, were the University of Michigan, the Audubon Club of Chicago, and several private individuals interested in the advance of natural history.

Mr. Xantus, whose explorations in Lower California have been mentioned in several of the previous reports, has been appointed by the government of the United States consul on the western coast of Mexico; and in this new and interesting region, I doubt not that, with unabated zeal, he will be able to add much that is new and important to the different branches of natural history. To facilitate his labors in the way of making collections, the Institution furnished him with a full set of articles necessary for the most efficient prosecution of a work of this kind.

Exchanges.—The system of exchanges still continues to render important aid to the literary and scientific intercourse of this country with other parts of the world. It is not confined on this side of the Atlantic to the United States, but extends to Canada, the West Indies, and South America. From the tabular statement given by Professor Baird, it appears that during the year 1862 there have been sent to foreign countries 1,203 packages, each containing, in most cases, a number of separate articles. These packages were enclosed in 114 boxes, measuring in the aggregate upwards of 1,000 cubic feet, and weighing 28,936 pounds. The number of packages received in return for parties in this country, exclusive of those for the Institution, was 2,105, which would on an average amount to upwards of 10,000 articles.

The Institution has received on its own account 5,035 articles, including those for the museum as well as for the library.

The thanks of the Institution continue to be due to various parties for their liberality in transporting boxes and packages free of charge, or in materially reducing the ordinary expenses. Those claiming especial mention are the North German Lloyds, between Bremen and New York, the Hamburg and New York Steamship Line, the Cunard Line, the Panama Railroad, and the Pacific Mail Steamship Company, all of which transport Smithsonian packages free of charge. The Adams's Express Line transmits our packages partly at reduced rates and partly free. The magnitude of the favors conferred by these companies may be readily understood by a reference to the statement made above that the weight of the packages sent to Europe alone amounted, during the year 1862, to nearly 30,000 pounds.

Library.—The library during the last year has continued steadily to increase, principally by exchanges, but also by purchase. By exchanges there have been received 1,211 octavos, 348 quartos, and 52 folios. Of parts of volumes in octavo 2 441, in quarto, 767, in folio, 161, maps and charts, 55, making a total of 5,035 articles. In addition to these about 500 volumes have been purchased.

A catalogue of transactions and proceedings of learned societies contained in the library of the Smithsonian Institution was published in July, 1858, and widely distributed, with a circular requesting that the deficiencies in the sets might be supplied, and other series be added to the collection from the duplicates in foreign libraries. This request has been so liberally complied with, and so many additions have been made to the collection, that a new edition of the catalogue has become necessary. This work is nearly ready for the press, and if the means of the Institution should permit, will be published during the coming year.

The value of this library will be much enhanced by the publication of the Systematic Index to all the articles contained in the transactions and proceedings of the learned societies of the world, now in course of preparation under the supervision of a committee of the Royal Society of London. This index will include the titles of papers published by the academies of Russia, Sweden, Denmark, Netherlands, Germany, Switzerland, France, Spain, Italy, and the States of North America. Some idea of the magnitude of the work, says General Sabine, may be formed from the fact, that it begins with the year 1800, and is brought down to the close of the year 1860. The titles are all in quadruplicate and now form sixty-two manuscript volumes. It is expected that the index will be completed within the year 1863. and that it will be published without unnecessary delay. It will be a work of immense importance to all engaged in scientific pursuits; it is difficult to estimate the amount of waste of time and labor of the student, arising from ignorance of what has already been achieved in the several departments of science; and none but one who has endeavored in the investigation of perhaps a single subject to explore the contents of scientific periodicals can judge of the weariness and discouragements of the search. A copy of this work will undoubtedly find a place in each of the principal libraries of the United States, and with the distribution of the catalogue before-mentioned will give the American student ready reference and access through the Smithsonian collection to all the important original papers on scientific subjects which have been published during the present century.

Gallery of Art.—The only additions made to the Gallery of Art during the past year, have been a bust of Professor Benjamin Silliman, senior, presented by his son, and another of General William H. Sumner, presented by George Wood, esq., of this city.

The Indian gallery belonging to Mr. Stanley still continues on deposit in the Institution. It is to be feared that by reason of the present condition of the country Congress will not think it advisable to purchase these characteristic illustrations of the aboriginal inhabitants of this continent, and it may perhaps become a subject of consideration with the Regents to make some provision for the preservation of the collection in its integrity, since it is possible that the owner may otherwise be obliged to dispose of it in parts, in order to meet his private pecuniary engagements.

Lectures.—On account of the uncertainty of the times and the preoccupation of the public mind no arrangements were made to furnish a course of public lectures on the part of the Institution for the winter of 1861–'62; but the use of the lecture-room was granted, in accordance with previous custom, to an association consisting principally of persons connected with the several departments of government to give a course of lectures in aid of a benevolent object. The privilege was granted, as usual, on the condition definitely expressed, that subjects of sectarianism in religion and special politics should not be discussed. But in this case experience proved that it is injudicious to allow the use of the room in times of great public excitement for lectures over which the Institution has no immediate control. The association could not, or at least did not, observe the restriction as to subjects, and the whole course became an exposition of political principles which were then under public discussion both in the papers of the day and on the floor of Congress. The evil of this was soon manifest in acrimonious attacks upon the Institution by members of Congress and editors of papers holding different political opinions. It was in vain to attempt to neutralize the effects of these attacks by stating the fact that the Institution ought not to be held responsible for the character of these lectures; the public could not be made to recognize the distinction between the lectures given under the immediate sanction of the Institution and those which were permitted to be delivered in the lecture-room under the direction of other parties.

Upon these considerations and those mentioned in the last report as to the inexpediency of frequently opening the Smithsonian building at night in the present state of this city, I concluded, after the course of political lectures was terminated, to restrict the use of the lecture-room exclusively to the lectures given under the immediate auspices of the Institution. This rule at first gave offence to some of the friends of the Institution, and was considered very unjust by another association which desired to give a course of political lectures in opposition to those which had been previously delivered. It has since, however, been generally approved by the reflecting public, and, indeed, must commend itself to all who have studied the history of establishments under the direction of State or national governments. Unless they are strictly guarded against the intrusion of political influence, their permanency and usefulness cannot long be maintained. So much were the Regents composing the Board, at its first sessions, impressed with this fact, that, at one of their early meetings, they unanimously adopted the following suggestion of one of their committees, namely:

"The party politics of the day, on which men differ so widely and so warmly, should not, your committee think, enter among the subjects treated of in any lecture or publication put forth under the sanction of the Institution. And they would deeply regret to see party tests and party wranglings obtrude themselves on the neutral grounds of science and education, endangering, as such intrusion surely would, the tranquillity of the Institution, disturbing the even tenor of its action, perhaps assaulting its welfare, and certainly contracting the sphere of its usefulness."

I need not say to the gentlemen now present, some of whom have been Regents since the beginning, how strictly the spirit of this resolution has been observed; notwithstanding the members of the Board from the two Houses of Congress are designedly elected from those holding opposite political opinions, in this hall the irritations of legislative discussion have been allayed or forgotten, and men of the most extreme political views have constantly met in this place as on a common ground of friendly sympathy, actuated apparently by no other feeling than the desire to guide and sustain the Institution in its mission of advancing and diffusing knowledge.

To make up for the dissatisfaction which might be felt on account of the restriction which had been put upon the use of the lecture-room, it was thought advisable to give in the winter of 1862–'63 a more extended series of lectures than had been given in the two preceding winters, but to confine them principally to courses on scientific and other subjects, which might be of service to those who desired actual instruction rather than mere amusement; although the attendance was not as large as in the case of the single lectures or the more exciting topics of the day, yet it was well maintained and comprised an attentive and decorous audience.

The following are the courses which were delivered, namely:

A course of six lectures, by Professor Daniel Wilson, of the University of Toronto, on "Unwritten History," embracing, 1st, Archæology; 2d, Physiology; 3d, The Lettered Races; 4th, The Maritime Races; 5th, The Origin of Civilization, and 6th, The Historic and Unhistoric Races.

A course of six lectures, by Professor Arnold Guyot, of Princeton College, on "The Unity of Plan in the System of Life, as exhibited in the characteristic ideas and mutual relations of the great groups of the vegetable and animal kingdom."

A course of five lectures, by Professor E. N. Horsford, of the Lawrence Scientific School, Cambridge, on "Munitions of War."

A course of six lectures, by Professor John Torrey, of New York, on "Artificial Light."

A course of four lectures, by Professor Henry Wurtz, of New York, on "Gunpowder."

Two lectures, by Dr. Solger, on "Ethnology."

One lecture by Arthur W. Edwards, on "The Microscope and its Revelations."

The courses of lectures by Professors Wilson, Guyot, and Wurtz will be published in the appendix to the report for 1862, and subsequent years.

Respectfully submitted.

JOSEPH HENRY,
Secretary Smithsonian Institution.

FEBRUARY, 1863.

APPENDIX TO THE REPORT OF THE SECRETARY.

Smithsonian Institution,
Washington, December 31, 1862.

Sir: I beg leave herewith to present a report, for 1862, of the operations intrusted to my charge, namely, those relating to printing, exchanges, and natural history.

Very respectfully, your obedient servant,

SPENCER F. BAIRD,
Assistant Secretary Smithsonian Institution.

Prof Joseph Henry, L.L.D.,
Secretary Smithsonian Institution.

PRINTING.

In an appendix will be found a list of the works published in 1862—the number of pages printed, quarto and octavo, greatly exceeding that reported for 1861. Several large works are also in press and will be issued early in 1863.

The following enumeration will show the number of pages printed during the year:

Works completed and issued, quarto .... pages..	1,390	
octavo .... pages..	1,226	
		2,616
Works still in press, quarto .... pages..	180	
octavo .... pages..	593	
		773
Total		3,389

A complete catalogue of all works published by the Institution up to June, 1862, was issued during the year, and will be found included in the list of articles published.

All the octavo publications of the Institution hitherto issued, and of which copies enough were on hand, have been arranged in a regular series, entitled "Smithsonian Miscellaneous Collections," corresponding to the "Smithsonian Contributions to Knowledge," and fill four volumes of over seven hundred pages each. An accompanying list shows the contents of these several volumes.

EXCHANGES AND TRANSPORTATION.

The operations in the department of exchanges, as reported for 1861, showed a considerable reduction below those of the previous year. During 1862, however, they have fully recovered their usual magnitude, and while the receipts of books and parcels from abroad are nearly equal to the maximum of any previous year, the transmissions have been greater than usual.

The thanks of the Institution and its correspondents continue to be due to various companies for their great liberality in transporting boxes and parcels free of charge over their respective routes, thereby reducing very materially the expenses of the department of exchanges. The lines deserving of especial mention are the North German Lloyd, between Bremen and New York; the Hamburg and New York steamship line; the Cunard lines, running from New York; the Panama Railroad Company; the Pacific Mail Steamship Company; the Adams's Express Company; the Hudson Bay Company, &c. The magnitude of the favors thus conferred may be readily understood by stating that a single transmission of packages to Germany during the year, through the North German Lloyd, consisted of 46 boxes of 424 cubic feet, or over ten tons by measurement.

The Institution is under many obligations for important services rendered in connexion with its exchanges and transportation, to the Hon. Hiram Barney, collector of customs in New York, and his assistant, Mr. George Hillier. Also in San Francisco, to Mr. A. B. Forbes, and Mr. Samuel Hubbard, of the Pacific Mail Steamship Company's service. The foreign agents of the Institution, Dr. Felix Flügel, of Leipsic; Gustave Bossange & Co., Paris, and Mr. William Wesley, 2 Queen's Head Passage, Paternoster Row, London, continue to discharge their duties with promptness and efficiency.

The following tables exhibit the statistics of the different branches of the system of exchanges:

A.

Receipt of books, &c., by exchange in 1862.

Volumes:		
Octavo	1,211	
Quarto	348	
Folio	52	
		1,611
Parts of volumes and pamphlets:		
Octavo	2,441	
Quarto	767	
Folio	161	
		3,369
Maps and charts		55
Total		5,035
Receipts in 1861		2,886
Excess in 1862		2,149

B.

Table showing the statistics of the exchanges of the Smithsonian Institution in 1862.

Agent and country.	Number of addresses.	Number of packages.	Number of boxes.	Bulk of boxes in cubic feet.	Weight of boxes in pounds.
Dr. F. FLÜGEL, *Leipsic*—					
Scandinavia	1	1			
Sweden	14	22			
Norway	6	11			
Denmark	13	23			
Russia	37	60			
Holland	40	52			
Germany	262	346			
Switzerland	31	47			
Belgium	16	23			
Total	420	585	46	424	12,780
G. BOSSANGE & Co., *Paris*—					
France	114	152			
Italy	64	86			
Spain	7	14			
Portugal	4	5			
Total	189	257	20	183	5,356
W. WESLEY, *London*—					
Great Britain and Ireland	166	259	16	159	4,700
Rest of the world	71	102	32	240	6,000
Grand total	846	1,203	114	1,006	28,836

C.

Addressed packages received by the Smithsonian Institution from parties in America for foreign distribution in 1862.

	Number of packages.
Albany, N. Y.—	
New York State Agricultural Society	28
Boston, Mass.—	
American Academy of Arts and Sciences	106
Massachusetts Historical Society	1
Massachusetts School for Idiotic and Feeble-minded Youth	3
Dr. C. T. Jackson	1

Cambridge, Mass.—	
Harvard College	12
Prof. L. Agassiz	50
Prof. Asa Gray	30
Prof. J. Marcou	3
Cleveland, Ohio—	
Dr. J. S. Newberry	73
Columbus, Ohio—	
Ohio State Board of Agriculture	126
Leo Lesquereaux	15
Dorchester, Mass.—	
Dr. Jarvis	20
Georgetown, D. C—	
Dr. L. Mackall	10
Leon, N. Y.—	
T. A. Cheney	100
Madison, Wisc.—	
Historical Society of Wisconsin	2
State of Wisconsin	73
Montpelier, Vt.—	
State Library of Vermont	24
Montreal, Can.—	
Prof. J. W. Dawson	10
New Haven, Conn.—	
American Journal of Science	35
Prof. J. D. Dana	15
New York.—	
New York Lyceum of Natural History	82
Philadelphia, Pa.—	
Academy of Natural Sciences	190
American Philosophical Society	196
Entomological Society of Philadelphia	12
Pennsylvania Institute for the Blind	36
Dr. Isaac Lea	20
Franklin Peale	3
A. F. Ward	25
William Sharswood	20
Providence, R. I.—	
State of Rhode Island	8
St. Louis, Mo.—	
St. Louis Academy of Sciences	8

Toronto, Can.—	
Canadian Institute	4
Washington, D. C.—	
Topographical Bureau	228
United States Coast Survey	50
United States Senate	250
Gen. A. A. Humphreys	75
Total	1,944

D.

Addressed packages received by the Smithsonian Institution from Europe for distribution in America, in 1862.

	Number of packages.
Albany, N. Y.	
Albany Institute	3
Dudley Observatory	9
New York State Agricultural Society	28
New York State Library	21
New York State Medical Society	1
New York State University	2
Professor James Hall	14
Amherst, Mass.	
Amherst College	6
Annapolis, Md.	
State Library	5
Ann Arbor, Mich.	
Detroit Observatory	4
Dr. Brunnow	7
Mr. Watson	1
Augusta, Me.	
State Library	4
Baltimore, Md.	
Maryland Historical Society	7
Peabody Institute	1
Dr J. O. Morris	2
Mr. Uhler	2
Blackwell's Island, N. Y.	
New York City Lunatic Asylum	1
Bogota, New Granada.	
Sociedad de Naturalistas Neo Granadinos	2
Boston, Mass.	
American Acad'y of Arts and Sciences	114
American Statistical Association	3
Boston Society of Natural History	76
Bowditch Library	3
Historic Genealogical Society	1
Massachusetts Historical Society	1
Prison Discipline Society	1
Public Library	8
State Library	6
Boston Journal of Natural History	1
North American Review	3
F. Alger	1
Dr. Brewer	3
A. A. Gould	2
Dr. E. Jarvis	1
Dr. Wilson	1
Brattleboro', Vt.	
Vermont Asylum for Insane	1
Brunswick, Me.	
Bowdoin College	7
Historical Society of Maine	1
Mrs. Parker Cleaveland	1
Burlington, Iowa.	
Historical and Genealogical Institute	1

D.—*Addressed packages received by the Smithsonian Institution, &c.*—Continued.

	Number of packages.
Burlington, Vt.	
University of Vermont	4
Cambridge, Mass.	
American Association for the Advancement of Science	32
Cambridge Observatory	17
Harvard College	15
Editor of Astronomical Journal	5
Professor L. Agassiz	39
G. P. Bond	4
Professor H. J. Clarke	3
Dr. B. A. Gould	14
Professor Asa Gray	13
Th. Lyman	1
Professor Jules Marcou	3
C. E. Norton	1
Professor B. Peirce	8
E. Schubert	1
J. E. Worcester	2
Dr. J. Wyman	3
Chicago, Ill.	
Academy of Sciences	2
Mechanics' Institute	3
Col. J. D. Graham, U. S. A	7
Chuquisaca, Bolivia.	
University of Chuquisaca	1
Cincinnati, Ohio.	
Astronomical Observatory	1
Historical and Philosophical Society of Ohio	1
Mercantile Library	4
Observatory	3
Western Academy of Science	1
Editor of the Dental Register of the West	1
Professor Mitchell	3
Clinton, N. Y.	
Dr. C. H. F. Peters	3
Columbia, Mo.	
Geological Survey of Missouri	5
Columbia, Pa.	
Professor S. S. Haldeman	2
Dr. Melsheimer	1
Columbus, Ohio.	
Ohio State Board of Agriculture	53
State Library	5
Leo Lesquereaux	2
W. Sullivant	1
Concord, N. H.	
New Hampshire Asylum for Insane	1
New Hampshire Historical Society	1
State Library	6
Davenport, Iowa.	
Right Rev. Henry W. Lee	1
Des Moines, Iowa.	
State Library	20
Detroit, Mich.	
Michigan State Agricultural Society	20
Dorchester, Mass.	
Dr. E. Jarvis	4
Easton, Pa.	
Dr. B. Clemens	1
Professor J. H. Coffin	1
Erie, Pa.	
L. H. Olmsted	1
Fall River, Mass.	
Niels Arnzen	1
Farmington, Conn.	
Edw. Norton	3
Frankfort, Ky.	
Geological Survey of Kentucky	8
State Library	5
Mr. Shaffner	1
Gambier, Ohio.	
Kenyon College	7

D.—*Addressed packages received by the Smithsonian Institution, &c.*—Continued.

	Number of packages.
Georgetown, D. C.	
Georgetown College	12
Dr. A. Schott	2
Hanover, N. H.	
Dartmouth College	6
Harrisburg, Pa.	
Pennsylvania State Lunatic Hospital	1
State Library	7
Hartford, Conn.	
Hartford Society of Sciences	4
Historical Society of Connecticut	1
State Library	5
Young Men's Institute	3
Havana, Cuba.	
Observatoire Physique et Météorologique	1
Real Sociedad Economica	1
Hudson, Ohio.	
Western Reserve College	2
Indianapolis, Ind.	
Indiana Historical Society	1
State Library	5
Iowa City, Iowa.	
State of Iowa	9
State University of Iowa	33
Janesville, Wis.	
State Institution for the Blind	2
Jefferson City, Mo.	
Historical Society of Missouri	1
State Library	3
Kalamazoo, Mich.	
Asylum for Insane	1
Lancaster, Pa.	
Thomas C. Porter	2

	Number of packages.
Lansing, Mich.	
Agricultural College	1
State Library	5
Lecompton, Kansas.	
State Library	3
Leon, N. Y.	
T. Apoleon Cheney	1
Louisville, Ky.	
Colonel Long	2
Professor J. Lawrence Smith	1
Madison, Wis.	
Historical Society of Wisconsin	9
Skandinaviske Presse-Forening	1
State Library	6
Wisconsin State Agricultural Society	30
Montpelier, Vt.	
State Library	6
Montreal, Canada.	
Natural History Society	3
Dr. Smallwood	1
Nantucket, Mass.	
Miss Maria Mitchell	2
New Brunswick, N. J.	
Professor G. H. Cook	2
New Haven, Conn.	
American Journal of Science	25
American Oriental Society	20
Yale College	6
W. P. Blake	3
G. J. Brush	3
Professor J. D. Dana	34
Professor E. Loomis	10
Professor Silliman	21
Professor W. D. Whitney	4
New York, N. Y.	
American Agriculturalist	2
American Ethnological Society	2

D.—*Addressed packages received by the Smithsonian Institution, &c.*—Continued.

	Number of packages.
New York, N. Y.—Continued.	
American Geographical and Statistical Society	30
American Institute	8
Astor Library	4
Mercantile Library Association	3
New York Historical Society	1
New York Lyceum of Natural History	65
University of the City of New York	8
New York Dental Journal	2
Farmer and Mechanic	2
New York Journal of Pharmacy	2
G. Boyd	1
Charles L. Brace	1
F. Delafield	1
Dr. Draper	1
Dr. D. C. Eaton	3
Thomas Egleston	13
Professor Wolcott Gibbs	1
S. H. Grant	1
H. Grinnell	2
Mr Harlan	2
Charles B. Norton	3
Baron Osten-Sacken	7
Professor Redfield	1
Professor Wynne	1
Norwich, Conn.	
Mr. Rockwell	1
Olympia, Wash. Ter.	
Territorial Library	3
Omaha, Nebraska.	
Territorial Library	3
Oswego, N. Y.	
Mr. Pumpelly	3
Philadelphia, Pa.	
Academy of Natural Sciences	142
American Philosophical Society	77
Central High School of Philadelphia	3
Franklin Institute	4
Geological Society of Pennsylvania	2
Geological Survey of Pennsylvania	1
Historical Society of Pennsylvania	5
Mercantile Library Association	1
Pennsylvania Hospital for the Insane	1
Pennsylvania Institute for the Blind	1
Wagner Free Institute	10

	Number of packages.
Philadelphia, Pa.—Continued.	
American Journal of Dental Science	1
Dental Cosmos	2
John Cassin	1
F. Clay	1
Dr. E. D. Cope	1
E. Durand	2
Dr. Hermann	1
H. S. Tanner	2
Professor F. L. Otto Koehrig	1
Dr. Isaac Lea	13
Dr. John Leconte	8
Dr. Joseph Leidy	14
George Ord	1
F. Peale	1
William Sharswood	5
Mrs. A. E. Thomas	1
Professor Wagner	1
Mr. Wetherell	1
Portland, Me.	
Neal Dow	1
Princeton, N. J.	
College of New Jersey	2
Professor A. Guyot	1
Providence, R. I.	
Brown University	1
Rhode Island Historical Society	1
State Library	4
Rio Janeiro, Brazil.	
Instituto Historico Geographico Brasiliero	2
Mr. Liais	1
Rochester, N. Y.	
Hon. Lewis H. Morgan	1
Rock Island, Ill.	
Dr. John Rittles	1
Sacramento, Cal.	
State Library	5
Salem, Mass.	
Essex Institute	2

D.—*Addressed packages received by the Smithsonian Institution, &c.*—Continued.

	Number of packages.
San Francisco, Cal.	
California Academy of Natural Sciences	31
Geological Survey of California	1
St. Louis, Mo.	
Deutsche Inst. für Bef. von Wiss. Kunst und Gewerbe	3
Geological Survey of Missouri	2
St. Louis Academy of Sciences	105
St. Louis University	5
Dr. George Bernays	1
Dr. George Engelmann	8
Dr. Hammer	1
Dr. Haven	1
Dr. Albert C. Koch	2
Dr. B. F. Shumard	7
G. C. Swallow	2
Santiago, Chile.	
Observatory	1
University of Chile	9
Springfield, Ill.	
State Library	5
Rev. L. P. Esbjörn	2
Stockton, Cal.	
Asylum for Insane	1
Toronto, Canada West.	
Board of Agriculture of Upper Canada	1
Canadian Institute	1
Trenton, N. J.	
Geological Survey of New Jersey	5
State Library	5
Utica, N. Y.	
American Journal of Insanity	3
New York State Lunatic Asylum	1
Valparaiso, Chile.	
Dr. T. A. Reid	1

	Number of packages.
Washington, D. C.	
Bureau of Ordnance and Hydrography	4
Library of Congress	9
National Observatory	87
Surgeon General U. S. Army	1
Topographical Bureau	1
Trigonometrical Survey	1
U. S. Coast Survey	35
U. S. Patent Office	133
War Department	3
Colonel Abert	2
Professor A. D. Bache	34
Professor S. F. Baird	6
W. D. Breckenridge	1
Major Emory	1
J. Ferguson	2
T. Gill	1
Captain J. M. Gilliss, U. S. N	37
Captain H. J. Hartstene, U. S. N	1
T. Hitz	1
Professor W. E. Jillson	1
Professor W. R. Johnson	1
J. C. G. Kennedy	3
Lieutenant S. P. Lee	2
F. B. Meek	3
Dr. J. S. Newberry	1
G. W. Riggs	2
H. R. Schoolcraft	5
Hon. W. H. Seward	1
W. Smyth	1
Prof. W. Stimpson	9
Hon. Charles Sumner	1
H. Ulke	2
Waterville, Me.	
Waterville College	2
Westchester, Pa.	
Dr. W. Darlington	1
West Point, N. Y.	
Professor Bailey	1
Worcester, Mass.	
American Antiquarian Society	2
State Lunatic Hospital	1
York, Pa.	
Rev. Mr. Zeigler	1

Total of addresses	273
Total of parcels	2,111

MUSEUM AND COLLECTIONS.

As might have been expected, the receipts of specimens of natural history during the year past have been materially curtailed. The entire number of donations in 1862 amounted to 124. The number for 1861 was 157, while that for 1860 was 404. Much of interest has, however, been received from different quarters, and the aggregate would have been more considerable if all the collections made in the Hudson Bay region by Mr. Kennicott, and other gentlemen, had not been kept back in consequence of the Indian outbreaks in Minnesota during the year.

Mr. Kennicott spent the spring and summer of 1861 at Fort Yukon, on the Yukon river, making large collections of specimens. He wintered on the Rocky mountains, at La Pierre's house, and made a visit to Fort Simpson early in the spring. News received there from home determined him to return to the United States, and he reached Fort Garry the beginning of September, arriving at Chicago in October, after an absence of three years and a half. A detailed report of his expedition will be prepared by himself, and submitted hereafter.

The gentlemen of the Hudson's Bay Company, of whom mention has heretofore been made as aiding him in every way, have continued their kind offices. To Governor Dallas, present governor of the Hudson's Bay Company, Governor Mactavish, Mr. Bernard R. Ross, Mr. Lawrence Clark, Mr. R. Mac Farlane, Mr. W. L. Hardisty, Mr. James Lockhart, Mr. C. P. Gaudet, Mr. James Flett, and others, the thanks of the Institution are very especially due for such aid to Mr. Kennicott as ensured the success of his expedition, and without which he could have accomplished little or nothing.

To most of the gentlemen above referred to acknowledgments have been made in previous reports for valuable contributions of specimens. Collections made by them in 1862, having been packed with those of Mr. Kennicott, have not yet arrived, but are expected early in the year 1863, as, at the last advices, they had reached St. Paul.

Mr. John Xantus, so well known on account of his scientific researches in California, in connexion with the Institution, has just entered into a new and entirely unexplored field, promising the most important results. Having been appointed United States consul at Manzanillo, he left New York on the 11th of December, and is doubtless now at his post. With their usual liberality, exercised so often before in the interest of science and the Smithsonian Institution, the Panama Railroad Company and the Pacific Mail Steamship Company gave free passage to him and his extensive outfit.

IDENTIFICATION OF SPECIMENS.

Much progress has been made by the various gentlemen mentioned on page 62 of the last report in identifying and labelling the collections of the Institution. It will not be long before the greater portion will be thoroughly worked p, and a general distribution of duplicates accomplished No new collections of any importance have been given out, although Mr. Cope expects to take up the North American saurians belonging to the Institution with the view of preparing a report on the subject.

Dr. Allen having completed the examination of the American bats intrusted to him, as far as his professional engagements would allow, has returned the specimens, and deposited a report on the subject, nearly finished, and to be hereafter completed by him. Dr. Wood has also returned a portion of the collection of Myriapoda. Dr. Slack has sent back the collection of monkeys, with a catalogue of the collection. Mr. George N. Lawrence has labelled the entire collection of humming birds, amounting to over 150 species. Professor Agassiz has determined and returned the collection of Echini.

Mr. Gill has continued his examinations of the fishes of Cape St. Lucas, collected by Mr. Xantus, and published many new species from them.

DISTRIBUTION OF SPECIMENS.

The distribution of duplicate specimens has been continued as fully as time and opportunity would allow. Several large collections of mammals, birds, and eggs have been sent off, together with a considerable number of sets of marine shells. Dr. Foreman has been engaged for some time in making up the duplicate Unionidae into series, of which about twenty sets will soon be ready.

WORK DONE IN THE MUSEUM.

The mounted mammals and birds which occupied the Museum Hall at the beginning of the year have all been identified, and to most of them neatly written labels have been affixed. This portion of the work will be completed as soon as the clerk assigned to the duty can accomplish it. All the specimens have been carefully examined, and those attacked by insects have been removed and baked or exposed to the vapor of benzine. A few skins have been mounted of species possessing a particular interest.

The collection of skulls has been cleaned and rearranged in the southeast gallery. The series of rocks, and in part that of minerals, have been placed on their proper shelves. Nothing further has been done with the shells, the collections in this department not having been returned by the gentlemen having them in charge.

All the miscellaneous boxes of specimens in the Institution have been unpacked and their contents assorted and distributed. All the crude material in the building has thus been put into condition for examination, and given out to investigators for arrangement, with the exception of a portion of the fossils and the general ethnological collection. These will, however, all be unpacked and examined in the ensuing year.

The progress of work upon the collections already in the Institution at the beginning of the year was much interfered with by the necessity of receiving a large number of specimens formerly belonging to the National Institute, and sent from the Patent Office (where they had been stored) by the Commissioner of Patents. Much of this material was in an exceedingly damaged condition, requiring instant attention and much labor to preserve it in even tolerable condition.

The cataloguing of specimens in the record books of the Institution has been carried forward during the year by the insertion of nearly 10,000 additional entries, many of them covering each a number of specimens. The present aggregate of entries is about 75,000, embracing at least 500,000 or half a million of specimens. When it is remembered that none of the plants, and insects, and but few of the fishes and invertebrates, have yet been recorded in this way, some estimate may be formed of the extent and value of the material for research in charge of the Smithsonian Institution.

Table showing the total number of entries on the record books of the Smithsonian collection at the end of the years 1861 *and* 1862.

	1861.	1862.
Skeletons and skulls	4,459	4,750
Mammals	5,550	5.900
Birds	23.510	26.157
Reptiles	6,088	6.311
Fishes	3,643	4,925
Eggs of birds	4,830	6,000
Crustaceans	1,287	1,287
Mollusks	9,718	10,000
Radiates	1,800	2,675
Fossils	1,031	2.100
Minerals	3,500	3,725
Ethnological specimens	550	825
Annelids	105	109
Total	66,075	74,764

LIST OF DONATIONS TO THE MUSEUM IN 1862.

Agassiz, Professor L.—Skins and eggs of birds from Labrador: specimens of *Haemulon.*

Akhurst, James.—Bat and crustaceans from South America.

Barnston, George.—Birds, fishes, &c., from Lake Superior.

Beadle, Rev. E. R.—Box of minerals.

Beitel, C. G.—Minerals and furnace slags from Pennsylvania.

Berthoud, Dr. E. L.—Minerals, fossils, and skin of *Lagopus leucurus.* Pike's Peak.

Bertolet, Dr. P. G.—Sections of wood of trees of Pennsylvania.

Bickmore, A. J.—Living *Phæton flavicauda,* from Bermuda.

Blackman, Mr.—Series of specimens of prairie chicken or pinnated grouse, from youngest chick to adult. Also skins of other birds, insects, eggs, &c., from Illinois.

Bland, Thomas.—West Indian shells.

Brandt, H.—Nests and eggs from Fort Riley.

Brush, G. J.—Box of minerals.

Buckner, Dr.—Set of models of edible fungi, prepared at Hildburghausen.

Cardeza, Dr.—Series of choice minerals from Pennsylvania.

Carlton, U. S. A., General J. H.—Skull of rattlesnake, from New Mexico.

Carothers, Rev. A. G.—Collection of reptiles from Martinique.

Chapman, N. H.—Box of birds' nests, from Ohio.

Cochran, T. G.—Eggs of Night Heron.

Cook, Prof. George H.—Fossils of New Jersey.

Cooper, Wm.—Fossils from the Isthmus of Panama. Cast of *Megalonyx* bones.

Couper, W.—Skins of *Aegiothus,* from Canada.

Cowles, P. M.—Insects from Cumberland Gap.

Dawson, Prof. J. W.—Postpliocene fossils, from Canada.

Dow, Captain J. M.—Mammals, birds, and other animals, from Nicaragua. Skins of birds, and various alcoholic specimens, collected at sea between New York and Panama, *via* Cape Horn.

Eaton, Dr. H. K.—Specimens of *Gryllotalpa.*
Edwards, A. M.—Mounted infusoria from Washington Territory.
Eglston, Thos.—Minerals from Massachusetts.
Elliot, D. G.—Four skins of European birds.
Feilner, Lieut. John.—Mammals, birds, &c., from Fort Crook, California.
Foreman, Dr. E.—Fossil tracks of animals from Maryland.
Forns, R. M.—Two skins of *Chordeiles minor,* Cuba.
Geoffroy, Mons.—Collections of birds from Bogota.
Gibbs, George.—Shells and fossils from Washington Territory
North American antiquities. (Deposited.)
Gilliss, U. S. N., Captain J. M.—Package of poisoned arrows brought from the Amazon by Lieutenant Herndon; Coral from Agatha's Bank.
Goss, B. F.—Nests and eggs of birds, from Kansas.
Gundlach, Dr. J.—Box of Cuban birds.
Gurley, R. R.—Golden eagle in the flesh, shot near Washington.
Habel, Dr. H.—Minerals from New York Island.
Hayden, Dr. F. V.—Fossils from Maryland and New Jersey.
Hayes, Dr. I. I.—Esquimaux dresses and other curiosities; Birds, mammals, and rocks, from North Greenland.
Heermann, Dr. A. L.—130 species and 250 specimens of Humming birds; *Vireo barbatula,* from Florida.
Hepburn, James.—Birds' eggs, from Pacific coast of North America
Hubbard, Samuel.—Thirty species of Californian fishes.
Hunt, U. S. A., Captain E. B.—*Hippa,* from Key West.
Kennicott, R.—Box of eggs and skins of mammals and birds from the Yukon river.
Lawrence, George N.—*Hylophilus,* from Guatemala; Mounted *Macrorhamphus scolopaceus.*
Lea, Isaac.—Box of *Unionidæ.*
Leonard, J.—Minerals from the Rocky Mountains.
Lewis, James—Large collection of land and fresh-water shells of United States, including selected series of *Paludina.*
Lewis, Joseph S.—Coleopterous insects, from United States.
Loweree, T., per *J. J. Major.*—*Pityophis,* from Guadalaxara.
M'Kenzie, J.—Birds from Moose Factory.
Major, J. J.—Eggs of *Pandion,* from the Similkameen.
March, W. T.—Series of birds of Jamaica.
Matthews, G. F.—Fossil shells of New Brunswick.
Meek, F. B.—Fossils from Maryland and New Jersey.
Mendenhall, Kirk.—Eggs of birds and fossils of Indiana.
Moses, Dr. S. G.—Living Duck-Hawk, hatched in Connecticut.
Newton, Alfred.—Thirty species of eggs of Arctic European birds.
New York; Panama R. R. Co.—Series of fossils from Isthmus of Panama.
Palmer, Dr. Edward.—Shells from Acapulco and San Diego.
Paris; Mus. d'Hist. Nat.—Series of serpents from various localities.
Parkinson, D. F.—Mammals, birds, &c., from North California.
Pease, W. H.—Shells from the Sandwich Islands.
Philadelphia Academy of Natural Sciences.—Specimens of *Rissa* and *Otus.*
Pierce, Warren.—*Asellus,* from Ohio.
Poey, Prof. F.—Fishes of Cuba.
Ried, Dr. A.—Skeletons, skulls, and antiquities of Patagonian and Araucanian Indians.
Reinhardt, Dr. J.—Box of European birds.
Ross, B. R.—Bale of large skins from Prince Rupert's Land.
Russell, Dr. W. H.—Seven species of Himalayan pheasants.

Samuels, E.—Series of infusorial slides.
Schenectady; Union College.—Box of minerals.
Schiffmann, R.—Insects from Missouri.
Schmid, Dr. H. E.—Reptiles, &c., in alcohol, from Japan; also living *Megalobatrachus*, or Giant Salamander.
Sclater, P. L.—Seventy-five species of Jamaican birds.
Scott, Ansel.—Indian relics from Bradford county, Pennsylvania.
Simpson, Jos.—Copper spear head and other relics.
Stone, C. J.—Box of minerals.
Swan, James G.—Skins of birds, crustaceans, shells, &c., Straits of Fuca.
Swift, D.—Minerals and Indian remains from Pennsylvania.
Thomsen, J. H.—Skeleton of sperm-whale porpoise, Nantucket; *Echinarachrus*, from New Bedford.
Tolman, J. W.—Birds' eggs from Illinois.
Totten, General Joseph G.—Coral from the Tortugas.
Totten, Colonel.—Fossils from line of Panama railroad.
Tryon, George W.—Type specimens of new shells; box of *Unionidæ*.
Van Cortlandt, Dr.—Fishes from the Ottawa river.
Washington; National Institute.—Four cartloads of miscellaneous specimens.
Whittlesey, E.—Coal from Ohio.
Williamson, Sergeant J. J., (company E, 2d New York artillery.)—Snowy owl, shot near Washington.
Willis, J. R.—Shells, fishes, birds, eggs, &c., of Nova Scotia.
Wingate, J. D.—Box of *Helices*, from Pennsylvania.
Wood, Dr. William.—Living Duck Hawk, from Connecticut; also skins of birds, eggs, and Indian remains.
Woodworth, Dr. J. M.—Shells, eggs, &c., from Iowa.
Wright, W. W.—Fossil wood, found five miles from Alexandria.
Wurdemann, W.—Shells of Florida, (deposited.)
Wyrick, David.—Bones of man and animals from ancient graves in Ohio.

LIST OF SMITHSONIAN PUBLICATIONS DURING 1862.

Discussion of the Magnetic and Meteorological Observations made at the Girard College Observatory, Philadelphia, in the years 1840—1845, by A. D. Bache, LL.D. First Section, Part II. Investigation of the Solar-Diurnal Variations of the Magnetic Declination and its Annual Inequality. June, 1862. 4to., pp. 28.

Discussion, &c. First Section, Part III. Investigation of the Lunar Effect on the Magnetic Declination. June, 1862. 4to., pp. 16.

Discussion of the Magnetic and Meteorological Observations, &c., continued. Second Section, comprising Parts IV, V, VI. Horizontal Force, Investigation of the Ten or Eleven Year Period and of the Disturbances of the Horizontal Component of the Magnetic Force; Investigation of the Solar-Diurnal Variation and of the Annual Inequality of the Horizontal Force; and of the Lunar Effect on the Same. November, 1862. 4to., pp. 76.

Monograph of the Diptera of North America. Prepared for the Smithsonian Institution by H. Loew. Part I. Edited, with additions, by R. Osten Sacken. April, 1862. 8vo., pp. 246, with fifteen wood-cuts and two plates.

Synopsis of the described Lepidoptera of North America. Part 1. Diurnal and Crepuscular Lepidoptera. Compiled for the Smithsonian Institution by John G. Morris. February, 1862. 8vo., pp. 376, and thirty wood-cuts.

Museum Miscellanea, or Aids to the Labelling, Cataloguing, and Recording of Specimens. 8vo., 1854—1862, pp. 48. This contains—

1. Abbreviations of names of States and Territories of North America, for labelling insects, shells, &c.
2. A series of small figures, from 1 to 1643.
3. A series of medium figures, from 1 to 2747.
4. A series of large figures, from 1 to 2599.
5. Blank check list of specimens.

Catalogue of Publications of the Smithsonian Institution, corrected to June, 1862. 8vo., pp. 52.

List of Foreign Institutions in correspondence with the Smithsonian Institution. 1863. 8vo., pp. 40.

Results of Meteorological Observations made under the Direction of the United States Patent Office and the Smithsonian Institution, from the year 1854 to 1859, inclusive; being a Report of the Commissioner of Patents to the Senate, at the first session of the 36th Congress. Vol. I. 1862. 4to., pp. 1270. (Vol. II is in press.)

Annual Report of the Board of Regents of the Smithsonian Institution, showing the operations, expenditures, and condition of the Institution for the year 1861. 8vo., pp. 464.

WORKS PARTLY PRINTED IN 1862, BUT NOT COMPLETED.

Meteorological Observations in the Arctic Seas. By Sir Francis Leopold M'Clintock, R. N. Made on board the Arctic searching yacht "Fox," in Baffin's Bay and Prince Regent's Inlet, in 1857, 1858, and 1859. Reduced and discussed by Charles A. Schott, Assistant U. S. Coast Survey. 4to., with one Map.

Notices of Ancient Mining on Lake Superior. By Charles Whittlesey. 4to.

Check List of Minerals, with their Symbols. Prepared for the Smithsonian Institution by T. Egleston, Jr. 8vo.

Synonymical List of the Coleoptera of North America, with Descriptions of New Species. By John L. Le Conte, M. D. 8vo.

Bibliography of American Conchology. By W. G. Binney. 8vo.

Synopsis of North American Air-breathing Shells. By W. G. Binney. 8vo.

Synopsis of North American Vivipara, &c. By W. G. Binney. 8vo.

WORKS CONTAINED IN THE FOUR VOLUMES OF MISCELLANEOUS COLLECTIONS.

Smithsonian Miscellaneous Collections. 1862. Vol. I, 8vo., pp. 732. Contains: (19.) Directions for Meteorological Observations; (87.) Coffin, Psychrometrical Tables; and (31.) Guyot, Meteorological and Physical Tables.

Smithsonian Miscellaneous Collections. 1862. Vol. II, 8vo pp. 692. Contains: (27.) Booth and Morfit, Recent Improvements in Chemical Arts; (115.) Proceedings Board Regents Smithsonian Institution in relation to the Electro-magnetic Telegraph; (53.) Stanley, Catalogue Portraits North American Indians; (108.) Baird, Catalogue of North American Birds; (49.) Baird and Girard, Catalogue of North American Reptiles—Serpents; (128.) Check List Shells, North America; (34.) Directions for Collecting; (137.) Circular to Officers H. B. Co.; (139.) Instructions for collecting Nests and Eggs; North American Grasshoppers; and North American Shells; (138.) Morgan, Circular respecting Relationship.

Smithsonian Miscellaneous Collections. Vol. III, 8vo., pp. 766. Contains: (102.) Osten Sacken, Catalogue Diptera North America; (118) Morris, Catalogue Described Lepidoptera North America; (136.) Le Conte, Classification Coleoptera, I; (117.) Catalogue Publications of Societies in Smithsonian Library.

Smithsonian Miscellaneous Collections. Vol. IV, 8vo, pp. 752. Contains: (134.) Hagen, Synopsis of North American Neuroptera, and (133.) Morris, Synopsis of North American Lepidoptera.

LIST OF METEOROLOGICAL STATIONS AND OBSERVERS

OF THE

SMITHSONIAN INSTITUTION

FOR THE YEAR 1862.

BRITISH AMERICA.

Name of observer.	Station.	North latitude.	West longitude.	Height.	Instruments.*	No. of months received.
		° ′	° ′	*Feet.*		
Acadia College	Wolfville, Nova Scotia	45 06	64 25	95	A.	10
Baker, J C	Stanbridge, Canada East	45 08	73 00	......	T.	12
Clarke, Lawrence, jr.	Fort Rae, Great Slave Lake	....	......	......	T.	9
Craigie, Dr. W	Hamilton, Canada West	43 15	79 57	......	B T	12
Delaney, Edward M. J.	Colonial Building, St. John's, Newfoundland.	47 35	52 40	170	B. T R.	7
Fleet, Andrew	Fort Norman, Hudson's Bay Territory.	64 00	124 00	200	T. R	5
Dickson, Walter	Little Whale River, Hudson's Bay Territory.	56 02	....	12	T.	11
Hall, Archibald, M. D.	Montreal, Canada East	45 30	73 36	57	A.	12
Hensley, Rev. J M Everett, Prof. J. D.	King's College, Windsor, Nova Scotia.	44 59	64 07	200	A.	3
Kirby, Rev. W. W	Fort Simpson, Hudson's Bay Territory.	61 51	121 25	......	T.	4
Magnetic Observatory	Toronto, Canada West	43 39	79 21	†108	A.	12
Mackenzie, John	Moose Factory, Hudson's Bay Territory.	51 15	80 45	......	B. T. R.	5
Phillips, H	Niagara, Canada West	43 09	79 20	270	A.	3
Rankin, Colin	Michipicoton, Canada West	47 50	85 05	....	T.	8
Richards, Thomas	Kenogumissee, Hudson's Bay Territory.	49 50	84 00	1,000	T.	7
Smallwood, Dr. Charles	St. Martin, Isle Jesus, Canada E.	45 32	73 36	118	A.	9

CALIFORNIA.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° ′	° ′	*Feet.*		
Ayres, W. O., M. D.	San Francisco.	San Francisco.	37 48	122 27	130	A.	12
Belcher, W. C	Marysville	Yuba	39 29	121 30	80	B. T. R.	12
Dunkum, Mrs. Elizabeth S.	Honcut.	Yuba	39 25	121 30	......	T. R.	12
Hays, W. W., M. D.	Presidio of San Francisco.	San Francisco	37 48	122 26	150	A.	3
Logan, Thomas M., M. D.	Sacramento.	Sacramento.	38 35	121 28	41	A.	12
Smith, M. D	Spanish Ranche	Plumas	39 56	120 40	3,700	B T. R.	3
Whitlock, James H	Meadow Valley	Plumas	40 20	120 15	3,700	B. T. R.	1

* A signifies Barometer, Thermometer, Psychrometer, and Rain Gauge.
B signifies Barometer.
T signifies Thermometer.
P signifies Psychrometer.
R signifies Rain Gauge.
N signifies No instrument.
† Above Lake Ontario.

COLORADO.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° '	° '	*Feet.*		
Ellis, Dr. Wm. T.	Mountain City.	Arapahoe	39 35	105 40	8,000	T.	1
Stanton, Frederick J	Denver City.	Arapahoe	...	...	...	T.	4

DISTRICT OF COLUMBIA.

MacKee, Rev. C. B.	Georgetown	Washington.	38 54	77 03	...	T. R	12
Smithsonian Institution	Washington	Washington	38 53	77 01	60	A.	12

CONNECTICUT.

Case, Jarvis	Canton	Hartford	42 00	73 00	700	T. R.	12
Harrison, Benjamin F	Wallingford	New Haven.	41 27	72 50	133	A.	7
Hunt, Rev. Daniel	Pomfret.	Windham	41 52	72 23	567	A.	12
Johnston, Prof. John	Middletown.	Middlesex	41 32	72 39	175	A.	12
Learned, Dwight W	Plymouth.	Litchfield	41 40	73 03	...	T.	7
Leavenworth, D. C	New Haven.	New Haven.	41 18	72 56	40	B. T.	3
Rockwell, Charlotte.	Colebrook.	Litchfield.	42 00	73 06	...	T.	12
Yeomans, William H	Columbia.	Tolland.	41 40	72 42	...	T.	12

DAKOTA.

Hill, G D. Lawson, George W Williams, Herbert G.	Yankton	...	42 51	97 31	...	...	4

FLORIDA.

Dennis, William C.	Key West.	Monroe.	24 33	81 28	16	B. T. R.	12
Ferguson, G. F. Oltmanns, J. G	Magnetic Observatory, Key West.	Monroe.	24 33	81 48	6	B. T. P.	5

ILLINOIS.

Aldrich, Verry	Tiskilwa	Bureau	41 15	89 66	550	N.	12
Babcock, E	Riley	McHenry	42 11	88 20	760	T. R	12
Bacon, E. E	Willow Creek.	Lee.	41 45	88 56	1,040	N.	12
Ballou, N E. M D.	Sandwich	De Kalb.	41 31	88 30	575	T R.	12
Bandelier, Adolphus F., jr.	Highland	Madison.	38 45	89 46	...	B. T. P.	11
Blanchard, Orestes A	Elmira	Stark	41 12	90 15	...	T. R.	5
Breed, M A.	Peoria	Peoria	40 38	89 46	512	T. R.	2
Brendel, Frederick, M.D	Peoria	Peoria	40 43	89 30	460	A.	12
Brookes, Samuel	Chicago	Cook	42 00	87 30	...	T.	12
Bryant, C. H	North Prairie.	Knox	41 08	90 06	...	N.	6
Byrne, Arthur M Roe, James H., and others	Chicago	Cook	...	...	...	B. T.	10
Cobleigh, Rev. Nels. E, D. D	Lebanon	St. Clair.	38 37	89 56	500	B. T. R.	6
Crandon, Frank.	Batavia	Kane	41 52	88 20	636	T.	1
Dudley, Timothy	Jacksonville.	Morgan	39 30	90 06	676	T. R.	12
Grant, John. Grant, Miss Ellen	Manchester	Scott	39 33	90 34	683	A.	12
Griffing, Henry	Willow Hill.	Jasper	39 00	88 00	...	N.	5
Little, J. Thomas	Dixon.	Lee	41 45	89 31	...	N.	7
Livingston, Prof. Wm.	Galesburg	Knox	...	...	...	A.	12
Mead, S. H, M. D.	Augusta.	Hancock	40 10	91 00	*203	T. P. R.	12
Meeker, Ralph E.	Dongola.	Union	37 26	89 21	...	T.	3
Merwin, Mrs. Emily H	Ottawa	La Salle	41 20	88 47	500	T. R.	12
O'Donoghue, John	Chicago	Cook	41 54	87 38	650	B. T.	2
Olds, Warren	Albany	Whiteside	41 40	90 16	600	N.	10
Patrick. Dr. John J Baker, Nathan T	Belleville	St. Clair.	38 29	90 06	600	B. T	12
Riblet, J. H.	Pekin	Tazewell	40 36	89 45	...	B. T. R.	12
Rogers, O. P. and J. S	Marengo	McHenry.	42 14	88 38	842	B. T. R.	12
Tolman, James W	Winnebago Depot.	Winnebago	42 17	89 12	900	B. T. R.	11

*Above low-water mark at Quincy.

INDIANA.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° ′	° ′	*Feet.*		
Anderson, Henry H	Rockville	Parke	36 00	87 00	1,100	N	11
Bullock, J T	Shelbyville	Shelby	39 00	87 00	... ...	N	11
Chappellsmith, John	New Harmony	Posey	38 08	87 50	320	A	12
Dawson, William	Cadiz	Henry	39 55	85 20	1,060	T.R	6
Dayton, James H	South Bend	St. Joseph	41 39	86 07	600	T.R	12
Haines, John	Richmond	Wayne	39 52	84 59	....	B.T	12
Larrabee, William H	Greencastle	Putnam	39 30	86 47	800	N	1
Rambo, Edward B	Richmond	Wayne	39 47	84 47	800	T.R	5
IOWA.							
Beal, Mrs. Celia	Grove Hill	Bremer	42 45	87 12	....	T	2
Belfield, H. H Dunwoody, Wm. P	Davenport	Scott	41 30	90 40	737	A	12
Collin, Prof. Alonzo	Mount Vernon	Linn	42 00	91 00	....	T	10
Deering, D S	Independence	Buchanan	....	....	....	T	2
Doyle, L. H	Waterloo	Black Hawk	42 30	92 31	....	N	12
Farnsworth, P. J., M. D	Lyons	Clinton	41 50	90 19	401	T.R	4
Foster, Suel	Muscatine	Muscatine	41 26	92 00	....	N	11
Horr. Asa, M. D	Dubuque	Dubuque	42 30	90 52	666	A	12
Hudson, A T., M. D	Lyons	Clinton	41 50	90 10	401	T R	8
Leonard, Wm. P	Kossuth	Des Moines	41 00	91 13	....	N	3
McConnel, Townsend	Pleasant Plain	Jefferson	41 07	94 54	950	T.R	12
McCoy, Franklin, M. D McCoy, Miss Elizabeth	Algona	Kossuth	43 01	94 04	1,500	T	12
McCready, Daniel	Fort Madison	Lee	40 37	91 23	....	T.R	12
Millard, Andrew J	Sioux City	Woodbury	42 33	96 27	1,258	T.R	12
Parvin. Prof. Theodore S	Iowa City	Johnson	....	....	....	A	12
Sheldon, Daniel	Forestville	Delaware	42 40	91 50	... ...	T	7
Ufford, Rev. John	Muscatine	Muscatine	41 25	91 02	586	A	12
Wheaton, Alex. Camp	Independence	Buchanan	42 25	92 00	....	T.R	9
KANSAS.							
Drew, Fred. P., M D., U.S.A.	Fort Riley	....	39 00	96 30	1,300	T.R	3
Fuller, Arthur N	Lawrence	Douglas	38 58	95 13	970	R	6
Goodnow, Isaac T	Manhattan	Riley	39 13	96 45	1,000	T.R	12
Oakfield, C. F	Emporia	....	....	....	....	T	1
Scott, James	Gardner	Johnson	....	....	....	T	1
Seymour, E. W., M. D	Junction City	Davis	38 58	96 32	1,260	T	3
Shaw, M	Leavenworth	Leavenworth	39 18	94 32	896	T	4
KENTUCKY.							
Beatty, O	Danville	Boyle	37 40	84 30	900	B.T.R	8
Mathews, Jos McD., D. D	Nicholasville	Jessamine	37 58	84 18	940	A	12
Mattison, Andrew	Paducah	Mt. Cracken	37 00	87 21	....	N	5
Savage, Rev. G. S., M. D	Millersburg	Bourbon	38 40	84 27	804	B.T.R	3
Swain, John, M. D	Ballardsville	Oldham	38 36	85 30	461	A	1
Woodruff, E. N	Louisville	Jefferson	38 22	85 38	....	A	11
Young, Mrs. Lawrence	Louisville	Jefferson	38 07	85 24	570	A	12
MAINE.							
Brackett, George Emerson	Belfast	Waldo	44 23	69 08	....	T.R	11
Dana, Wm. D	North Perry	Washington	45 00	67 03	100	A	12
Dewhurst, Rev. Eli	Pembroke	Washington	44 53	67 15	40	B.T	9
	Baldwinsville	Worcester	42 37	72 05	847	B.T	1
Gaines, Rev. A. G	Bethel	Oxford	44 20	70 52	650	T.R	2
Gardiner, R. H	Gardiner	Kennebec	44 40	69 46	90	B.T.R	12
Guptill, G. W	Cornishville	York	43 40	70 44	800	T.R	12
Moore, Asa P	Lisbon	Androscoggin	44 00	70 04	130	T.R	12
Parker, J. D	Steuben	Washington	44 44	67 50	50	A	12
Pratt, J. Frank, M. D	New Sharon	Franklin	44 37	70 03	... .	N	2
Reynolds, Henry	East Wilton	Franklin	44 44	70 17	... ...	N	11
Van Clarcom, James	Vassalboro'	Kennebec	44 28	69 47	....	B.T	12
West, Silas	Cornish	York	43 40	70 44	784	T.R	12
Wilbur, Benj. F	Dexter	Penobscot	44 55	69 32	700	T R	12

MARYLAND.

Name of observer.	Station.	County.	North latitude. ° '	West longitude. ° '	Height. Feet.	Instruments.	No. of months received.
Baer, Miss Harriott M......	Sykesville.....	Carroll........	39 23	76 57	700	T.P.R..	12
Bell, Jacob E...............	Leitersburg....	Washington...	39 35	77 30	....	T.R....	6
Dutton, Prof. J. Russel....	Chestertown...	Kent	39 12	75 59	....	A......	10
Goodman, Wm. R.	Annapolis.	Anne Arundel.	38 59	76 29	20	A......	12
Hanshew, Henry E.........	Frederick	Frederick	39 24	77 26	....	A......	12
Johns, Montgomery, M. D.	Agric'l College.	Prince George's	....	... .	....	A	3
Lowndes, Benjamin O	Bladensburg...	Prince George's	38 57	76 58	112	T.R....	10
Stephenson, Rev. James...	St. Inigoes	St. Mary's.....	38 10	76 41	45	A.......	12

MASSACHUSETTS.

Astronomical Observatory.	Williamstown..	Berkshire	42 43	73 13	725	B T.R..	1
Bacon, William	Richmond	Berkshire	42 23	73 20	1,190	T.R ...	9
Brown, Nathan W	Topsfield	Essex	... ...	....	....	T R.....	6
Davis, Rev Emerson	Westfield.....	Hampden	42 06	72 48	180	A.......	12
Fallon, John.....	Lawrence.....	Essex	42 42	71 11	133	A.......	9
Metcalf, John Geo., M. D..	Mendon	Worcester.....	42 06	71 34	.... .	T.R....	12
Prentiss, Henry C., M. D ..	Worcester	Worcester.....	42 16	71 48	528	A..	12
Reynolds, Orrin A.........	Randolph.....	Norfolk	42 10	71 00	311	N......	1
Rodman, Samuel..........	New Bedford ..	Bristol	41 39	70 56	90	A.......	12
Snell, Prof. E. S...........	Amherst	Hampshire	42 22	72 34	267	A......	12

MICHIGAN.

Blaker, Dr. G H., jr..... Bacon, Frank M...	Marquette.....	Marquette.....	46 32	87 41	630	A.......	12
Coffin, Matthew.........	Otsego	Allegan	42 28	85 42	662	N.... ..	6
Crosby, J B.	New Buffalo...	Berrien	41 45	86 46	651	B.T.R..	5
Pitcher, Zena, M. D.......	Detroit	Wayne........	42 24	82 58	597	A.......	4
Schetterly, Henry R......	Northport	Leelenaw	45 13	85 24	....	N.......	7
Smith, Rev. L. M. S.	Mill Point.....	Ottawa.......	43 06	86 11	....	T.	6
Streng, L. H	Holland	Ottawa........	42 00	85 00	680	T.R.....	11
Van Orden, Wm, jr.......	Clifton	Keweenaw....	47 00	88 00	800	T.......	11
Walker, Mrs. Octavia C...	Cooper........	Kalamazoo...	42 40	85 30	690	T.R.....	11
Whelpley, Miss Florence E.	Monroe	Monroe........	41 56	83 23	590	T.R.....	11
Woodard, C. S............	Ypsilanti	Washtenaw....	42 15	83 47	751	A......	12

MINNESOTA.

Garrison, O. E.............	St. Cloud......	Stearns.......	45 45	94 23	....	T. R	7
Paterson, Rev. A. Bell, D. D.	St. Paul	Ramsey	44 53	93 05	800	T. R	7
Riggs, Rev. S. R	Pajutazee	Brown	45 00	94 00	....	T. R	7
Smith, Henry L	Forest City....	Meeker.......	45 45	94 00	....	T. R	6
Thickstun, T. F	Hastings.	Dakota..	....	....	... .	T. R	5
Wieland, C..........	Beaver Bay....	Lake..........	47 12	91 18	657	B. T.....	12

MISSOURI.

Bowles, S. B., M. D...	Greenfield ...	Dade.....	37 22	93 41	1,800	N........	8
Christian, John............	Harrisonville...	Cass	....	....	....	N.......	11
Engelmann, George, M D..	St Louis......	St. Louis......	38 37	90 15	481	A.	12
Fendler, Augustus	St. Louis......	St. Louis......	38 37	90 16	470	B. T. P..	12
Lunemann, John H, S. J ..	St. Louis......	St. Louis......	38 40	90 15	475	A.	1
Maxey, W. F.............	Paris	Monroe.......	39 30	92 00	700	T. R ...	1
Myers, J. H................	Kirksville.....	Adair.........	40 38	92 50	1,000	N.......	2
Ray, George P.............	Canton........	Lewis.........	....	....	....	T......	10
Tidswell, Miss Mary Alice..	Warrenton ...	Warren........	38 37	91 16	825	T.......	12

NEBRASKA.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° ′	° ′	*Feet.*		
Bowen, Miss Anna M. J....	Elkhorn City ..	Douglas	41 22	96 12	1,000	T.......	12
Evans, John	Fontenelle.....	Dodge	41 31	96 45	1,000	T.......	6
Hamilton, Rev. Wm........	Bellevue.......	Sarpy...... . .	41 08	95 50	....	T. R ...	12
White, Bela...............	Kenosha	Cass	40 51	95 54	1,050	N.......	5

NEW HAMPSHIRE

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Brown, Branch......	Stratford	Coos	44 08	71 34	1,000	T. R ...	12
Chase, Arthur	Claremont.....	Sullivan.......	43 22	72 21	539	B. T. R..	12
French, Isaac S., M. D	London Ridge .	Merrimack	43 20	71 25	475	T R.....	11
Nason, Rev. Elias	Exeter.........	Rockingham...	42 58	70 55	125	B T.....	10
Odell, Fletcher............	Shelburne	Coos	44 23	71 06	700	B T.....	4
Pitman, Charles H.........	North Barnstead	Belknap.......	43 38	71 27	....	T.	12

NEW JERSEY.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Cooke, Robert L	Bloomfield.....	Essex..........	40 49	74 08	120	A.	12
Rhees, Morgan J., M. D ...	Mount Holly..	Burlington.....	....	....	30	B T. ...	12
Thompson, George W......	New Brunswick	Middlesex	40 30	75 31	90	T	12
Whitehead, W. A.........	Newark........	Essex	40 45	74 10	35	B T. R..	12
Willis, Oliver R	Freehold	Monmouth	40 15	74 21	....	T.......	2

NEW YORK.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Bartlett, Erastus B	Vermillion.....	Oswego	43 26	77 26	327	T.	12
Beauchamp, Wm. M......	Skaneateles ...	Onondaga......	43 00	76 30	932	B. T	11
Bowman, John...........	Baldwinsville..	Onondaga	43 04	76 41	....	T.	12
Cowing, Philo.............	Seneca Falls...	Seneca	42 54	76 51	463	B T....	12
Dill, John B...............	Auburn........	Cayuga	42 55	74 28	......	T...... .	12
Denning, William H.......	Fishkill Landing	Dutchess	41 34	74 18	42	B. T R..	12
Dewey, Prof. Chester.... Kreyer, Carl T...........	Rochester	Monroe.......	43 08	77 51	516	B. T. R..	12
Gregory, S. O..............	Theresa	Jefferson	44 12	75 48	365	T. R.....	12
Guest, W. E	Ogdensburg....	St. Lawrence ..	44 43	75 37	232	N.......	12
Heimstreet, John W	Troy	Rensselaer.....	42 44	73 37	58	A..	12
Hibberd, A. A.............	Hermitage.....	Wyoming......	42 09	78 14	....	T R.....	8
Holmes, Dr. E. S..........	Wilson	Niagara	43 20	78 56	250	T........	12
House, John C	Waterford	Saratoga......	42 47	73 39	70	A.	12
Howell, Robert............	Nichols...... ..	Tioga	42 00	76 32	. ...	T........	12
Ives, William..............	Buffalo	Erie	42 50	78 56	600	A..	12
Mack, Rev E. T	Flatbush	Kings	40 37	74 02	54	B. T. R..	7
Mackie, Matthew	Clyde	Wayne	43 10	77 10	400	B T	6
Malcom, Wm. S....	Oswego	Oswego	43 28	76 30	250	B. T. R..	11
Mathews, M M., M. D....	Rochester	Monroe........	43 08	77 51	525	A........	12
Monroe, Prof. A. T	Fordham	Westchester...	40 54	73 57	147	B. T .	2
Morris, Prof. Oran W......	New York.....	New York.....	40 43	74 05	25	A.	12
Paine, H. M., M. D........	Clinton	Oneida	43 03	75 15	600	T P R..	12
Russell, Cyrus H..........	Gouverneur....	St. Lawrence..	44 19	75 29	....	B. T. R..	12
Spooner, Dr. Stillman	Wampsville ...	Madison.......	43 04	75 50	500	T. R ...	12
Sylvester, Dr. E. Ware.....	Lyons	Wayne	....	....	....	B T	8
Titus, Henry Wm..........	Bellport	Suffolk	40 44	72 54	15	A.	6
Wadsworth, A. S	Henrietta	Monroe........	43 06	77 51	600	B. T. P..	6
Wakeley, Charles C., Rutherford's Observatory.	New York... .	New York. ...	40 44	73 59	41	A	10
White, Aaron	Cazenovia.....	Madison.......	42 55	75 46	1,260	A.	12
Willis, Oliver R	White Plains...	Westchester....	41 05	73 40	....	T..... .	9

OHIO.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° ′	° ′	Feet.		
Abell, B. F	Welshfield	Geauga	41 23	81 12	1,205	T. R.	12
Adams, D. P	Marietta	Washington	39 25	81 31	630	T. R	7
Atkins, Rev. L. S	Saybrook	Ashtabula	41 52	81 01	650	T.	8
Benner, Josiah F	New Lisbon	Columbiana	40 45	80 45	961	B. T. R.	12
Clark, Wm. P	Medina	Medina	41 07	81 47	1,255	A.	12
Colbrunn, Edward	Cleveland	Cuyahoga	41 30	81 40	665	T.	12
Cotton, D. B., M. D	Portsmouth	Scioto	38 45	82 50	523	B. T. R.	3
Crane, George W	Bethel	Clermont	39 00	84 00	555	T. R	1
Davidson, Wilson	Freedom	Portage	41 13	81 08	1,100	B. T. R.	5
Dille, Israel	Newark	Licking	40 07	82 21	825	T.	10
Dole, J. G Griffing, C. S S.	Austinburg	Ashtabula	41 54	80 52	816	T. R.	2
Fraser, James	Little Hocking.	Washington	39 25	81 00	....	N	11
Hammitt, John W	College Hill	Hamilton	39 19	84 26	800	N.	12
Harper, George W	Cincinnati	Hamilton	39 06	84 27	*500	A.	12
Haywood, Prof. John	Westerville	Franklin.	40 04	83 00	....	A.	12
Hill, F. G	Dallasburg	Warren	39 30	84 31	800	N.	11
Huntington, George C	Kelley's Island.	Erie	41 36	82 42	587	B. T. R.	12
Hyde, Gustavus A. Hyde, Mrs	Cleveland	Cuyahoga	41 30	81 40	643	B. T. R.	12
Ingram, John, M. D	Savannah	Ashland.	41 12	82 31	1,098	A.	12
Jerome, A. E.	New Westfield.	Hood	41 13	83 49	692	T. R.	9
Johnson, Thos. H	Coshocton	Coshocton	40 18	81 53	765	A.	2
King, Mrs. Ardelia C	Madison	Lake	41 50	81 00	620	T. R	12
McClung, Charles L	Troy	Miami	40 03	84 06	1,103	B. T. R.	12
McMillan, Smith B	East Fairfield.	Columbiana	40 47	80 41	1,152	A.	12
Newton, Rev. Alfred	Norwalk	Huron	41 15	82 30	....	T.	12
Peck, Wm. R., M D	Bowling Green.	Wood	41 15	83 40	700	B. T. R.	12
Peiree, Warren	Garrettsville	Portage	41 15	81 10	900	T.	8
Phillips, R C. and J. H	Cincinnati	Hamilton	39 06	84 27	540	B. T. R.	12
Pillsbury, Mrs. M. A.	East Cleveland.	Cuyahoga	41 31	81 38	659	B. T	10
Smith, C H., M. D	Kenton	Hardin	41 30	84 41	....	B T.	4
Thompson, Rev David	Milnersville	Guernsey	40 10	81 45	....	T.	9
Thompson, Rev. Elias	Croton	Licking	40 13	82 38	......	T. R	12
Tappan, Eli T	Cincinnati	Hamilton	39 07	84 27	*470	A.	12
Trembley, J. B., M. D.	Toledo	Lucas	41 39	82 32	604	B. T. R.	12
Ward, Rev. L F.	Seville	Medina	39 59	81 47	1,075	A.	7
Warder, A. A	Cincinnati	Hamilton	39 08	84 35	800	T. R	11
Williams, Prof. M G	Urbana	Champaign	40 06	83 43	1,015	B. T R.	12
Wilson, Prof. J. H	College Hill	Hamilton	39 19	84 26	800	B. T. R.	11
Young, Prof. Chas. A. Stuart, E W. Pettingell, W. Elliott, J. C Watterson, H. R.	Hudson	Summit	41 15	81 24	1,137	B. T. R.	12

* Above low-water in the Ohio river at Cincinnati.

PENNSYLVANIA.

Boyers, W. R	Blairsville	Indiana	40 31	74 43	1.010	T R.	12
Brugger, Samuel	Fleming	Centre	40 55	77 53	780	T. R.	12
Darlington, Fenelon	Parkersville	Chester	39 54	75 37	218	T. R	12
Friel, P	Shamokin	Northumberl'd.	40 45	76 30	700	T. R	12
Hance, Ebenezer	Morrisville	Bucks	40 12	74 48	30	B. T. R.	12
Heisely, Dr. John	Harrisburg	Dauphin	40 16	76 15	....	A.	12
Heyser, William, jr	Chambersburg	Franklin	39 58	77 45	618	A.	1
Hickok, W. O	Harrisburg	Dauphin.	40 20	76 50	320	A.	12
Hoffer, Dr. Jacob R	Mount Joy	Lancaster	40 08	76 30	....	A.	12
Huebener, O. T Rickseeker, Lucius E	Nazareth	Northampton	40 43	75 21	530	B. T. R.	8
Jacobs, Rev. M Jacobs, H. E.	Gettysburg	Adams	39 49	77 15	624	B. T. R.	12
Kirkpatrick, Prof. Jas. A.	Philadelphia	Philadelphia	39 57	75 10	50	A.	12
Kohler, Edward	N. Whitehall	Lehigh	40 40	75 26	250	T.	10
Lyceum, Jefferson College	Cannonsburg	Washington	40 17	80 10	936	A.	5
McNett, E. L.	Carpenter	Lycoming	41 37	76 53	....	T.	5
Martindale, Isaac C.	Byberry	Philadelphia	40 05	75 00	70	T. R.	12
Meehan, Thomas Meehan J	Germantown	Philadelphia	....	....	....	T.	8

PENNSYLVANIA—Continued.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° '	° '	*Feet.*		
Muller, Prof. Rudolph.....	Latrobe.......	Westmoreland.	40 27	79 32	985	B. T. R..	9
Ralston. Rev. J. Grier......	Norristown....	Montgomery...	40 08	75 19	153	A........	12
Scott, Samuel..............	Worthington...	Armstrong... .	41 50	79 31	1,050	T. R....	12
Swift, Dr Paul	W Haverford..	Delaware......	40 00	75 21	400	T. R...	10
Taylor, John.............	Connellsville...	Fayette........	40 00	79 36	....	T.......	4
Tracey, George H...........	Sewickleyville.	Alleghany......	40 38	80 14	656	B. T. R..	1

RHODE ISLAND.

Caswell, Prof. Alexis......	Providence ...	Providence	41 49	71 25	120	A........	12
Sheldon, H. C.......... ..	Providence....	Providence ...	41 50	71 25	....	B. T. R..	12

TENNESSEE.

Stewart, Prof. Wm. M....	Clarksville.....	Montgomery...	36 28	87 13	481	A.......	12

UTAH.

Pearce, Harrison..........	St. George.....	Washington....	37 00	114 00	....	T. R....	6

VERMONT.

Buckland, David	Brandon	Rutland	43 45	73 00	....	T. R...	12
Chickering, Rev J. W.....	Springfield	Windsor.......	43 18	72 33	300	T. R....	11
Cutting, Hiram A.........	Lunenburg	Essex	44 28	71 41	1,124	A	12
Mead, S O...............	Rutland	Rutland.....	....	....	.....	T..	12
Paddock. James A.........	Craftsbury	Orleans........	44 40	72 29	1,100	T. R.....	12
Parker, Joseph.	West Rupert...	Bennington....	43 15	73 11	750	T.......	12
Petty, McK	Burlington.....	Chittenden ..	44 27	73 10	367	A.......	11
Tobey, James K......	Calais	Washington...	44 22	72 09	. .....	T. R....	5

WASHINGTON TERRITORY.

Swan, James G...........	Neeah Bay....	....	28 41	124 37	40	T.......	7

WISCONSIN.

Atwood, Isaac	Lake Mills....	Jefferson	43 00	89 00	....	N..	1
Bell, James H........... .	Kilbourn City..	Columbia......	43 30	90 00	945	N........	2
Curtis, W W.............	Rocky Run ...	Columbia	43 26	89 20	......	T. R....	12
Ellis, Edwin. M D	Ashland	Ashland.	46 33	91 00	610	T. R	12
Gridley, Rev. John	Kenosha......	Kenosha	42 35	87 50	600	B T. R..	12
Kelley, Charles W.........	Delafield	Waukesha	43 06	88 36	900	B. T.....	9
Kelsey, Prof Henry S.....	Beloit.	Rock..........	42 30	89 04	750	B. T. R..	12
Lapham, Increase A......	Milwaukee ...	Milwaukee	43 03	87 56	593	A.	12
Lups, Jacob...............	Manitowoc...	Manitowoc....	44 07	87 45	[illegible]	B. T.....	12
Mann. William	Superior	Douglas.......	46 46	92 03	680	T. R.....	12
Mathews, George.........	Brighton	Kenosha	42 36	88 03	700	N..	6
Powers, M. H......	Dartford......	Green Lake ...	43 30	89 25	......	B. T.....	4
Sterling. Prof. John W... Fallows, William........	Madison......	Dane....... ..	43 05	89 25	1,068	A........	7
Strunk, Daniel	Janesville	Rock..........	42 43	89 39	780	T........	5
Winkler, Carl, M. D.......	Milwaukee	Milwaukee. ...	43 03	87 57	600	B T. R..	12
Woods, William	Weyauwega...	Waupaca......	44 15	88 50	850	T........	12

MEXICO.

Name of observer.	Station.	Latitude.	Longitude.	Height.	Instruments.	No. of months received.
		° ′	° ′	*Feet.*		
Laszlo, Charles	San Juan Bautista, Tabasco	17 47	92 36	40	A	2
Nieto, J. A	Cordova, Vera Cruz	18 51	....	....	B. T. R.	12
Sartorius, Dr. Charles	Mirador, Vera Cruz	19 15	96 25	3,600	A	12

CENTRAL AMERICA.

Name of observer.	Station.	Latitude.	Longitude.	Height.	Instruments.	No. of months received.
Riotte, C. N	San José, Costa Rica	9 51	84 06	3.772	T. R.	12
Canudas, Antonio	Guatemala College, Guatemala	14 37	90 30	4,856	A	12
White, William T., M. D.	Aspinwall	9 21	79 54	....	A	3

BERMUDA.

Name of observer.	Station.	Latitude.	Longitude.	Height.	Instruments.	No. of months received.
Royal Engineers, (in the Royal Gazette.)	Centre Signal Station, Saint George's.	....	....	....	A	12

SOUTH AMERICA.

Name of observer.	Station.	Latitude.	Longitude.	Height.	Instruments.	No. of months received.
Hering, C. T	Government Plantation, Rustenberg, colony of Surinam, Dutch Guiana.	....	....	....	A	12

DEATHS OF OBSERVERS.

Lucian Fish, Burlingame, Kansas, early in 1861.
Andrew G. Carothers, at Martinique, West Indies, October 20, 1862.
Andrew J. Babcock, of Aurora, Illinois, at Pittsburg Landing, Tennessee.

Colleges and other institutions from which meteorological registers were received during the year 1862, included in the preceding list.

Nova Scotia	Acadia College	Wolfville.
	King's College	Windsor.
Canada	Grammar School	Niagara.
	Magnetic Observatory	Toronto.
Connecticut	Wesleyan University	Middletown.
Illinois	Lombard University	Galesburg.
	McKendree College	Lebanon.
	University of Chicago	Chicago.
Iowa	Cornell College	Mount Vernon.
	Griswold College	Davenport.
	Iowa State University	Iowa City.
	Yellow Spring College	Kossuth.
Maine	Oak Grove Seminary	Vassalboro'.
Maryland	Agricultural College	Prince George's county.
	Washington College	Chestertown.
Massachusetts	Amherst College	Amherst.
	State Lunatic Hospital	Worcester.
	Williams' College	Williamstown.

Colleges, &c., from which meteorological registers were received, &c.—Continued.

Michigan	Marine Hospital	Detroit.
Missouri	St. Louis University	St. Louis.
New Jersey	Freehold Institute	Freehold.
New York	Institution for Deaf and Dumb	New York.
	St. John's College	Fordham.
	University of Rochester	Rochester.
	Young Men's Association	Buffalo.
Ohio	Farmers' College	College Hill.
	Halcyon Academy	Croton.
	Otterbein University	Westerville.
	Urbana University	Urbana.
	Western Reserve College	Hudson.
	Woodward High School	Cincinnati.
Pennsylvania	Central High School	Philadelphia.
	Haverford College	West Haverford.
	Jefferson College	Cannonsburg.
	St. Vincent's College	Latrobe.
	Sewickleyville Academy	Sewickleyville.
Rhode Island	Brown University	Providence.
Tennessee	Stewart College	Clarksville.
Vermont	University of Vermont	Burlington.
Wisconsin	Beloit College	Beloit.
	Wisconsin University	Madison.
Central America	Guatemala College	Guatemala.

LIST OF METEOROLOGICAL MATERIAL CONTRIBUTED IN ADDITION TO THE REGULAR OBSERVATIONS.

Asiatic Society of Bengal.—Journal of the Society, 1860, containing abstracts of hourly meteorological observations, taken at the surveyor general's office at Calcutta, for June to November, 1859, inclusive.

Journal No. 1, 1861, containing same for May, June, and July, 1860.

Bowen, Miss Anna M. J.—Summary of observations of thermometer, winds, and clouds, for the hours 7 a. m. and 2 and 9 p. m., and for each month and season during the year 1862, at Elkhorn city, Nebraska.

Boettner, Gustav A.—Drawings of snow crystals observed in the winter of 1862–'63, at Chicago, Illinois.

Brackett, George Emerson.—Monthly abstract of observations in 1862, at Belfast, Maine, printed in the "Republican Journal;" summary for the year printed in the "Maine Farmer," Augusta.

Brown, Rev. John J.—Summary for 1861 at Dansville, N. Y., giving mean, extremes, and range of barometer and thermometer for the year, and amount of rain and number of days without frost.

Buchner, Dr. Otto.—Der Meteorit von Shalka in Bancoorah und der Piddingtonit. Von dem W. Haidinger. (Sonder Abdruck aus dem XLI. bde. d. Sitzungsb. d. Kais. Akademie d. Wissenschaften.) 8vo. p. 8.

Einige neuere Nachrichten über Meteoriten, namentlich die von Bokkeveld, New Concord, Trenzano, die Meteoreisen von Nebraska, von Brazos, von Oregon. Von W. Haidinger. 8vo. p. 6.

Die Calcutta-Meteoriten, von Shalka, Futtehpore, Pegu, Assam und Segowlee im k. k. Hof-Mineralien-Cabinete. Von W. Haidinger. 8vo. p. 14.

Die Meteoritenfälle von Quenggouk bei Bassein in Pegu und Dhurmsala im Punjab. 8vo. p. 7.

Meteoreisen von Rogue River mountain in Oregon und von Tuoson, Sonora, gesandt von Herrn Dr. Charles T. Jackson. 8vo. p. 2.

Die Dandenong-Meteoreisenmasse in Melbourne. 8vo. p. 1.

Der Meteorit von Parnallee bei Madura im k. k. Hof-Mineralien-Cabinet. 8vo. p. 4.

Der Meteorit von Yatoor bei Nellore in Hindostan. 8vo. p. 2.

[The preceding eight articles are by W. Haidinger, and are separate pamphlets extracted from "Seitzungsb. d. Kais. Akademie d. Wissenschaften."]

Carothers, Andrew G., U. S. consul, Martinique.—Printed copy of the daily meteorological observations made at the military hospitals, Guadaloupe, from October, 1861, to May, 1862, inclusive.

Extract from the Physical History of the Antilles, by M. Moreau de Gonnès, giving the dates of a number of hurricanes since 1825.

Caswell, Professor Alexis, D. D.—Monthly summaries of observations made at Providence, Rhode Island, during the year 1862. (Published in the "Providence Daily Journal ")

Connolly, Henry.—Observations at Rigolet, Esquimaux bay, Labrador, from July, 1860, to June, 1862, inclusive.

Dana, Wm. B.—Meteorological tables, being a summary for the year 1859, by Henry Willis, at Portland, Maine. (Printed.)

Dudley, Timothy.—Summary of observations for the year 1862 at Waverley, Illinois, giving the mean and extremes of thermometer and amount of rain for each month and for the year; also, date of earliest and latest frost.

Foster, W., jr.—Notices of meteors and aurora in July and August, 1862. (Providence, R. I., Journal, August 23, 1862.)

Frantzius, Dr. A.—Thermometer and barometer observations at San José, in Costa Rica, Central America, at 7 and 10 a. m. and 4 and 7 p. m., daily, from September, 1861, to August, 1862, inclusive.

Frey, S. C.—Newspaper record of thermometer and barometer at Springfield, Ohio, from February 4, 1861, to May 11, 1862.

Gardiner, R. H.—Printed summary of his observations at Gardiner, Maine, for the winter of 1861–'62, and a comparison with the mean and extremes of the preceding twenty-six winters.

Hague, Captain, astronomer of the North American Boundary Commission on the part of the English Commission—Monthly means and extremes of observations with barometer, thermometer, and psychrometer; also, the number of rainy days and amount of rain, from August, 1860, to December, 1861, at Fort Colville, Washington Territory; latitude 48° 39′ 58″ N., longitude 118° 3′ 52.8″ W.; height above the sea, 1,268 feet.

Hays, W. W., M. D.—Summary of the observations on temperature and rain made at the presidio of San Francisco, California, by the surgeons of the United States army at the post from July, 1852, to December, 1862.

Amount of rain measured at Benicia barracks, California, during each month from March, 1856, to February, 1863; also, a separate table showing the amount in the "rainy season" of each year during the same period.

Herschel, Sir J. F. W.—Manual of meteorology, by Sir J. F. W. Herschel, Bart. Extracted from the Admiralty Manual of Scientific Inquiry, third edition, 1859. 16mo. pp. 52.

Letter from Sir J. F. W. Herschel, Bart., to Sir J. W. Lubrock, Bart., on shooting stars. London, 1861, 8vo., pp. 4.

Report of the meteorological committee, part 1. Read July 17, 1837. Printed at the Gazette office, Cape Town, Cape of Good Hope. 8vo., pp. 20.

Hyde, Gustavus A.—Summary of observations at Cleveland, Ohio, for the year 1862, and comparison with the preceding six years.

Ives, William.—Article on the climatology of Buffalo, New York, prepared by him for H. Thomas's Buffalo City Directory; five pages.

Lake Winnepissiogee Cotton and Woollen Manufacturing Company, New Hampshire.—Amount of rain for each month in 1862 at the outlet of Lake

Winnepissiogee, in the town of Laconia, New Hampshire, and also at Lake Village, about four miles south on the same stream of water. Transmitted by J. B. French.

Lapham, I. A.—Dates at which the ice left Milwaukee river in each year from 1837 to 1863, inclusive.

Table showing the date of the arrival at Milwaukee, Wisconsin, of the first vessel in each year from the "lower lakes" from 1837 to 1863.

Lewis, James, M. D.—Hourly observations of temperature at Mohawk, New York, registered by his metallic self-recording thermometer during the year 1862, with the means calculated for each half month.

Logan, Thomas M., M. D.—Hydrography, meteorology, and hyetography of Sacramento, California, for a series of years, embracing chart of the oscillations of the Sacramento river from 1853 to 1862, inclusive; monthly and annual means of barometer, thermometer, and psychrometer, with force and direction of wind for the same period; monthly and annual amount of rain from 1849 to 1862, inclusive, together with general remarks on the weather and seasons.

Macgregor, Charles John, M. A.—Head master of the Grammar School, Stratford. Abstract of observations for the years 1861 and 1862, taken at Stratford, Canada West; 8vo., 6 pps.

McLain, Wm. D.—Observations on temperature and state of the weather at Central City, Gilpin county, Colorado Territory, from December, 1860, to May, 1861.

Martindale, Isaac C.—Summary of meteorological observations at Byberry, Philadelphia county, Pennsylvania, for the year 1862.

Notes of the weather at Byberry, Pennsylvania, at various times from the year 1798 to 1847, collected by I. C. Martindale from authentic accounts.

Mullan, Lieut. John, U. S. A.—Meteorological record kept by Theodore Koleski, at Cantonment Wright, Rocky mountains, on the military road expedition under command of Lieutenant John Mullan, United States army, during the winter of 1861–'62.

Navy Department, Bureau of Medicine and Surgery.—Monthly meteorological registers kept at naval hospital, Chelsea, Massachusetts, year 1862, except April and May; naval hospital, New York, year 1862; naval hospital, Philadelphia, year 1862.

Newton, H. A.—An account of two meteoric fireballs observed in the United States August 2 and August 6, 1860, with computation of their paths, by H. A. Newton, of Yale College. (From American Journal of Science and Art.) 8vo., 12 pp.

Nitchy, F. A.—Account of a severe hail storm at Herman, Gasconade county, Missouri, June 17, 1862.

Observatoire Impérial, Paris.—Annales, tome XX, containing meteorological observations for 1859, at Paris.

Daily meteorological observations received at the observatory from various parts of Europe by telegraph, and lithographed for distribution.

Observatorio Fisico-Meteorico de la Habana, Don Andres Poey, director.—Hourly meteorological observations with full sets of instruments from July to December, 1862, inclusive.

Paine, Dr. H. M.—Monthly summaries of observations at Clinton, Oneida county, New York; printed slips.

Payot, Venance.—Observations météorologiques faites a Chamounix, pendant l'année 1858, Janvier et Février 1859, faisant suite à celles publiées en 1857, par M. Venance Payot, naturaliste. (Extrait des annales de la Societé d'Agriculture, d'histoire naturelle et des arts utiles de Lyon, dans sa séance du 14 Mars, 1862.) 8vo., 20 pp.

Pollard, T. F.—Observations at Brookfield, Vermont, of temperature, winds, clouds, and weather, from June 24, 1859, to the end of the year 1862.

Radcliffe Trustees.—Astronomical and meteorological observations made at the Radcliffe Observatory, Oxford, in the year 1858, under the superintendence of Manuel J. Johnson, M. A., late Radcliffe observer, reduced and printed under the superintendence of the Rev. Robert Main, M. A., Radcliffe observer. Vol. XIX; published by order of the Radcliffe trustees, Oxford, 1861.

Rankin, Colin.—Register of barometer and thermometer from Moose Factory to Lake Superior, June 17 to July 2, 1862.

Riblet, J. H.—Summary for the year 1862 at Orchard Farm, near Pekin, Illinois, giving the mean and extremes of thermometer and amount of rain for each month and for the year, and the date of the earliest and latest frost.

Royal Geographical Society, London.—Proceedings of the society, vol. 6, No. 2, 1862, containing a notice of the earthquake of Erzerum, in latitude 39° 55′ 20″, longitude 41° 18′ 31″, June, 1859, by Robert A. O. Dalyell, esq., F. R. G. S., her Britannic Majesty's consul at Erzerum.

Sheldon, H. C.—Monthly summaries of observations made at Providence, Rhode Island, in 1862. (Published in the "Evening Press.")

Societa di Acclimazione e di Agricultura in Sicilia.—Proceedings containing monthly registers of observations at Palermo.

Société des Sciences Naturelles de Neuchâtel.—Bulletin 1859 to 1861, volume 5, containing:

Precipitation de la rosée pendant le jour, par M. Favre. pps. 1–4.

Rapport du comité météorologique pour l'année 1858:—Resumé des phénomènes les plus remarquables qui se sont passès à Neuchâtel dans le 16me siècle.—Résumé météorologique pour l'année 1858; le résumé comprend les stations de Neuchâtel, de Chaumont, de Fontaines, et de la Chaux-de-Fonds, les observations limnimétriques des trois lacs de Neuchâtel, de Bienne et de Morat. pps. 102–147.

Résumé des observations météorologiques relatives aux vents, faites à Corneaux de 1812 à 1819, par M. le pasteur Peters; calculé et presenté, par M. le professeur H. Ladame. pps. 148–154.

Rapport du comité météorologique pour l'année 1859:—Résumé de phénomènes les plus remarquables à Neuchâtel dans le 17me siècle.—Résumé des observations faites en 1859 dans le canton et les observations limnimétriques des trois lacs. pps. 266–321.

Résumé des travaux de M. Schonbein sur l'ozone, présenté par M. Kopp, professeur. pps. 322–345.

Note sur quelques instruments météorologiques enregistreurs, par M. Hipp. pps. 587–590.

Rapport du comité météorologique pour l'année 1860:—Résumé des phénomènes les plus remarquables à Neuchâtel de l'an 1700 à l'an 1750.—Résumé météorologique pour l'année, 1860. pps. 675–752.

Note sur la température du lac à différentes profondeurs, par H. Ladame, professeur. pps. 753–761.

Note sur la température de l'eau des fontaines de la ville de Neuchâtel, par H. Ladame, professeur. pps. 762–763.

Société Météorologique de France.—Annuaire, 1861.

Swan, Caleb.—Amount of rain at the Eastern Dispensary, Grand street, New York, during each month of the year 1862, from the record kept by Dr. J. P. Loines, house physician of the dispensary; also, during each year from 1854 to 1861, inclusive.

Taylor, John.—A table showing the cold days in each year from 1843 to 1861, inclusive, at Connellsville, Pennsylvania; also, the mean temperature and amount of snow for each month in the year 1861.

Thatcher, A. E.—Three small manuscript books containing barometer and thermometer observations and notes on the weather, kept at New York from August, 1855, to June, 1862.

Tolman, James W.—Monthly and annual summary of observations for 1862, made at Winnebago, Illinois. (Published in the "Rockford Register.")

Trembley, J. B., M. D.—Monthly summaries of observations during the year 1862 at Toledo, Ohio, with remarks on the weather and comparisons with previous observations. (Published in newspaper.)

White, W. T., M. D.—Surgeon-in-chief of the Panama railroad. Fall of rain at Aspinwall, New Granada, from 1857 to 1862.

Whitehead, W. A.—Monthly and annual summaries of observations made at Newark, New Jersey, during the year 1862, with comparisons of the same with the means and extremes of a series of years. (Published in the "Sentinel of Freedom and Weekly Advertiser.")

Woodhouse, Mrs. E. A.—Observations of Professor Parker Cleveland, (her father,) from 1808 to 1859, at Bowdoin College, Brunswick, Maine.

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 1862, with general estimates for the year 1863:

General Statement.

RECEIPTS.

The whole amount of Smithsonian 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
The extra fund of unexpended income is invested as follows, viz:	
In 75,000 Indiana 5 per cent. bonds, yielding	3, 750 00
In 53,500 Virginia 6 per cent. bonds, yielding nothing in 1862.	
In 12,000 Tennessee 6 per cent. bonds, yielding nothing in 1862.	
In 500 Georgia 6 per cent. bonds, yielding nothing in 1862.	
In 100 Washington city 6 per cent. bonds, yielding during 1862.	6 00
Premium on sale of $1,875 gold, (interest) paid on Indiana bonds, July 1, at 40½	759 37
Total income	35, 425 51
Balance in the hands of the treasurer, January, 1862	22, 045 17
Total receipts	57, 470 68
Total expenditure	27, 961 07
Balance in the hands of the treasurer, January, 1863	$29, 509 61

EXPENDITURES.

For building, furniture, and fixtures	$2, 237 66
For general expenses	11, 674 41
For publications, researches, and lectures	7, 744 44
For library, museum, and gallery of art	6, 304 56
Total expenditure	$27, 961 07

Statement in detail of the expenditures of 1862.

BUILDING, FURNITURE, AND FIXTURES.

Building incidentals	$1, 672 34	
Furniture and fixtures in general	80 02	
Furniture and fixtures for museum	485 30	
		$2, 237 66

GENERAL EXPENSES.

Meetings of the Board	$81 00	
Lighting and heating	1, 142 26	
Postage	353 36	
Transportation, general	733 10	
Exchanges	1, 550 32	
Stationery	281 38	
General printing	441 46	
Apparatus	119 06	
Laboratory	408 45	
Incidentals, general	315 02	
Extra clerk hire	405 00	
Salaries, secretary	3, 500 00	
Chief clerk, bookkeeper, messenger, &c	2, 344 00	
		11, 674 41

PUBLICATIONS, RESEARCHES, AND LECTURES.

Smithsonian Contributions	$932 97	
Smithsonian Reports	219 88	
Smithsonian Miscellaneous Collections	3, 774 25	
Meteorology	1, 963 08	
Researches and investigations	94 75	
Lectures	759 51	
		7, 744 44

LIBRARY, MUSEUM, AND GALLERY OF ART.

Cost of books and binding	$1, 513 63	
Pay of assistants in library	1, 215 00	
Transportation for library	346 76	
Incidentals for library	44 25	
Museum, salary of assistant secretary	2, 000 00	
Transportation for museum	354 54	
Incidentals for museum	146 59	
Explorations	555 29	
Gallery of art	128 50	
		6, 304 56
Total expenditures		$27, 961 07

It will be seen that the whole income during the year 1862 was $35,425 51, instead of the estimated income, $34,666 14. This difference is due to the receipt of $759 37 as premium on the gold in which the first half year's interest on the Indiana bonds was paid.

The expenditures during 1862 were $27,961 07, leaving $7,464 44 to be added to the balance in the hands of the treasurer on the 9th of January, 1862.

The amount of bills for work already contracted for will not exceed $2,500.

The foregoing statement is an actual exhibit of the Smithsonian funds, irrespective of credits and payments which have been made in behalf of other parties. For example, the Institution during the past year has paid several bills for work done on account of the government, the amount of which has been refunded and credited to the appropriations from which the expenditure was originally made.

The appropriation from Congress for the preservation of the collections of the exploring and surveying expeditions of the United States has been expended, as heretofore, under the direction of the Secretary of the Interior, in assisting to pay the expenses of extra assistants in the museum, and the cost of arranging and preserving the specimens. The sum received from this source has been credited to the museum, and has served to diminish the amount of expenditures for that object on the part of the Institution, although it has not been sufficient to defray all the expenses on account of the preservation and public exhibition of the specimens.

The articles intrusted to the care of the Institution are in good condition, and the work of the distribution of the duplicates of the government as well as those of the Institution is still in progress.

A part of the expenditure on the building is due to refitting the apparatus room, and re-covering, with tin, the northern portion of the roof of the west connecting range which was blown off during the storm of February 24.

From the foregoing statements it will appear that the financial affairs of the Institution are still in a prosperous condition, and that the Board of Regents could resign their trust to-day, with the undiminished original bequest of Smithson in the treasury of the United States, with over one hundred thousand dollars on hand or in secure investments, and with $66,000 in southern State stocks, from which it is hoped at some future time interest may be received.

The committee submit the following approximate estimates for the year 1863

Estimated income		$34, 666 1:

ESTIMATED EXPENDITURE.

For building, furniture, and fixtures	$2,000	
For general expenses	10,500	
For publications, researches, and lectures	10,500	
For library, museum, and gallery of art	9,000	
		$32,000

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

Respectfully submitted.

JOS. G. TOTTEN.
A. D. BACHE.

FEBRUARY, 1863.

JOURNAL OF PROCEEDINGS

OF

THE BOARD OF REGENTS

OF

THE SMITHSONIAN INSTITUTION.

WASHINGTON, *January* 21, 1863.

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.

Present: Hon. H. Hamlin, Vice-President of the United States, Hon. W. P. Fessenden, Hon. S. Colfax, Hon. S. S. Cox, Hon. E. McPherson, and the Secretary.

Several members being absent on account of a severe storm, the Regents devoted the meeting to the examination of the building, museum, library, and collections..

The Board then adjourned, to meet on Saturday, January 31, 1863.

WASHINGTON, D. C., *January* 31, 1863.

An adjourned meeting of the Board of Regents was held this day, at 11 o'clock a. m. Present: Vice-President Hamlin, Hon. L. Trumbull, Hon. G. Davis, Hon. S. S. Cox, Professor A. D. Bache, the treasurer, Mr. Seaton, and the Secretary.

The Vice-President was called to the chair.

The Secretary announced the death of Hon. James A. Pearce since the last session of the Board, and stated that Hon. Garrett Davis, who was present, had been appointed a Regent by the President of the Senate to fill the vacancy.

Professor Bache, after a series of appropriate remarks, offered the following resolutions, which were unanimously adopted:

Resolved, That the Board of Regents of the Smithsonian Institution deeply mourn the loss of their distinguished fellow-regent, James Alfred Pearce.

Resolved, That in the death of Mr. Pearce our country has lost a refined and influential citizen, the Senate of the United States an able, judicious, honest statesman, and this Institution an active, intelligent, and learned regent.

Resolved, That we sincerely condole with the afflicted family of Mr. Pearce, and offer to them our heartfelt sympathy in their great bereavement.

Resolved, That a copy of these resolutions be communicated by the secretary of the Smithsonian Institution to the family of the deceased.

On motion of Mr. Trumbull, it was

Resolved, That Professor Bache be requested to furnish a copy of his remarks in relation to Hon. J. A. Pearce for insertion in the journal of the Board of Regents.

The Secretary announced the death of William McPeak, who had been the janitor of the Institution from its organization, and recommended the payment by the Board of his funeral expenses.

On motion, the Secretary was authorized to pay the bill of funeral expenses of the late janitor of the Institution.

Professor Bache, in behalf of the executive committee, presented a general statement of the financial condition of the Institution, and an account of the expenditures during the year 1862.

The Vice-President suggested the propriety of taking some action respecting Mr. George E. Badger, whose name still appeared as a member of this Board, but who was known to be in rebellion against the government.

After some remarks relative to the knowledge of the fact of Mr. Badger's present position, by several members of the Board, on motion of Mr. Trumbull the following resolution was adopted:

Resolved, That the secretary be directed to inform the Congress of the United States that George E. Badger, one of the Regents of this Institution, has not attended the recent meetings of the Board, and they are advised that he is now in rebellion against the government of the United States, and submit whether the name of said Badger should longer remain on the list of Regents of said Institution.

On motion, the Board adjourned to meet on Tuesday, February 3, at 7 o'clock p. m.

WASHINGTON, *February* 3, 1863.

The Board of Regents met this day, pursuant to adjournment, at 7½ o'clock p. m., in the Regents' room. Present: Hon. L. Trumbull, Hon. G. Davis, Hon E McPherson, Hon. S. S. Cox, Hon. R. Wallach, General J. G. Totten, Professor A. D. Bache, and the Secretary.

Mr. Trumbull was called to the chair.

The minutes of the preceding meeting were read and approved.

The Secretary presented his annual report of the operations of the Institution during the year 1862, which was read and approved.

Professor Bache presented the report of the executive committee, containing an account of the receipts and expenditures for the year 1862 and estimates for 1863, which was read and approved.

On motion of Professor Bache, it was

Resolved, That the chairman appoint a member of the Board to fill the vacancy in the executive committee occasioned by the death of Mr. Pearce.

The chairman appointed Hon. Richard Wallach to fill the vacancy.

The Secretary presented to the Board the following communications:

WAR DEPARTMENT,
Washington City, January 26, 1863.

SIR: I have this day requested Hiram Barney, esq., collector of the port of New York, to send to you the books, maps, papers, and other articles, now in his possession, taken by the United States forces in South Carolina, to be held in the Smithsonian Institution, subject to the orders of this department. I would be pleased to confer with you in regard to this matter if you will be so good as to call at this department Tuesday, at three o'clock p. m.

I am, sir, very respectfully, your obedient servant,

EDWIN M. STANTON,
Secretary of War.

Professor JOSEPH HENRY,
Secretary of the Smithsonian Institution.

WAR DEPARTMENT,
Washington City, January 29, 1863.

SIR: The Secretary of War directs that you take possession of the books and papers of Bishop John Johns, at his late residence at Fairfax Seminary, and transmit them under sufficient guard to Professor Henry, at the Smithsonian Institution, in this city.

Very respectfully, sir, your obedient servant,

EDWARD CANBY,
Brigadier General United States Volunteers.

Brigadier General J. P. SLOUGH,
United States Volunteers, Military Governor of Alexandria.

Professor Henry stated that in his interview with the Secretary of War the latter had requested that an inventory of the books, &c., should be made, and that they should be carefully preserved in a room by themselves. The Secretary, on behalf of the Regents, provisionally agreed to these propositions on condition that the expense of the shelving and fitting up of the room, and the preparation of the list, should be at the expense of the government.

The Board of Regents acquiesced in the propriety of taking charge of these libraries, and of carefully preserving them until the termination of the present war.

The Secretary stated that the libraries had been received—the one from South Carolina, in thirty-three boxes and one bundle, by the transportation company from New York, and that of Bishop Johns, in loose volumes, by army wagons from Alexandria. It is proposed to place these libraries in the unoccupied apartment in the south tower above the Regents' room.

The following extracts from the correspondence of the Institution were then presented, after which the Board adjourned, to meet again, if necessary, at the call of the Secretary.

WESTERN UNION TELEGRAPH COMPANY,
Secretary's Office, Rochester, N. Y., April 21, 1862.

DEAR SIR: Your favor of the 15th instant, enclosing circular, is received. I recommend that you get one hundred copies of your circular printed and send them to E. Creighton, esq., Superintendent of the Pacific Telegraph, Omaha, Nebraska Territory. Mr. Creighton will see them distributed along the line at the proper places, and will renew the same from time to time with his instructions. You will please give him particular directions so as to secure what you want.

I would like to have you send me a few copies after they are printed, that I may assist you in getting the several telegraph companies between Brownsville, in Missouri, and Washington, to transmit for you free this class of business for a limited time at least.

I have written to Mr. Creighton and sent him your circular, but as many copies will be required they had better be printed, as I suggest.

Yours, truly,

HIRAM SIBLEY,
President Western Union Telegraph Company.

The following is the circular referred to in the preceding letter, which has been distributed to the telegraph offices on the line between Missouri and California:

Directions for telegraphing storms to the Smithsonian Institution, Washington.

Violent storms usually come from the west—therefore, after a storm has commenced, send a telegram eastward, giving

1. The time of beginning of storm.
2. Direction of the wind.
3. Character of the storm, whether wind, rain, snow.

After the storm is over, send the following:

4. Time of ending of storm.
5. Changes of the wind.
6. Changes of temperature.

In accordance with this arrangement the Institution has received occasionally notices of storms commencing in the Rocky mountains, and even in California.

St. Louis, *August* 14, 1862.

Dear Sir: I believe I have before informed you of Dr. Parry's botanical exploration in the Colorado Territory. I have now a long series of barometrical observations made by him on the different points visited by him, and among them the snow peaks Mount Guyot and Pike's Peak.

From a preliminary calculation I find that the latter rises above Fontaine qui bouit, at its base, about 7,700 feet; the fountain itself Frémont finds 6,350, and I about 6,500, so that the peak is doubtless in the neighborhood of 14,000 feet, "snow-capped, but easy of access." The timber reaches to within 2,200 feet of the top. Mount Guyot is found to be about 13,000 feet high; Berthoud's Pass, 11,400, (all timbered.)

These results, which I think are approximatively correct, show the great elevation of the base of the mountains, (Denver, 5,300 feet; Mount Vernon, 6,400 feet;) the great elevation of the peaks, and the great height of the limit of timber. 10,000 to 12,000 feet seems to be that limit between forty and thirty-five degrees latitude in the Rocky mountains.

Very respectfully, yours,

GEORGE ENGELMANN.

Professor Henry.

The following remarks relative to this letter have been received from Professor Guyot, to whom it was submitted:

"I return to you, with my thanks for its communication, the interesting letter of Dr. Engelmann. I had become acquainted with a part of that information by a letter from himself. It is exceedingly gratifying to see that interesting field of labor beginning to be explored. I trust that Dr. Parry will be able to continue his useful investigations.

"A great desideratum for the mountain measurements of the far west is the determination of some points near the base of the great chains with some degree of accuracy. We would obtain such points by a few regular barometric stations. Could not an observer be found in Denver or Colorado City, for instance, who would at the same time furnish suitable corresponding observations for measurements in the mountains, which are indispensable for the accurate determination of the high peaks of the Rocky mountains? It is rather provoking to have the consciousness that we do not know the true altitude of any point in these 2,000 miles of inland country, within one or two hundred feet, to say the least.

"I suppose that the barometric correction by Plantamour, mentioned in the report of the proceedings of the British Association, relates to the influence of the hour of the day at which barometric measurements are made, as derived from the means of St. Bernard and Geneva. It is the correction the amount of which I tried to determine in the latitude of the Black mountain and elsewhere, and which I apply in all my measurements. It amounts to $\frac{1}{100}$ of the difference measured, in the hottest part of the day, above the mean, or $\frac{1}{50}$ if we take the daily extremes. It is thus of considerable importance, though usually neglected. I think, however, that the whole needs a considerable revision. Temperature is the main cause, but the diurnal variation of pressure also comes in with contrary effects."

United States National Observatory,
Washington City, January 8, 1862.

Dear Sir: You are, I believe, aware that for some time past I have been engaged in investigations relative to Biela's comet. In the course of these investigations I have collected and discussed all the observations that could be found for each of the recorded six appearances, and, by help of independent elliptic elements for each, have digested these observations into a series of twenty-five normal places, extending though, with wide intervals, from 1772 to 1852. I have also computed rigorously the effect of planetary perturbations from 1846 to 1858, and am now engaged in continuing this computation to the next return in 1865. Moreover, I have carefully studied the relative motions of the two nuclei into which the comet is now divided, and find that the time and place of their separation can be indicated with a good degree of approximation, thus limiting the field of possible causes of the catastrophe.

It was my expectation, at the outset, to have extended the calculation of perturbations over the whole interval from 1772, so as to unite, if possible, by a single theory, all the observed places of the comet; but a nearer contemplation of this task, and a little actual trial, show that with my present official duties this would be a work of many years. It has occurred to me, however, that it would be in accordance with the plans of the Smithsonian Institution, and in keeping with the generous interest it has always shown in scientific investigations, to assist in this work by enabling me to employ a computer, to whom can be intrusted the more mechanical details of calculation. I venture, then, to suggest this proposition to you, and if, as I would fain hope, it should meet your approval, and you authorize me to enter into such an arrangement, I will gladly resume the work in accordance with my original plan, and with renewed hopes of success.

Trusting to hear from you on the subject, I am, respectfully and truly yours,

J. S. HUBBARD.

Professor HENRY.

The investigations to which the foregoing communication relates are of a highly interesting character, and well worthy the assistance of the Smithsonian Institution. The prosecution of the work has, however, for the present been suspended.

WEST CHESTER, PA., *October* 31, 1862.

DEAR SIR: For the last two or three years I have been employing and amusing the leisure hours of my old age in collecting *materials* for brief notices of men and events in my native *county of Chester*, in the State of Pennsylvania. I obtained imperfect accounts of about one hundred and thirty men of the county, who, in their day and generation, had acquired some character and consequence among their contemporaries of the province from its first settlement, under the auspices of William Penn, down to the present time. Those materials were, indeed, very defective, owing to the culpable indifference and negligence of our ancestors in preserving them. But, such as they were, I endeavored to make the best use I could of them, and caused them to be printed in *numbers*, under the title of NOTAE CESTRIENSES, in a newspaper of this village. I cut the articles from the paper as they were published, and arranged copies of them, in numerical order, in three several *scrap-books* for preservation and convenient reference. One of these scrap-books I shall deposit in the library of the Chester County Cabinet; another will be deposited in the library of the Pennsylvania Historical Society, at Philadelphia; and the third I propose, with your permission, if you can allow it the space it may occupy, to put in the library of the Smithsonian Institution, at Washington, with the view and hope that in each of those depositories, the said NOTAE may be accessible to any and every one who may have curiosity enough to wish to refer to them.

My humble *memorials* of the men of Chester are very meagre; yet, when I review them, and consider how careless and indifferent our people have been in such matters, I am surprised even at my own success in gathering my inadequate materials for the undertaking, scattered as they were over so extensive a district.

I am, dear sir, your feeble yet faithful octogenarian friend,

WILLIAM DARLINGTON.

Prof. JOSEPH HENRY.

The foregoing letter is from our much respected and esteemed correspondent the venerable Dr. William Darlington, of West Chester, Pennsylvania. It relates to a work performed in the evening of a long and laborious life, devoted to the advance of science and the practice of Chistian love and charity. Its publication may induce others to render a like service to their neighborhood, and thus increase the inducements to well doing through the desire inseparably connected with our instincts of *a future*—to live favorably in the memory of those who may succeed us. (Since this letter was presented to the board Dr. Darlington has departed this life. He died on his eighty-first birth-day, April, 1863.)

PARIS, *November* 24, 1862.

DEAR SIR: Mr. Hermann de Schlagintweit has enclosed me this note for you, asking me to add something as an introduction, which is scarcely necessary, seeing that he is one of the brothers Schlagintweit whose labors in Thibet and high India are so familiar to us. Those of the specimens I have seen are highly interesting, and the fact that they were collected by the Schlagintweits in person gives them a full guarantee.

Your obedient servant,

THEODORE LYMAN.

"MUNICH, (BAVARIA,) *November* 7, 1862.

DEAR SIR: Some days ago I had the pleasure of making the acquaintance of Mr. Theodore Lyman, who gave me many interesting facts respecting your Institution, and especially about your important practical and scientific meteorological researches. He also gave me a most lively description of the extent and the variety of the collections of the Institution. As he has kindly given me an introduction to you, I take the liberty of addressing you this communication relative to such parts of our collections of ethnography and natural history as we are now about to dispose of.

Of the objects mentioned in the accompanying statement, Mr. Lyman has seen but few, but sufficient, I trust, to enable him to give you more particulars as to their character if it should be desired, especially as he has had Professors Kaup and Siebold's opinion of them.

Besides the objects of natural history. I may mention the photographic colored fac-similes of a great number of my water-color drawings, their number amounting to 125, and including only such objects as are not among the plates published in our atlas. Mr. Brockhaus has made several series for England, one for Paris, and two for India, but has a few still remaining in his hands. The price he charges is 70 thalers, or £10, for thirty views, or £40 for the series. Mr. Lyman has seen them, and will perhaps be good enough to let you know in a few words how they are executed, as my description might too easily be influenced by the fact that I made the originals myself, or worked over those made by my brother Adolphe.

My address for this winter will be: Dultplatz 10, Munich, Bavaria.

With the expression of my most sincere consideration, I remain yours, most truly,

"HERMANN DE SCHLAGINTWEIT.

Ethnography.—The objects are:

Twenty complete skeletons, head and body, (put up,) of India Thibet, and Turkistan. They include savage tribes of India, such as Gonds, Santhals, &c. Price of the original, £20 to £30; of a copy in natural size, color, &c., in papier mache, £5.

Fifty skulls, without the body, from the same regions. Price of an original, £4 to £6; of a copy in papier mache, £1½

I mention especially our collection of *facial casts*, of which I send you the prices of copies in metal and also in plaster. The metallic casts are better able to resist the effects of time and exposure, but for a most careful reproduction of any detail the plaster copies can be entirely relied on. Price in metal, 24 shillings; in plaster, 4 shillings a cast.

Zoology.—We have about fifty large stuffed animals, many in duplicate, among which are *Bos grunniens*, *Equus hemionus*, *Asinus onager*, male and female, *Ovis argali*, (ammon,) in fact, of nearly all the larger animals of Thibet, one to three specimens of each. Price of each, £9 to £15.

Of smaller animals. such as *Cervus moschatus*, the different ovine and caprine domestic animals of Central Asia, etc., the price is from £2 to £6. Numerous skeletons of mammalia, put up; price, £2 to £5. Besides, skulls of *Equus hemionus*, £5; of a rhinoceros. from Bhutan Tarai, £8; of *Ovis argali*, £5, &c. An elephant skull, unusually large, with a weight (198 lbs.) and cranial capacity of about 6,200 cubic inches, determined by filling it with large shot. The *brain* of another elephant skull in plaster. It was obtained by a skull defective in its facial parts, being filled with plaster, and then gradually broken into small pieces, so that the very foldings of the dura mater remained untouched; the original in plaster, £15; a fac-simile in papier mache, £5.

I might add details of about 400 species of *birds*, 2 to 10 shillings, in nearly 1,800 specimens, chiefly from the Himalaya and Thibet; reptiles, 8 shillings to £1, and fishes, 5 to 10 shillings, determined by Dr. Gunther, (Proceedings Zool. Soc., London;) butterflies, 20 for £1, determined by Dr. Moore; insects, 40 for £1, as well as plants and geological collections.

The objects mentioned are at a country seat of ours, Jaegersburg, near Forchheim, Bavaria, not very far by rail from Munich.

CHRISTIANIA, *January* 6, 1863.

SIR: The knowledge of the countries and nations of the earth being particularly useful as well to the mariner as to the trader, it ought to be especially cultivated by us Norwegians. One of the most effective means of calling forth the sympathies of the people in behalf of a science is that of establishing public museums, or collections of objects, presenting immediately to the eye things of which no accurate idea or conception can be conveyed by a mere description. The public having thus, so to say, intuitively acquired a feeling of interest in the subject, it will be possible, by united efforts, in the course of some years, to bring together collections not only instructive to the nation possessing them, but also deserving the admiration of foreigners.

The British government has in this respect set the greatest example. * * *

An ethnological museum having now been established at the University of Christiania with the object of illustrating the manners, mode of living, and civilization of the various nations, it is to be hoped that the numerous and enterprising class of Norwegian seamen will avail themselves of the many opportunities offered on distant voyages to procure objects of interest for such a common national depository. The managers of the collection have already had frequent occasions to express their thanks even to common sailors for gifts to the institution, for which the state has also set apart a sum that, although moderate, will enable the managers to refund expenses incurred in procuring objects for the museum. Thus it will be within the power of any seaman to contribute towards enlarging the collection.

Trusting to the kind support of the public also for the future, the managers consider it expedient to lay down some rules for the guidance of those who, for the sake of public utility, may be willing thus to contribute towards enlightening their fellow citizens:

1. The nations and countries, the condition and state of which it will be of particular interest to see illustrated and exhibited, are especially those most differing from our own country and our own people; consequently, in the first place, the nations out of Europe, and, among the Europeans, those least known, and of the most antiquated manners. As a rule, objects of antiquity are also of greater rarity, and will be more acceptable to a collection than things now in use.
2. The objects, suitable for illustrating the condition of such nations, are innumerable. Sacred images, weapons, tools used in the principal trades, clothing, furniture, domestic implements, and products of industry, may be mentioned. Of course, models, drawings, and especially photographs, will afford quite as trustworthy information as the object itself. Articles liable to spoil, or the preservation of which would involve expenses, cannot be received in a museum.
3. The limited means at the disposal of a Norwegian institution will, as a matter of course, necessitate the selection of articles the price of which is

not excessive. The exhibition of objects of silver or gold or of regal luxuries is also, in fact, of less importance than illustrating the condition of the people itself.

4. In order, however, that the objects of interest to a museum may be applied according to their purpose—illustrating the nature of a country or the mode of living of the people—it will be necessary that every article forwarded be accompanied by an accurate account of where it was procured; by what nation used or made; in what way used, and for what purpose intended.

In order to give this request due publicity, it is desirable not only to assist in distributing these lines to those who may be supposed willing to advance the interests of the Institution, but also, perhaps even more so, to make the subject known by personally applying to such acquaintances as are able to procure articles of interest.

I remain, sir, yours, very respectfully,

LOUIS KR. DAA.

In answer to this circular, the directors of the Museum of Christiania have been informed that the Smithsonian Institution will co-operate with them by contributing ethnological specimens from its own collections, and by forwarding articles of a similar character which may be presented by others.

Newark, Ohio, *December* 8, 1862.

Dear Sir: In the last report of the Regents I notice the Smithsonian Institution proposes to prepare a map of that portion of the United States in which aboriginal antiquities are found. Feeling it to be the duty of every one to contribute to the general fund of knowledge, I take this opportunity of calling attention to two classes of archæological remains, which I have studied cursorily, and have not seen described in any work on the subject.

1. Remains of ancient cities and villages in Missouri. I have seen many of these. Whether large or small, they are similar in character. The remains consist of a series of tumuli, from one to two feet in height, and varying from sixteen to twenty-four feet square. These tumuli are in straight lines or rows, some numbering hundreds, and others, as of villages, tens; the rows cross each other at right angles, and the little mounds vary from four to eight rods apart. On digging into these mounds broken pieces of pottery are found, such as are common to all the antiquities of the country. In one instance an entire vessel was turned up. About the centre of each tumulus charcoal and ashes are found. I have examined several, and the pottery, charcoal and ashes are constants in all I have opened. Fredericktown, in Madison county, Missouri, seems to have been the site of a considerable city, extending from a branch of the Castor creek, which flows east of the village to near the east fork of the St. Francis, on the west, being perhaps a mile and a half long and a mile wide from north to south.

2. A different class of tumuli are common in southern Tennessee. The first I observed at the site of old Fort Pickering, two miles below Memphis. It is a parallelogram, some 15 to 20 feet high, 120 feet long by some 60 wide, and surrounded by a great many smaller works, just traceable, of various fancies and designs. I examined this in the winter of 1847–'48. Last spring I visited the site of the battle of Shiloh. This system of antiquities was very abundant there, but not so large as the one near Memphis. In a walk of half a mile I counted eleven of these parallelograms, generally 60 feet long and 25 wide on the top. Several of them were appropriated to the burial of our dead, killed in the battle of Shiloh. The smaller works are innumerable, and are generally circles. I found them all on the west bank of the Tennessee river, and the

most interesting on the south side of Owl creek. I noticed one at the village of Savannah, and a very remarkable circular mound, raised by a gradual and equal slope from all sides to the centre, which (the centre) I judged to be about 3 feet higher than the sides, and the diameter of the whole about 100 feet. I regretted that I had not more leisure to devote to the study of these singular antiquities, but my mission was one of mercy, and afforded me little time for antiquarian research or speculation.

When peace shall be restored to our distracted country, it would be well for some one or association to pursue a systematic and thorough examination of all our antiquities, and to trace the progress of those remains from the rude structures in Canada, south to Central America; for, from my own observations, I am satisfied that these remains attest a gradual improvement of the race or races that constructed them from the north to the south. The Missouri cities and villages were doubtless mere mud huts, and perhaps adobes, such as still are used in northern and New Mexico, which in central and southern Mexico and Central America are improved into structures of solid masonry, with sculptures and hieroglyphics. Whether these were all the work of one race of men, called by some the mound-builders, or of several distinct races, may never be tatisfactorily settled; but a systematic study of the whole will afford an inseresting chapter in the unwritten history of man.

Truly yours,

I. DILLE.

VALPARAISO, *September* 17, 1862.

DEAR SIR: In reply to your esteemed favor of the 23d July, I beg leave to state, what I had omitted in my former communication, that the case of skulls, &c., from Patagonia, was forwarded by me to the care of the United States consul at Panama, by the British mail steamer, expenses thereon paid by me in advance as far as Panama. I would gladly have done so to the port of delivery, but found no means of effecting it. By this time I shall hope that they are in your possession. Since you manifest an interest in the subject, I shall take an early opportunity of forwarding to the Institute the specimen of an *Atacama mummy*—one which I found myself several years ago in the neighborhood of the volcano of that name, and which I left deposited with a friend. I have ordered it to be sent to the coast, along with the utensils and articles of dress and use that were found with the body. At the same time I shall take the liberty of adding a few observations explanatory of the circumstances under which these relics are generally found, and the probable origin of the custom.

Very respectfully, your obedient servant,

AQ. RIED, *M. D.*

JOSEPH HENRY, Esq.,
Smithsonian Institution, Washington.

The account of the human remains mentioned by Dr. Ried in the foregoing letter will be found in the appendix to the present report.

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261 GREENE STREET, NEW YORK,
November 7, 1862.

DEAR SIR: I enclose herewith a draft of the proposed circular (philological) accompanied by the alphabet drawn up by Professor Whitney, and the standard comparative vocabulary used by Gallatin and Hale. It was at first my intention to modify and enlarge the latter, but on a full review of the subject, including a list of proposed words, and on consultation with Mr. Shea, I concluded not to do so without reference to yourself. With certain defects it is yet a very judicious selection, and it has already been used so largely that a change may now be injudicious. Still, if you think best, I will advise with Shea, Squier, and Bartlett, and with them prepare a catalogue of additional words as an appendix. I am of opinion, however, that the instructions will lead collectors in the right track.

I had also intended to have the circular translated into French here, but the health of the person from whom I expected this assistance is too bad to allow him to do it at present, and I do not wish to detain it longer. I would recommend that a translation be made into Spanish, (Mexican,) French, and German, both of this circular, if adopted, and the one on archæology. To save trouble with the vocabulary, I send a duplicate blank. There are many French priests both in the Hudson's Bay country and in Oregon; the Spanish, or rather Mexican, priests in New Mexico, California, and elsewhere, may prove valuable assistants, and among the Germans, even many soldiers in the army, are excellently fitted to collect, and well disposed to do so. It will be well to send a number, say one hundred copies, to the Bureau of Indian Affairs, to be distributed among the agents; an additional hundred to the Bureau of Topographical Engineers; a like number to the Surveyor General of the Land Office, for the use of his department; and to the Secretaries of State and of the Navy, for distribution among our consuls and officers bound to the western coast of Mexico, and elsewhere. The governor of the Hudson's Bay Company can, I presume, be also interested in the subject, and though last, not least, the governor of Russian America.

One thing that escaped my memory in preparing the archæological circular, was to say that due credit would be given to collectors. It will be better, however, if you prefix the whole by an official notice emanating from yourself to this effect. An acknowledgment from the head of an institution like the Smithsonian is a great inducement to exertion.

I see in the last annual report Mr. Morgan's circular respecting an archæological map. If you recollect, this was one of the points which I proposed to embrace in my work. I think not only of preparing a general map of North America, embodying the great families, but special maps of particular districts inhabited by a large number of tribes, included in a few families, who live within a small space. Such are Russian and British America, Washington Territory, Oregon, and California, for all of which I possess minute information. Mr. Bartlett has furnished me with the ranges of the New Mexican and Texan tribes, and I have material from other sources covering other parts of the country. Of course, all this, with the consent of the bureau, will belong to the Smithsonian, and I have no wish to monopolize the merit of such a work; but as I know that no one else possesses the material that I do, at least of the country west of the Rocky mountains, I should be unwilling to relinquish to another so important a task. Besides this, until the comparison of the languages is completed, the ethnological part of the map cannot be perfected.

I would, therefore, suggest that skeleton maps be issued, as soon as prepared, to various persons interested in ethnology, and who are familiar with particular regions, to be filled up with such information as they may possess, to be afterwards reduced, giving to each the credit of his contributions. Mr. Shea, Mr. Bartlett, General Charles P. Stone, Mr. Buckingham Smith, Dr. Hayden, and other gentlemen, can all add largely to such a work. I have already transmitted departmental maps to each Territory, under the sanction of the Indian bureau. Permit me, further, to recommend that the proposed map should extend far enough north to embrace all the Esquimaux tribes; and west, to take in the sedentary Tchuktchi or Namollos of Russian Asia.

I am very much afraid that the amount of information proposed to be contained in the ethnological map will render it confused. I would suggest whether it would not be better to have *two* maps—one showing the character of the country, whether forest or prairie, desert or arable, with the isothermal lines, &c; the other, its counterpart, containing merely the principal topographical features, such as the rivers and main chains of mountains, upon which the boundaries and names of the tribes and local names should appear. The first might indeed have designated upon it the boundaries of the families, but should not be colored. The latter to be colored and show the tribal subdivisions.

Begging to apologize for the length of this communication, I am, very truly, your obedient servant,

GEORGE GIBBS.

Professor JOSEPH HENRY,
Secretary Smithsonian Institution, Washington.

NOVEMBER 18, 1862.

In regard to the proposed map of the continent mentioned in your letter of the 13th, I have the honor to submit the following suggestions: The preparation of a base map to serve for these various uses is a subject of the greatest interest to every one concerned in scientific pursuits, and will form a lasting monument of the wisdom and efficiency of the Smithsonian Institution. The urgent need of such a one is evident from the fact that I have been unable to procure in this city a tolerably correct and recent map, embracing the whole continent on a scale of convenient size for ethnological purposes, and have actually been compelled to send to Germany for one. The *scale* recommended by Mr. Morgan strikes me as very suitable. In my remarks upon his propositions I meant only to object to the introduction upon a strictly ethnological map of the details of topography, meteorology, and hypsometry. My own idea, in which I am supported by other gentlemen engaged in the same pursuit, is, that an ethnological map should exhibit the principal features of the country, the rivers, mountain chains, and particularly the *passes* in the mountains, and the great Indian trails, where the nature of the country was such as to render these fixed and distinct; that it should also have the nomenclature fully given, in the popular form to enable collectors in the field to decide upon exact localities, but in such a type as to distinguish the popular from the true Indian names. Where, as I shall presently suggest, sectional maps on a larger scale are prepared, this nomenclature may, however, better be confined on the *general* map to a few main objects, such as the larger rivers, in order to avoid perplexity. Political divisions should be as few as possible and faintly indicated. Indeed, in an ethnological point of view, they are almost worthless, as ours are generally arbitrary and not founded on geographical features.

Besides this general map, I would also have a series of maps on a larger scale, comprising particular sections of country, having direct reference to the distribution of tribes and families. Thus, for instance, one map might show the country occupied by the different tribes of the Dakota, another of the Snake or Shoshonee, &c. The advantage of this would be, that whereas in the general map a single color must be used to indicate a great family, composed of numerous tribes, and its subdivisions could not be indicated without leading to confusion, these collateral maps could be made to exhibit the districts occupied by each. This is very important where the languages spoken by various tribes differ greatly, as among the Snakes, the Bannak from the Ute, and that from the Comanche. The scale on which these sub-maps should be constructed would vary greatly, depending upon two points: first, the number of tribes occupying a given region; second, upon the amount of minute information likely to be acquired. On the western coast of America, or that district lying between the Cascade and Sierra Nevada mountains and the sea, there are a great number of tribes, speaking quite a number of absolutely different languages, and of some of these there are various dialects, differing sufficiently to require designation. Having fixed and permanent villages, their nomenclature will be much more extensive than what we are likely to get from nomadic bands who roam in large numbers and cannot be followed up. The same state of things existed on the Atlantic, and of portions of the country we have still quite rich historical material.

You will see that the above will contemplate, in fact, an ethnological atlas, and that its preparation will be a matter of much time and labor, and occupy the attention of all those interested in the study. As regards expense, that need not be great at any one time, for the sub-maps, like the general one, might, in the first place, be prepared in skeleton, and distributed like the circulars to invite inquiry and contribution of material. As the work progresses, the topography may be filled in, for these maps will afford room to exhibit it in much greater detail than the larger one.

In fact, as regards a general map, even upon the scale proposed, and for purposes of topography itself, I doubt the propriety of going greatly into details. The best general European maps avoid this. The map of the Pacific railroad explorations, prepared under Mr. Davis's instructions, is almost useless from its very minuteness. All the principal features are lost in the details of topography. But, above all things, it appears to me that multiplicity of object should be avoided. Of course, a map showing the amount of rain per annum should exhibit the causes of variation in different districts, but this depends on great features and not on minute ones; and until it is shown that magnetics, for instance, influence the amount of precipitation, it would be improper to introduce lines of equal variation on a map intended to show those of equal rain fall. So with a general topographical or ethnological map.

I am engaged, with the assistance of the others, in drawing up the details which we think it would be well to include in the sub-maps, indicating the boundaries and scales, which I will forward, as soon as completed, for your consideration. Of course, I do not know how far you may be inclined to extend this subject, but the inquiry will at any rate be the means of ascertaining some valuable facts as to the amount of information at hand.

This leads me to another subject. I find the field which I at first proposed to myself has increased to such formidable proportions that, on consultation with Mr. Bartlett and others, I have concluded to propose the following scheme in its place: Professor Henry to request Messrs. Bartlett, Shea, Squier, Buckingham Smith, and such others as he may think fit, to unite with the writer in a comprehensive work upon the ethnology and philology of North America, to be published by the Institution, and to prepare materials for maps showing the location of the Indian tribes at various periods. The work can appear in parts, if thought advisable, as each finishes his portion. Mr. B. R. Ross would doubtless undertake British North America, (except the immediate coast,) including the Chepewyan family, the Crees, and Knisteneaux, and perhaps the Esquimaux, and prepare a memoir giving the history of the subject, and all that is valuable regarding those tribes. Mr. Shea to take the country east of the Mississippi, except Georgia and Florida, which might be assigned to Mr. Smith. Shea's knowledge and material exceed those of any one, on the people of this region, and his critical acumen is remarkable. Mr. Squier would assume Central America, and Mr. Bartlett, Texas and New Mexico. The writer to take the northwest coast, Washington Territory, Oregon, and California. There would remain the country intermediate between the Mississippi and Rocky mountains and Mexico. Mexico I would suggest should be assigned to some of the ethnologists of that country, with an invitation to prepare a general view of the subject as relates to it.

The work should embrace histography, ethnological divisions of families, founded on comparative philology, habits, &c., and psychology—in fact, to have as wide a range as there are reliable materials to work on. As it would take too long to await its entire completion, it might appear in a series of monographs, such as you have already published on various subjects, but the field is too wide for any one man to undertake an exhaustive work, embracing the whole.

I should explain that this suggestion comes entirely from myself, and that I am led to it by consultation with these gentlemen as to their views of the demands of ethnology in a work of this kind, not that they desired to invite such a request.

GEORGE GIBBS.

DECEMBER 26, 1862.

I had the honor to receive, in due course, your letter of December 18, informing me that the questions submitted in mine of December 3 had been referred to Professor Whitney, and shall hope to hear from him in reply.

Pursuant to the directions I received in a former letter, I wrote to Dr. Davis, requesting him to make any suggestions which might occur to him in regard to the archæological circular; but having received no reply, I presume that he is absent from the city. I called on Mr. Squier, who promised to send me his views on the same subject, but have not yet received them. I mention this as the cause of the delay in communicating the result to you. If I do not hear from you to the contrary, I will let the circular stand as it is.

In accordance with your desire that I should prepare a list of additional words to accompany the philological circular, I have gone into one at some length, in concurrence with Mr. Shea. We agree in submitting to you that the publication of this additional vocabulary be deferred for the present, and appear hereafter as a sort of supplement, when we shall have rendered it tolerably perfect. It may be advisable to extend it to some two thousand or twenty-five hundred words and phrases, some of them generally applicable, others to only particular parts of the country. The reason for this extension is as follows:

As regards nouns, the almost entire absence of generic terms renders it necessary that each object should be as specifically designated as possible; for instance, each particular kind of animal, tree, &c. Mr. Morgan's circular illustrates this point in respect to relationships, which are distinguished by singular complication and a great variety of names. In the pronouns there are not only absolute but copulative pronouns, sometimes both personal and possessive, the copulative being joined to or incorporated with nouns and adjectives or verbs, as the case may be. In some languages, at least, there are two and even three sets of cardinal numbers, one being positive or simple, another personal or applied to men, and still another to the counting of money. Again, of the verbs, the degree of detail into which these languages run may be seen from the fact that while there may be no abstract word for "to wash" or "kill," there will be found separate words for to wash the hands, face, and clothes, and to kill by stabbing, shooting with a bow, gun, &c.

You will therefore perceive that in order to arrive at any degree of precision it will be necessary to furnish quite a numerous collection of words, and that reference must be had in the selection both of these and the phrases to the idiom of the language and turn of thought of the speaker. To accomplish this in a way satisfactory to yourself will require some time, but in the mean while the present circular will perform its own more limited task.

The scientific names of animals, &c., should, of course, be given; but whether it will be best to undertake a translation of the whole into other languages is a question, for there are many words of daily use in Indian life which have no synonyms in dictionaries, or except in the various patois.

DECEMBER 26, 1862.

I owe you an apology for my omission to comply with your request that I should send you an account of what I am doing. It is, in brief, this:

I propose to give a connected series of vocabularies of all the known Indian languages west of the Rocky mountains, deriving from them a classification of the various tribes into families, and upon this basis to form an ethnological map of that part of the country. In addition to this, I propose to give a memoir upon the character, customs, &c., of those tribes with which I have been in direct communication, more especially as regards their habits of thinking, mythology, &c., and to include or append sub-memoirs by other persons upon particular districts out of my own range, and a *résumé* of the statistics of population at various periods so far as known. Of course the various authorities will be referred to, so as to give the bibliography, history, &c., of each section.

I have limited the above mentioned to the country west of the Rocky mountains, because I am satisfied that it is all that I can accomplish within a reasonable time, and that the labors of several investigators are required for an exhaustive discussion of what has already been collected in various parts of the continent. In a former letter I took the liberty of suggesting the allotment of other parts of this work to several gentlemen who have pursued separate examinations; such as Mr. Squier for Central America; Mr. Bartlett for Texas, New Mexico, and Arizona; Mr. Shea for the Atlantic section, except Georgia and Florida, which should fall to Mr. Buckingham Smith; and Mr. B. R. Ross, of the Hudson's Bay Company, for British America, except the northwest coast, which would come within my own field. Mr. Smith suggests El Exmo. Señor Don Fernando Ramirez, of Mexico, as the proper man to give the Mexican part, and thinks that he would willingly undertake it. It appears to me that the calling in assistance from Canadian and Mexican sources would not only add value to the contributions, but be a matter of policy as regards the Institution itself, making it a North American centre, instead of one confined to the United States alone. I need not say that the value of the ethnological series which you may publish will be greatly enhanced by the fact that each contribution is a specialty. It would, moreover, give the opportunity to make each paper exhaustive within its own region; Señor Ramirez, for example, giving the literary history of the Mexican tribes, as well as their philology and ethnology.

Mr. Ross's vocabularies, together with Mr. Kennicott's, are of the utmost importance in furnishing materials for comparison between the northern Chepewyan languages and the southern branches, which extend into New Mexico and Chihuahua. His notes are carefully prepared and well written. If you deem it desirable, I will forward them for your examination.

GEORGE GIBBS.

JANUARY 20, 1863.

I herewith enclose a memorandum of what is doing in the way of ethnology, so far as I am informed.

Mr. Shea has two more numbers of his series out, copies of which will immediately be sent you. One of them is the vocabulary of the San Antonio mission Indians, the one which Mr. Taylor denominates "Sextapay," but the correctness of which title is questionable. Mr. Shea has edited this with great care, rearranging the whole, as the manuscript was in a confused state. I beg to refer you to his preface, as also to the appeal at the end of the work. The other is Mr. Smith's *Nérome.*

The Sextapay, or San Antonio, is one of the numbers due on your contribution for 1861. Its publication has been delayed by the labor incident to putting it in presentable form, and by the necessity of casting some special type. This, together with the Mutsun and Yakama grammars, fills the programme for that year.

On your subscription for 1862 (on which no payment has yet been made) Smith's *Névome* is the first. The *vocabulary* of the Mutsun is now going through the press, and will be immediately followed by a number containing three of my larger vocabularies, the Chinook *proper*, the Clallam, and the Lummi, the last two being languages of the Selish family.

Shea proposes to follow up for 1863 with the Mohawk radicals, a valuable Jesuit manuscript, a Jesuit grammar of the Micmac, and my dictionary of the Nisqually. I trust sincerely you will find it convenient to continue your aid, for I am not alone in considering this the most valuable series of philological publications now going on. Mr. Moore, the librarian of the New York Historical Society, recently told me that neither England nor France could show anything to equal it.

The Chinook jargon is now finally in hand, and I trust to send you the proofs of the first signatures this week, as also of the circulars.

GEORGE GIBBS.

JANUARY 21, 1863.

Mr. Buckingham Smith called upon me to-day and showed me a letter from Don José Fernando Ramirez, of Mexico, from which I enclose an abstract:

"There exist no vocabularies of the languages, nor have the grammars ever been preserved, written by the early missionaries. It is almost impossible to bring together those that have been printed. On this subject a work has been commenced, entitled 'Cuadro descriptivo y comparativo de las lenguas indigenas de Mexico,' compendium descriptive and comparative of the native tongues of Mexico, by Don Francisco Pimentel. The first volume only has been printed, which comprehends the analysis of twelve languages. Unfortunately, material is wanting. Those contained in the first volume are the Huaxteco, Mixteco, Mame, Othomi, Mexican, Zapateco, Tarahumar, Tarasco, Totonaco, Opata or Teguema, Cahita, and Matlaznica. If you have succeeded in publishing the grammars of which you informed me, (Pima and Sleve,) and they should arrive in time, they will be examined in the work. I have not and am unacquainted with the 'Archæology of the United States, by Samuel F. Haven,' about which you write me. Of the Smithsonian Contributions I have only the second, third, and fourth volumes, unless the first volume should be 'Ancient Monuments of the Mississippi Valley,' which I possess. At present there is no way of sending books to Mexico, unless the Department of State will take charge of them."

Mr. Smith has handed me the above with the view that I might ask of you to send to Señor Ramirez such other papers of the "Contributions" as belong to archæology. That gentleman is well known as one of the most distinguished scholars in that department in Mexico, and one whom it would be desirable for the Institution to number among its correspondents. I am, however, astonished at the account he gives of the paucity of works on the indigenous languages of that country, so entirely opposite to our general belief here. Under any circumstances, Pimentel's work should be procured if possible.

I am, sir, very respectfully, your obedient servant,

GEORGE GIBBS.

Prof. JOSEPH HENRY,
Secretary to the Smithsonian Institution.

NEW YORK, *November* 1, 1862.

DEAR SIR: The Indian works now printing and to be completed before the close of the year, beside the Sextapay or San Antonio vocabulary, are:

I. The Mutsun vocabulary of Padre Felipe Arroyo de la Cuesta. This is a collection of phrases, but seems to include all the known words of the language, and, with the grammar already printed, will furnish all necessary means of comparing the language with others. To complete the subject, nothing will be needed but good comparative vocabularies of the Soledad, and other dialects.

II. The Micmac grammar of M. Maillard. Mr. Gallatin drew some ideas from an extract from fragments of this, but the entire work is necessary as the best known treatise of the most easterly branch of the Algonquin family.

III. The radical words of the Mohawk language by Rev. James Bruyas. This work treats the language on the system introduced by the Port Royalists, of learning the roots or radical words of a language and then deducing the derivatives. It divides the whole language into conjugations, and gives under each root the derivatives with many examples.

IV. An alphabetical vocabulary of the Chinook language by Mr. George Gibbs, in all probability the largest that will ever be made, as the tribe is fast vanishing. I have also in hand, and may have ready in time, some others, as—

V. Vocabularies of the Klallam and Lummi, by Mr. George Gibbs.

VI. A Névome or Pima grammar, edited by Buckingham Smith, esq. Next to these I wish to bring out—

VII. A Nisqually dictionary, by Mr. Gibbs.

VIII. An extremely valuable and ample dictionary of the Illinois language, compiled, I judge, by the Rev. Father Le Boulanger, and for extent, clearness, and variety, one of the most important labors of the kind known to us.

IX. Huron radical words by Father Carheil, revised by Father Potier, also a very ample and important work. Nos. 8 and 9 will each form a volume of 500 pages, such as the Onondaga dictionary published by me, and their publication is an undertaking of such magnitude that it can be carried out only by the active co-operation of those interested in philological studies.

I thank you for the information as to the forthcoming Cree grammar, which I will make note of in the Historical Magazine.

Your obedient servant,

JOHN GILMARY SHEA.

DEVON, SASKATCHEWAN, HUDSON'S BAY TERRITORIES,
July 4, 1862

SIR: I beg to forward to you the enclosed schedule, which I received about a fortnight since, and have now filled up according to the request of L. H. Morgan, esq. I have had some little difficulty in ascertaining the precise word for some of the relationships noted down, but I trust that the results of my investigations will be as free from error as can well be expected, and that the paper as now returned will meet the wishes of the gentleman who sent it to me. The postal arrangements of this country are in so primitive a state that I fear some months will elapse ere this letter and the enclosure reaches you; but it shall be despatched from here by the first opportunity that may occur. I have been living amongst the Cree Indians for ten years, and have long been so far acquainted with their language as to be able to preach to my congregations extempore. For some time past my scraps of spare time have been devoted to the work of compiling a *dictionary* of the native tongue, as nothing of the kind is yet extant, and my own experience in past years has taught me that it is greatly needed. I have now completed the first part, namely, the English-Cree, which contains very nearly 6,000 English words, with their corresponding Indian terms, and numerous idiomatic expressions; but I have still sufficient to occupy my disposable hours for many months in order to complete all that I contemplate. A friend had advised me to apply to yourself respecting the publication of the work when finished, and it was my intention to do so; but as the failing health of Mrs. Watkins has compelled me to seek permission to visit England next summer, I have declined doing so, as probably the Church Missionary Society, with which I am connected, may undertake to carry the work through the press while I am in my native land, and have the opportunity of correcting any typographical errors which may be made in the proof-sheets. Still you will perhaps allow me to ask if the Smithsonian Institution is in the habit of publishing such books as dictionaries of the native languages, and upon what terms it undertakes to have them printed, as I have no means of ascertaining this point. If you have any circular or pamphlet at hand explanatory of the principles and aims of the institution with which you are officially connected, I should feel much obliged by your kindly forwarding one to me.

Believe me, sir, yours, very obediently,

E. A. WATKINS.

Professor HENRY.

The writer of this letter was informed that the Institution does publish works of the kind mentioned, if approved by a commission of examination, provided that no other means exists of bringing them before the public.

DEER CREEK, NEBRASKA TERRITORY,
September 19, 1862.

HONORED SIR: As I am unable to express myself in English as well as is necessary and as I wished, I take the liberty of sending a German letter, and beg for a kind excuse and acceptance of the same.

When I arrived here two years ago, unfortunately, immediately after the disappearance of the missionary Brunninger, I found some publications of the Smithsonian Institution relating to various observations. I regret that I was neither in a position to read them cursorily through, nor had I time to even grasp their contents. When, however, time and other business did permit, I looked into them carefully and found great pleasure in so doing, for which reason I also recognize it as my duty as far as I am able with my feeble powers to show my gratitude. Only I must also add with much regret that understanding and apparatus are wanting to me in many branches. Because, however, I stand here so ignorant, I considered it to be well and necessary first to ask whether or with what subjects I could render to your honored institution my feeble services. One point which I in the first place considered as appropriate is the language of the Shyenne Indians. I have now passed a year with them in a capacity which is well known to you, and for a quarter of a year I have travelled about with them for the purpose of learning the language, but have still learned comparatively little. I permit myself, however, to contribute to your honored institution a small extract from the treasure which I have learned and collected for their kind consideration. Should it be acceptable to your honored institution, I will, if you desire, send more. In the mean time I remain, honored sir, with the highest esteem, your obedient,

GEORGE FLACHENECKER,
Evangelical Lutheran Missionary.

To the SMITHSONIAN INSTITUTION.

HONOLULU, *July* 15, 1862.

MY DEAR SIR: I am in receipt of your favors, dated April 14 and 21, enclosing an order from Professor Bache for tidal apparatus, which I forward to

San Francisco by this mail. I am in hopes to receive it, for I am not aware that any observations have ever been made, at a position similar to that of our islands, a long distance from a coast line.

I have also received the reports of the Smithsonian Institution for the years 1854–'60, check lists of American shells, and catalogue of publications of societies, for which I beg to return my sincere thanks.

I forward you, per this mail, a catalogue of the works in my library relating to the Sandwich Islands, which I believe to be near, if not quite, complete. You will notice in it three periodicals, formerly published at our islands, not in your catalogue. They contain a few scientific articles. Please look over the catalogue, and any works published here which you may wish to obtain, please inform me, and if possible, they will be sent you.

Your wishes in regard to a series of shells described by me shall be attended to. I also shall furnish you, as soon as I can obtain it, a specimen of the bat living on our islands, (the only indigenous mammal here,) for the reason that I received a letter by last mail from Dr. Gray, of the British Museum, acknowledging the receipt of one from me, which he decides, after a hasty examination, to be identical with a species common to the east and northerly part of America, usually called the "New York Bat." He was to exhibit it at the meeting of the Zoological Society the evening of the day he wrote me. If he is correct, it will be a singular exception to the laws of animal distribution.

I am about to commence the publication of a serial work in England, on the Natural History of the Pacific Islands, which will be furnished you from there.

My illustrated catalogue of the shells of the Sandwich Islands and their animals must be deferred for a time, as my collector on the islands south of the equator, who has been occupied near three years in searching them, informs me that he has been very successful, having obtained 600 new land and marine species, and discovered facts of great value to me in regard to distribution of species, &c.

I notice in your report of 1860 an announcement of the intended publication of several pamphlets on shells, three by Mr. Carpenter. The one on west coast species and one on United States expedition shells I particularly wish to see, as the latter I shall be able to correct. All pamphlets, however, on shells will be of use to me.

I beg strongly that duplicates of shells from the Indo-Pacific province may be sent me by the Institution; full value will be returned. Are there none left of Wilkes's expedition, or of Rodgers's Japan expedition?

All packages in future, please address to me and forward to Wells, Fargo & Co., New York.

Do not fail to make use of me in any way you may consider of value to the Institution.

Yours, most truly,

W. H. PEASE.

Professor JOSEPH HENRY.

The books and specimens referred to in this letter have been received at the Institution. The species of bat so remarkable, as being the only native mammal found on the Sandwich Islands, has since been identified by Dr. Gray, of the British Museum, as the *Lasiurus Grayi*, belonging to the coast of Chile.

HEADQUARTERS, GENERAL GRANT'S ARMY,
Jackson, Tennessee, November 5, 1862.

DEAR SIR: A large and perhaps valuable, but incomplete, herbarium has fallen into my hands, captured from the confederates, or, at least, belonging to some institution of learning, and wanting an owner. It consists of about a dozen thick folio volumes of plants belonging principally to *West Tennessee*, very neatly arranged. They were gathered together by a friend of mine, and as the soldiers were destroying them, I have taken charge of them, with a view of presenting them to some scientific institution. Please inform me if it would be worth while to send them to the Smithsonian Institution, and let me know whether I shall forward them by Adams's Express, so that they will go safely, and whether I must pay the charges, &c.

Respectfully, &c.,

H. R. WIRTZ,
Surgeon United States Army, Medical Director.

Professor HENRY,
Smithsonian Institution.

P. S.—I have them in a box about 3 feet by 2½.

The collection of plants above referred to has been received, and will be carefully preserved separately until the close of the war. No information has been obtained as to the original owner or collector.

LEIPZIG, *May* 31, 1861.

MY DEAR SIR: In sending back, through Dr. Flügel, agent of the Smithsonian Institution, the ferns which have been communicated to me by Dr. Eaton, in your name, I cannot omit to express my warmest thanks, not only for the kindness shown me in this instance, but also for the collection of ferns destined for the herbary of the university.

I am under a great obligation to the Institution for having given me an opportunity of examining these ferns, whereby it has materially assisted me in my studies.

Accept the assurance of my deepest gratitude and the highest esteem.

Yours,

G. METTENIUS, *Professor.*

Professor Dr. HENRY,
Secretary of the Smithsonian Institution.

The ferns sent back are the *uniques* of Brackenridge's collection, and will be placed with the rest of the collection now in Dr. Torrey's hands.

DANIEL C. EATON.

NEW YORK, *January* 13, 1863.

GENEVA, *Switzerland, May* 25, 1862.

DEAR SIR: Your letter of the 28th of February was duly received, and that it has not been sooner answered must be ascribed to the throng of pressing occupations in which I have been absolutely absorbed. I am under great obligations for the interest which you have been pleased to take in procuring for me the books and *duplicata* of your mammifers which I had requested.

In regard to the catalogue of the Hymenoptera, I regret to say that I have not reached it, having been engrossed by various other labors. I have commenced a large work on America, of which I have had the satisfaction of sending to the Smithsonian Institution the two first parts, the Crustacea and the Myriapodes. Since then I have been engaged with the Orthoptera, and have proposed next to proceed with the Hymenoptera. The plates of the Orthoptera will be forthwith sent to the engraver, but I have been dreadfully retarded by a succession of mishaps. My original draughtsman died; another whom I had trained quitted this service to engage in that of the railroads; a third found a lucrative place and left me to shift for myself, and the fourth threatens to do the same thing. Nevertheless, I hope to get through some day or other.

My taxidermist, whom I had left in Mexico to complete my collections, after having done nothing but cajole me for several years, has ended by leaving the country without sending me anything of consequence. All this has greatly hindered me. Still, my descriptive treatise on the Vespidæ of America is ready, but I wish to revise the manuscript and correct it from beginning to end, for it is my custom to leave my manuscripts in the drawer for one or two years and then remodel them by means of the materials I have in hand. It is only in this way that zoological works can be worthily composed. If you are urgent, however, I will leave the Orthoptera in order to take up the Wasps and will send you the manuscript this winter. Please indicate to me your wishes on this subject.

As to the other families, I cannot undertake them till I have done with the Orthoptera and the Wasps. But as you seem in haste, I think your best course would be to intrust this work to some American whose special line of study lies in that direction, and Mr. Edward Norton is well qualified for it. With the same view I have already prevailed on him to take in hand the Ichneumonidæ.

It would be impossible to make purely and simply a *catalogue* of the Hymenoptera. The number of known species is much too restricted. There would be needed a descriptive work, and you could not acquit yourself of it under less than ten large volumes. The labor upon the Hymenoptera is a colossal one. When I shall have published the Wasps, I will see whether I can undertake another family for you, and believe that I can; but, trust me, you must proceed *by families* or you will have nothing satisfactory.

I take this opportunity of informing you that in July I shall forward to you a package containing three memoirs of mine and my map of Mexico; and in addition, certain books for several learned Americans. Have you Saussure's Treatise on Hygrometry? Be so kind as to have the books distributed accord-to the address of each.

Please accept, dear sir, my cordial good wishes and the expression of my entire esteem.

D. H'Y DE SAUSSURE.

P. S.—If you have still any Vespidæ to send me to complete my manuscript, it will be necessary to do so soon, that I may be enabled to employ them as materials. My manuscript will form a volume in 8vo.

You will inexpressibly oblige me if you will have the recent American work on Tehuantepec sent to me.

We have since learned that the manuscript work on the *Vespidæ* or Wasps of America, mentioned in the foregoing letter, has been completed. It will be published as soon as practicable after it has been received and translated from the French

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New York, *September* 16, 1862.

Dear Sir: My brother wishes me to address you in regard to a *cabinet of minerals* which he would like *to sell* to the Smithsonian Institution. He has a collection of minerals, mostly Californian and Mexican, between 2,000 and 3,000 in number, and containing about $1,500 in gold and silver. His reason for selling is, that it is entirely too valuable to retain in his office, as he is afraid of being robbed. He asks $3,000 for it, and refers to Professor Whitney, of California, for an examination.

You would confer a favor by returning an answer to this note when convenient; and perhaps, if the Smithsonian Institution does not want the collection, you may be cognizant of some other institution which might desire to obtain it.

Very respectfully, yours, &c.,

Professor HENRY. ———.

The foregoing is one of many propositions to sell specimens of natural history, &c., to all of which the answer has been made that the Institution does not purchase articles of the kind.

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The following letter was received from M. Romero, Mexican minister, in reply to a request made to him to furnish a letter to facilitate the explorations of Mr. Xantus, in Mexico:

WASHINGTON, *December* 4, 1862.

DEAR SIR: I have the honor to acknowledge the receipt of your letter of the 1st instant, informing me of the object of the appointment of Mr. John Xantus as United States consul at Manzanillo, and asking me to furnish him with such letter of introduction for the governors of the States of Colima, Michoacan, and the adjacent ones, as may help him in the prosecution of the scientific investigations he intends to make in a portion of western Mexico whose natural productions are very little known.

Being desirous to contribute to the success of Mr. Xantus's scientific researches and labors, I enclose you herewith letters of introduction for the governors of the States of Michoacan, Jalisco, Colima, and Sinaloa, which I hope will fully answer his purpose. Should he desire letters for any other governor, I will furnish him with them as soon as you let me know it.

As regards the entrance of the scientific apparatus Mr. Xantus may take with him to be used in making collections in natural history for the museum in charge of your Institution, I am happy to say that I think he will not have any difficulty with the customs authorities at Manzanillo, such articles being free from duty according to the Mexican tariff.

I am, sir, very respectfully, your obedient servant,

M. ROMERO.

Professor JOSEPH HENRY,
Secretary of the Smithsonian Institution, &c

EULOGY

ON

HON. JAMES A. PEARCE, OF MARYLAND,

UNITED STATES SENATOR,

ONE OF THE REGENTS OF THE SMITHSONIAN INSTITUTION,

BY

PROF. A. D. BACHE, LL. D., Superintendent of the U. S. Coast Survey.

At a meeting of the Board of Regents of the Smithsonian Institution, held January 31, 1863, Professor Henry, the Secretary, announced the death of Hon. James A. Pearce, one of the Regents.

Prof. Bache, after appropriate remarks, offered the following resolutions, which were unanimously adopted:

Resolved, That the Board of Regents of the Smithsonian Institution deeply mourn the loss of their distinguished fellow-regent, JAMES ALFRED PEARCE.

Resolved, That in the death of Mr. Pearce our country has lost a refined and influential citizen, the Senate of the United States an able, judicious, honest statesman, and this institution an active, intelligent, and learned Regent.

Resolved, That we sincerely condole with the afflicted family of Mr. Pearce, and offer to them our heartfelt sympathy in their great bereavement.

Resolved, That a copy of these resolutions be communicated by the Secretary of the Smithsonian Institution to the family of the deceased.

On motion of Mr. TRUMBULL, it was—

Resolved, That Professor Bache be requested to furnish a copy of his remarks in relation to Hon. James A. Pearce, for insertion in the journal of the Board of Regents.

EULOGY.

Again has death invaded our circle, and taken from our councils and our active sympathies one of the most admirably gifted intellects which has at any time been called upon to shape the destiny or direct the labors of the Smithsonian Institution. A member of the executive committee from nearly the second year of the organization under the act of Congress of 1846, attentive to every detail, whether scientific, administrative, or financial, Mr. Pearce was always prompt at the call of every duty. His entire and cordial acquiescence in the form of organization adopted for the Institution, his liberal and zealous co-operation with the Board of Regents, his earnest support of, and unfaltering confidence in, the discretion and integrity of its Secretary, were as conspicuous as they were productive of the most lasting and important benefits. And though it is true that the general form and policy of the Institution were determined under the authority of Congress, by its first Board of Regents, yet it is quite as certain that strenuous action was afterwards needed to maintain it in its adopted course, and secure it from projected innovations which, though strenuously advocated at the time, few now regard with aught but disfavor. To this end no one lent more effectual aid than our lamented colleague. Although, from taste and the conditions of his active life, he might more properly be styled a literary man, yet were his scientific attainments by no means inconsiderable, and a liberal and cultivated mind, which admitted of no narrow views, enabled him to embrace, in all its comprehensive simplicity, the idea of the generous foreigner who, in founding this Institution, consecrated his fortune to "the increase and diffusion of knowledge among men."

In whatever Mr. Pearce engaged he exhibited the same spirit. Marked as a leader from his boyhood, at school as at college, in his profession as in the councils of the nation, in his neighborhood, his State, his country, as well as in the church to which he had dedicated his faith, he stood distinguished for an enlightened estimate and an efficient support of whatever is elevated and calculated to elevate. To him the work of construction was ever far more congenial than that of demolition; to improve and preserve was an instinct, to confound and destroy, an innate aversion of, his nature. Refined in his tastes, brilliant in society, instructive from the affluence of his ideas and extent of information, without ostentation as without pretension, social, genial, even playful among his intimates—such was the associate whom we must long mourn, feeling that at the council board as in the familiar and friendly circle, we have lost one who strengthened us in our adhesion to what is right, good, or true, while ever prompt to lead us wherever progress held out rational hopes of improvement.

Generally, men of the temperament we have described are impatient of details; but this was not at all so with our departed friend. It afforded him pleasure to systematize and reduce to order even the dry details of finance, and a wonderful memory and a quick perception enabled him to pass them in rapid review with a scrutiny of every particular. His mental vision was as minute as comprehensive, and his analytical faculty never dismissed a subject of investigation until he was thoroughly satisfied with the arrangement, the method, the results: in a word, he was content with little less than the perfection of whatever occupied his attention or claimed his solicitde.

The objects which in Congress occupied most of his attention, and which it gave him most pleasure to defend and sustain, were those connected with literature and science, and in these he showed the same qualities which as chairman of our executive committee he has here so often exhibited. With the great interests of state and the high objects of national politics he was abundantly qualified to grapple; in fact, he shrunk from no occasion in which to exert himself when enlarged views and skilful powers of debate could be rendered serviceable to his country or the world. But if duty called upon him from time to time for such efforts, still it was to objects promotive of art and science and high civilization, to means for man's moral and intellectual improvement, and for the enlargement of his knowledge and power over nature, that he turned with ever new and unwearied interest. To him probably more than to any other senator the library of Congress was indebted for the augmented fund which it has now for some years enjoyed, and for the care taken in the selection of the materials which render its shelves so useful. The exploring expedition was more than once indebted to his earnest and persistent efforts for the continuance of the means of publication of its results; the Coast Survey for expositions of its importance to the country and the world; the Smithsonian for warding off assaults, and reconciling enthusiastic but misguided opposition; the naval and military expeditions, boundary surveys, and explorations, for close, searching investigations, which led to important improvements and to cordial support. The great work of the extension of the Capitol found in him a wise advocate and judicious friend. Not afraid of what was new, he yet aimed at nothing for the sake of novelty. In connexion with the decoration of our public buildings, our sculptors and painters found in him a most enlightened appreciator of their works, and one always ready to promote the great cause of their art by legitimate means.

He had a remarkable power of attaching to himself men of science, literature, and art, and, in return, found in them some of his most intimate and highly-prized companionships. His friendships were warm, and once formed, were proof against all trials of absence or change of fortune. Many of his ardent attachments reverted to the friends and associates of his parents, and to family relations of even an older date, acquiring in his breast a sacred title by the claims of the past.

The genial elements of his character naturally expanded most freely in the circle of his family and friends, where he was truly and ever at home. His garden, its fruits and flowers, were his habitual delight; his farm and its operations seemed to touch by association the springs of his deepest affections. He superintended every process with a judgment rarely at fault, and watched all the varied developments of nature with the interest of the amateur or the naturalist. Whoever had not seen Mr. Pearce in his dwelling, in his garden, or upon his farms, knew him but imperfectly.

JAMES ALFRED PEARCE, the colleague, the counsellor, the friend, to whom we must now bid a final adieu, was born in the town of Alexandria, then part of the District of Columbia, December 14, 1805. His parents, who were of Scottish descent, and citizens of Maryland, dying during his childhood, the care of his education devolved upon his maternal grandfather, the late Dr. Dick, of Alexandria, an eminent physician of that day, who will be remembered by the student of American history as having been one of the medical attendants who ministered at the dying bed of Washington. So rapid yet thorough was the progress of the young student in the rudimentary stages of education, that he graduated at Princeton College at the boyish age of seventeen, bearing away from competitors of no ordinary ability, and much subsequent distinction, the highest honors of his class. Having adopted the law as his profession, and permanently settled at Chestertown, Maryland, the former residence of his parents, he soon received the earnest of future success in the confidence, affection, and support of the community—a community to whose favor he might, indeed, already look forward in virtue of the memory of a meritorious and distinguished ancestry. His first step upon the more public stage which was thenceforth to be the scene of his labors and success was his unsolicited election to the legislature of Maryland, in 1831. From that day, with a single interval of two years, his talents and time were devoted to the service of his fellow-citizens in the halls of legislation, his career having led him, by a progression founded on the uncanvassed but ever-increasing confidence and respect of the people, through the House of Representatives to the Senate chamber, in which he was fulfilling the unexpired term of a third election at the period of his death.

His characteristic qualities and tendencies as a legislator have been already slightly touched upon in this memorial, but whoever recalls the momentous events, the gigantic and often acrimonious struggles for ascendency, the portentous and brilliant debates which, from 1835 to 1861, fixed the public attention, and excited the alternate hopes and fears of contending parties; whoever pictures to himself the majestic forms which then occupied the legislative arena, will remember that, through all these events, and measuring himself in no unequal competition with the foremost men of that earnest time, our colleague continued to advance steadily in public appreciation, to fill a yet wider and wider space in the eyes of the country, that on him rests no imputation of having ever purchased favor or advancement by a sacrifice of the slightest principle, or of having once deviated into any of those equivocal positions which sometimes bring disrepute on illustrious names; whoever shall recall and consider these things will undoubtedly be qualified to form a more adequate and vivid conception of his labors and his worth than could be derived from any portraiture which this occasion would permit, or perhaps even the most labored eulogy could supply.

Nor were striking testimonials wanting to his peculiar and conspicuous merits: it rested but with himself to have occupied positions of the highest public distinction. A place in the cabinet and a seat in the federal judiciary were successively offered him; on more than one occasion his name was publicly canvassed in connexion with the presidency of the United States. The former, however, he declined; the latter he steadily discountenanced. He seems to have felt that the Senate chamber was the proper sphere for his peculiar tastes and powers—a sphere equal to his well-regulated ambition, not below his admitted merit. The patronage incident to the executive branch of government involves much that would have been repugnant to his feelings; the judiciary has objections peculiar to itself in the ever-recurrent and monotonous nature of its functions; the representative department of Congress was for him too much influenced by the fluctuations of popular opinion. The Senate, in the stability of its tenure, and the vivacity and variety of its discussions, in its character of a consultative and executive as well as legislative body, in the dignity and importance of its deliberations, involving the interests of States and the relations of national intercourse, seemed exactly fitted to give scope to his abilities, and to satisfy every aspiration he might indulge for usefulness or consideration. Perhaps it was in the committee-room that his influence made itself more particularly felt, for here the extent of his information, the weight of his character, the directness and integrity of his purpose, his patience for details, his familiarity with the forms of business, and aptitude in applying them with logical acuteness to the disentanglement of questions of fact and law, his co-operative spirit, his genial and companionable nature and manner, all conspired to give authority to his decisions, and to conciliate reliance and acquiescence on the part of those with whom he acted.

Had Mr. Pearce not embraced the profession of law, he would doubtless, under suitable circumstances, have been celebrated as an agriculturist. Had he not resigned himself to political life, he could not have failed of eminence in science or in literature. It is indeed rare to meet with one whose capabilities and excellencies were so varied and so distinct, nor is it possible that, knowing him as I have done, I should speak of him otherwise than frankly and from the heart, though conscious of the imperfect representation which I have been able to give of a man so intrinsically great in all the elements which constitute true greatness, so entirely beloved for all that refers itself to the amenities of social intercourse and the sacred endearments of home.

In conclusion, it is proper to add that the peculiarities which marked his character during the active years of his life exhibited themselves in the closing period of his career under a new but harmonious aspect. Afflicted with an incurable malady, he contemplated his approaching end and endured his intense suffering with the unwavering faith and resigned patience of a Christian. The religious principles which he had imbibed in childhood, and which had perhaps imperceptibly formed the basis of his character, became the dominant objects of his thoughts, and the consolation and happiness of his last hours.

GENERAL APPENDIX

TO THE

REPORT FOR 1862.

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 THE

UNDULATORY THEORY OF LIGHT,

By F. A. P. BARNARD, S. T. D., LL. D.,

LATE CHANCELLOR OF THE UNIVERSITY OF MISSISSIPPI.

[In preparing the material of these lectures for publication, some transpositions have been made in the original order of topics, and the lecture form has been generally abandoned. Mathematical illustrations have also been occasionally introduced, which would not have been quite in place before a popular audience.]

PART I.

INTRODUCTORY.

OUTLINE OF OPTICAL DISCOVERY.

The knowledge which we possess of the material objects surrounding us in the universe is principally received through the sense of vision. From the other senses we derive a much more limited range of impressions. The touch furnishes us with a valuable means of confirming or correcting the information we receive from sight; but its usefulness extends only to objects in our own immediate vicinity. The hearing, though through it, by the aid of spoken language, we are supplied with a vast multitude of ideas which have had their origin in impressions previously made upon other senses, contributes of itself, in any other form, but very slightly to the great stock of our knowledge.

Such therefore being the pre-eminence of vision among the senses, light, which is its medium, is, and has ever been, the most important of physical instrumentalities in promoting the intellectual development of the human race, and making progress a possibility. But, while occupying this peculiar relation to the history of our advancement in the knowledge of nature, while so fertile in the revelations it has unfolded to us of the properties and qualities of other things, it is remarkable that light has itself furnished, in its own nature, one of the most difficult and perplexing of all the subjects of physical inquiry; so that, even down to an advanced period of the present century, the world of science may be said to have been upon no other subject more widely at variance than upon the elementary and fundamental question, What is light?

Nor is it possible to explain this want of harmony by supposing the inquiry to have but recently originated. Since, in the physical world, light has been the ever present and ever most efficient handmaid of the human understanding, its phenomena must, to some extent at least, have attracted the attention of the first intelligent inhabitants of our planet. The first man who breathed could not have failed to notice the images of visible objects, formed by reflection in the bosom of every quiet pool; and the first rude navigator who endeavored to float himself from shore to shore, across waters too deep to be traversed by ordinary and simpler expedients, must have been struck by the singular distortion of his paddle at the line where it entered the water. The natural alternations of light and darkness, their coincidence with the rising and setting of the sun, the appearance and disappearance of the stars, the changes of the moon, the rainbow in the clouds, the differences between different bodies as luminous and non-luminous, transparent and opaque, and finally the very fact of vision itself—all these phenomena constantly, from the earliest times, presenting themselves without being sought, must have excited the curiosity of men and invited investigation, centuries before the systematic study of nature, in any of her varied departments, had had a beginning.

But the difficulties which perplex the inquiry manifest themselves in the imperfection of the speculations which have come down to us from the earliest philosophers regarding the subject, and in the extremely slow progress of discovery which marks much of the later history of this interesting branch of science. A notion was for a very long time prevalent among the ancients that vision is effected by means of rays proceeding from the eye to the object. This idea is not found in Aristotle; but it was introduced into the school of Plato, and continued to be received for many centuries. The persistency of the doctrine is remarkable, inasmuch as the light which is self-evidently indispensable to vision, proceeds from sources foreign to the observer.

The elementary phenomena of reflection and refraction suggest a natural division of the science of optics into two branches; and this distinction is made by the earliest systematic writer on the subject whose works have descended to us. This was Euclid—supposed to have been the geometrician of that name—who lived about 300 years before our era. The general laws which govern the reflection of light, being comparatively easy of detection, were stated by him with tolerable correctness; but what he has written on refraction is of little value.

Ptolemy, the astronomer of Alexandria, who was born about the year 70 of our era, attempted to discover the law of refraction by experiment. His apparatus was ingenious, and was not different in principle from that which has been employed by Silbermann, Soleil, and others, in our own time, for the same purpose. He measured the angles of refraction corresponding to various angles of incidence, between 0° and 90°, for both water and glass, and left his measurements recorded in his System of Optics. In order that we may judge of the degree of accuracy attained by him it is necessary to anticipate what is to follow, so far as to define, in this place, a few technical expressions. By the *angle of incidence* made by light falling upon a reflecting or refracting surface, is meant the angle between the *ray* and the *perpendicular to the surface*. By the *angle of refraction* is meant the angle between the ray which has passed through the surface and the same perpendicular on the other side. By the *angle of reflection* is meant, in like manner, the angle between the reflected ray and the perpendicular. By the *plane of incidence*, the *plane of refraction*, or the *plane of reflection* is meant the plane which contains the *incident ray and the perpendicular*, the *refracted ray and the perpendicular*, or the *reflected ray and the perpendicular*. All these planes are coincident, except in cases where *double refraction* takes place, when one of the planes of refraction is *not* usually coincident with the corresponding plane of incidence.

As a measure of the amount of deviation or change of direction produced in a ray by refraction, the *sine* of any given angle of incidence is divided by the *sine* of the corresponding angle of refraction, which latter is determined by observation. The quotient is *constant* for the same substance, no matter what be the angle of incidence taken. It is called the *index of refraction*. The constancy of this quotient was not known to Ptolemy. The discovery of its constancy, at a comparatively recent period, marks an era in the history of the science; and it was, as we shall see, the discovery of the *law* of refraction.

The ascertained index of refraction for water is 1.33582. If we make a computation of its value from the measured angles of Ptolemy, we find a mean of 1.30147. But if we take his measurements at the incidence of 50°, where the relative variations of the angles of incidence and refraction are most marked and most easily measured, we obtain 1.33555, which is exceedingly near the truth.

The true index of refraction for glass is between 1.48 and 1.60, according to the materials and density. Crown glass varies from below 1.50 to about 1.525. Ptolemy's mean determination would be 1.484. But at 50° he approaches nearer the truth, his angles giving 1.5321.

For rays passing from water to glass, the relative index computed from his measurements would be 1.1390, the true being 1.14145. The near agreement of these numbers with modern determinations is remarkable, especially considering that Ptolemy's measures are given only to the nearest half degree.

Ptolemy was unable, however, to derive any practical advantage from these results, since the magnitudes of the angles seemed to be governed by no law which he could detect. And in this unsatisfactory condition the whole subject of refraction remained for the fifteen succeeding centuries.

As an astronomer, Ptolemy could hardly fail to notice the effect of atmospheric refraction upon the apparent positions of the heavenly bodies; and he has the merit of having recognized the fact, which others after him disputed, that the displacement is always in a vertical plane, and also that it attains its maximum in the horizon and is zero in the zenith.

About half a century later than Ptolemy flourished Claudius Galen, the celebrated Greek physician. In a treatise on the uses of the members of the human body he speaks at some length of the phenomena of vision, and lays down the fundamental law on which the stereoscope has been very recently constructed, that the picture which we see of a solid body is made up of two pictures dissimilar to each other, one seen by each eye separately.

But it was impossible that optical science should make any important progress so long as the law which determines the path of a ray in passing from one medium to another remained unknown. We are compelled, therefore, to descend to the earlier portion of the 17th century before we find a practicable ground on which to build a systematic science, or lay even a foundation for the splendid superstructure which the future had in reserve in this department of physical inquiry. In the year 1626 Willebrord Snellius, professor of mathematics at Leyden, died at an early age, leaving behind him manuscripts, among which was contained a statement of the important law in question under the following form:

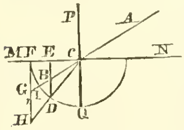

Fig. 1.

If MN be a plane horizontal surface, dividing a denser medium below it from a rarer one above, and if a point at D be observed by the eye at A, the apparent place of D will be at B, vertically above D, in the line AC produced; and whatever be the inclination of the ray to the surface, the line CD will be to the line CB in a constant ratio. Or, if CD be made the radius of the circular arc FDQ, and DE be drawn perpendicular to the surface, the radius CI, being the visual ray AC produced, will be divided at B in a constant ratio. If, at F, we draw to the circle the tangent FH, producing CD and CB to meet it at H and G, then CH and CG, which have the same ratio to each other as CD and CB, will be the secants of the angles HCF and GCF, or the co-secants of the angles HCQ and GCQ, (=ACP,) formed by the refracted and incident rays with the perpendicular, PQ, to the refracting surface MN, technically called the angles of refraction and of incidence. The geometrical law of Snellius, therefore, translated into the language of trigonometry, is this: That when a ray, passing from one medium to another, undergoes refraction at the common surface, the ratio of the co-secant of the angle of incidence to the co-secant of the angle of refraction is constant. As the co-secants of angles are inversely as the sines of the same angles, the law may be more conveniently expressed by saying that, in the circumstances supposed, the *sines* of the angles mentioned are in a constant ratio. It was in this form that the law was first published by Descartes, eleven years after the death of Snellius. It is, therefore, frequently referred to as the law of Descartes.

It may be proper to mention that, previously to the discovery of this important law by Snellius, it had been remarked by the illustrious Kepler that for incidences below thirty degrees a ratio almost constant exists between the angles of incidence and of refraction themselves. This is true because for small angles the increments of the arc and of the sine are nearly proportional. But when the incidence is moderately large, the divergency of the two ratios becomes very wide.

An examination of the figure given above will show that the refraction of a plane surface produces no distortion in lines which are at right angles to the surface, but only diminishes or increases their apparent length according as the medium in which the object is situated is denser or rarer than that on the side of the observer. Thus the line ED is reduced to the apparent length EB. The amount of this reduction increases with the obliquity of the visual ray, for the ratio of CD to CB, which is constant, is always less (except when the incidence is perpendicular) than the ratio of ED to EB, and the divergency of these ratios is always increasing. It follows that the apparent depth of a fluid is always less than the real depth, and that the illusion is more striking in proportion as the point observed is more remote from that immediately beneath the eye. Thus the horizontal bottom of a cistern or pool of uniform depth presents a curved appearance like that here represented. If MN be the surface and KL the horizontal plane at the bottom of a sheet of water, the eye being placed at the point P above it, this plane will present a conchoidal appearance like that of the curve D′E′A″. The position of the points of the bottom which, to an eye situated at P, appear in the directions PD, PE, &c., may be found by a simple geometrical construction. Drawing the perpendicular PAA′, divide the depth AA′ at the point A″, so that AA′ shall be to AA″ in the ratio of n to 1:—n being the index of refraction. Through A″ draw VW parallel to the surface. Produce PD, PE, &c., until they intersect the bottom at G and H, and with the radii DE and EH describe the circular arcs GG′ and HH′. Through G″ and H″ where DG, EH, intersect VW, draw perpendiculars to the bottom, intersecting the arcs in G′ and H′. Join DG′ and EH′. The points D′ and E′, where the joining lines intersect the bottom, are the points which will be seen from P in the directions PD, PE, and the apparent positions of those points will be at D″ and E″, where the visual rays PD and PE produced meet perpendiculars drawn from D′, E′, to the surface.

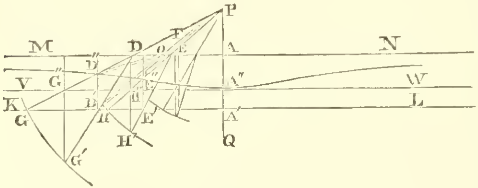

Fig. 2.

Any number of points being thus found, the curve drawn through them all will show the appearance of the level bottom M′N′ as it is seen from a point above the surface as P. This curve is a conchoid, whose polar equation is

$$r = p\sec\varphi + \frac{q}{n}\sec\varphi' ; \quad \text{or, } r = \frac{p}{\sqrt{1-\sin^2\varphi}} + \frac{q}{n\sqrt{1-n^2\sin^2\varphi}} ;$$

in which p is put for PA, q for AA′, n for the index of refraction, φ for the angle EPA, and φ' for EE′E″.

It is apparent from the foregoing that all lines seen through a single plane refracting surface, unless they are perpendicular to the surface itself, are more or less distorted. A straight rod partly immersed in water, as FD′, appears sharply bent at the surface, and slightly curved beneath, assuming the apparent direction OD″. Moreover, though, as in this case, the real direction should pass through the eye, so that in a uniform medium, only the extremity could be visible, the effect of refraction gives a lateral view of all the part immersed.

The next important step in the progress of optical discovery, after the detection of the general law of refraction, was made by the illustrious Newton, who, in the year 1672, communicated to the Royal Society the experimental researches by which he established the compound nature of light, and the unequal refrangibility of its component rays. He held that the common white light of the sun is made up of elementary rays differing at the same time in color and in refrangibility. The number of tints which he considered sufficiently distinct to be regarded as independent components is seven. It seems unnecessary, however, to suppose the existence of more than three elementary colors, it being possible, by mingling these in various proportions, to produce all the rest, while the degrees of refrangibility between the extreme limits vary through an infinite number of infinitely small differences.

Newton's method of demonstrating the truth of his doctrine was as simple as it is ingenious. The colors which border the images of objects observed through prisms of glass or other transparent substances, or through cylindrical or globular vessels filled with water, had long been familiar. Newton placed such a prism in the path of a ray of the sun's light, introduced through a small aperture into a dark room, and received the refracted image or spectrum upon a white screen placed at some distance. Before the interposition of the prism the beam produced upon the screen a white and circular image of the sun itself. But after the rays had been bent by refraction the image appeared very much elongated in the direction of the refraction, and brilliantly colored in a series of tints, passing by insensible gradations from red, through orange, yellow, green, blue, and indigo to violet. This last color was at the end most refracted. In turning the prism around an axis parallel to its edges, Newton observed that the deviation of the spectrum from the original direction of the sun's rays was variable, increasing from a certain minimum (experimentally found) by turning the prism either to the right or to the left. This minimum corresponds to that particular position of the prism at which the angles of incidence and emergence are equal. Upon this observation he founded a test experiment in regard to the refrangibility of the rays of different colors. Making a small circular aperture in the screen upon which the spectrum was formed, at a point where, by turning the prism, he could pass the entire spectrum over it, he placed behind the aperture a second prism, which thus received, successively, rays of a single color only. At a distance behind the second prism a second screen intercepted the light which passed through it, when it was observed that this second image, instead of being elongated like the first, remained sensibly circular, while the positions of the circles of different colors upon the screen were further and further removed from the original direction of the unrefracted rays as the tints ascended from red to violet. This phenomenon of the separation of the component colors of light by refraction has been called *dispersion*. Newton was of opinion that the dispersive powers of all bodies are equal; or, in other words, proportional to their refractive powers; and that, the mean refractive powers of two bodies being equal, their refractive powers for each particular color must be equal also. Both these suppositions, as we shall see, are unfounded.

The discovery of Newton furnished an easy explanation of the interesting natural phenomenon of the rainbow. This beautiful meteor had been the subject of many unsatisfactory speculations; and though *de Dominis*, as early as 1611, had furnished a true theory of the manner of formation of the inner bow, he had not been able to account for its colors. He showed that there is a certain incidence at which, if the parallel rays of the sun fall upon the anterior surface of a transparent globe, they will be reflected from within so as to emerge, still parallel to each other, at a point on the other side of the centre. The emergent rays will form a constant angle with the incident rays, and, entering the eye of the observer standing with his back to the sun, will form the same angle with a line supposed to be drawn from the sun through the eye. This line from the sun through the eye being made an axis, and the above supposed reflected ray being revolved around it, there will be traced out in the heavens a circle, from every part of which, if rain-drops are present, there will come an amount of light above that which is reflected from the surrounding cloud.

This explanation satisfactorily determines the *locus* of the bow; but it fails to account for its tints, or the extent of surface over which they are spread. It would require that the arc should be white, and that it should be no broader than the sun; that is to say, that its breadth should be only about half a degree. The actual breadth of the inner bow is, however, two degrees and a quarter; and that of the outer three degrees and three quarters. Newton's discovery furnished the necessary supplement to the theory.

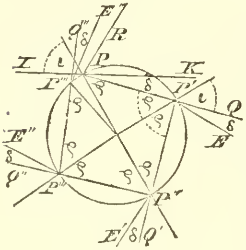

Fig. 3.

In fact, if the circumference PP′P″ be a section through the centre of a transparent globe, and IP a ray of the sun falling on it in this plane, it is easy to see that this ray, or portions of ~~it,~~ will undergo many reflections within the globe, while portions will successively emerge at the points in which reflection takes place. There will first be some loss by external reflection in the direction PR. The portion which enters the globe will be bent, by refraction, from the original direction PK to the direction PP′. At P′ a portion will emerge in the direction PE, being bent from the direction PQ as much as PP′ was bent from PK. The same thing occurs at P′, P‴, and so on. Put ι = the angle of incidence (the angle made by the incident ray with the radius)—the angles of emergence are all of this same value. Put ρ for the angle of refraction. The figure shows that *all* the angles of internal reflection have this value. Let δ represent the bending or deviation of the ray by refraction at each incidence or emergence. Then $\delta = \iota - \rho$. And the amount of deflection of the successive reflected rays from the original direction being represented by D, D′....$D^{(\mathrm{m})}$, and that of the successively emergent rays by Δ, Δ'....$D^{(\mathrm{m})}$, we shall have (an entire circumference being denoted by 2π)

Deflection of $PP' = \delta$; deflection of $PE = \Delta = 2\delta$.

Deflection of $PP'' = D = \delta + \pi - 2\rho$; deflection of $P''E' = \Delta' = 2\delta + \pi - 2\rho$.

Deflection of $P''P''' = D' = \delta + 2\pi - 4\rho$; deflection of $P'''E'' = \Delta'' = 2\delta + 2\pi - 4\rho$.

Deflection preceding *m*th emergence $= D^{(\mathrm{m})} = \delta + m(\pi - 2\rho)$; deflection *m*th emergence $= \Delta^{(\mathrm{m})} = 2\delta + m(\pi - 2\rho)$.

If, for δ, we put its value $= \iota - \rho$, we shall have—

$$\Delta' = 2\iota + \pi - 4\rho.$$
$$\Delta'' = 2\iota + 2\pi - 6\rho.$$
$$\Delta^{(\mathrm{m})} = 2\iota + m\pi - 2(m+1)\rho.$$

The law of the formation of these expressions is obvious. The deflection of each of the successively emergent rays is increased at each reflection within the globe by the angular amount $\pi - 2\rho$.

Now, as all these values contain the angle ι, it is obvious that the deflections cannot be equal when the incidences are unequal; or, in other words, that the emergent rays will usually diverge from each other. Moreover, the deflections do not regularly increase and diminish with the incidence.

Putting the incidence $= 0°$, $\Delta' = 180°$, and $\Delta'' = 360°$.

$$\left.\begin{array}{l}\text{Putting the incidence} = 90°,\ \Delta' = 166°,\ \text{and}\ \Delta'' = 248°,\\ \text{Putting the incidence} = 70°,\ \Delta' = 139°,\ \text{and}\ \Delta'' = 229°,\end{array}\right\}\ \text{nearly, for water.}$$

Neither of the last values is intermediate between the two preceding in the same column. In both cases, therefore, there appears to be some point between the extreme incidences, where the deflection is a minimum; and it being the law of maxima and minima that variations in their vicinity are insensible, it follows that near the incidences corresponding to those values the emergent rays will be sensibly parallel. But when the general expression—

$$\Delta(^{m})=2\iota+m\pi-2(m+1)\rho$$

becomes a minimum*, the cosine of the incidence must have the value—

$$\cos\iota=\pm\sqrt{\frac{n^2-1}{(m+1)^2-1}},$$

in which n denotes the index of refraction.

This determines, therefore, the incidences at which the deflections are minima; and hence, those at which the emergent rays are (to use the term employed by Newton) *efficacious*. It will be seen that, when the index of refraction is given, the value of $\cos\iota$ will be affected only by the variable m, which is the number of internal reflections. If this be made zero, $\cos\iota$ will be infinite; in other words, when the rays are not reflected at all, they do not emerge efficacious.

By putting $m=1$ and $m=2$ we shall obtain values corresponding to the deflections which produce what are called the inner and outer bows. From these values we may deduce the apparent diameters of the arcs; and the theoretic results thus obtained are found to accord with actual measurements. By putting $m=3$, 4, 5, &c., successively, we may obtain the *loci* of an infinite number of additional bows; but after the second reflection, the light ceases to be intense enough to produce an impression on the eye.

Since, with a very slight alteration of ι the rays cease to be efficacious, it is evident that, if the sun were but a point, and the index n invariable, the bow would be reduced to a simple line of light. But as every point of the sun will produce its separate bow, the visible breadth, with n constant, would be that of the sun itself—that is, about half a degree. Newton's experiments on dispersion, however, showed that the value of the index n sufficiently varies, in passing from the red to the violet, to alter sensibly the angle of incidence corresponding to the efficacious rays of the several colors, and sufficient, accordingly, to alter the amount of deflection which those several rays undergo before reaching the eye. As the bows appear in the direction of these deflected rays, it follows that the different colors will not be *superposed*, and that the breadth of the compound bow will be greater than the breadth of the sun by the total amount of their want of conformity. The index for the red may be taken at 1.346; that for the violet at 1.333. Employing these values, we have for the bow by one reflection:

Violet rays. . $\iota_v=58°\ 40'$. $\Delta'_v=139°\ 43'$. Radius of bow $=40°\ 17'$.
Red rays. . . . $\iota_r=59°\ 23\frac{1}{2}'$. $\Delta'_r=137°\ 58\frac{1}{3}'$. Radius of bow $=42°\ 1\frac{2}{3}'$.

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* The general expression for the deflection being—

$$\Delta(^{m})=2\iota+m\pi-2(m+1)\rho,$$

its differential is $d\Delta(^{m})=2d\iota-2(m+1)d\rho$; which, when $\Delta(^{m})$ is a minimum, is equal to zero.

From this we obtain the ratio, $\frac{d\iota}{d\rho}=m+1$.

From the Snellian law, $\sin\iota=n\sin\rho$, n being the index of refraction. This furnishes ananother value of the same ratio, since $\cos\iota d\iota=n\cos\rho d\rho$.

Or, $\frac{d\iota}{d\rho}=\frac{n\cos\rho}{\cos\iota}=m+1$; and $(m+1)\cos\iota=n\cos\rho$.

Squaring this, and adding to it $1-\cos^2\iota=n^2\sin^2\rho$, member for member, we obtain—

$$[(m+1)^2-1]\cos^2\iota+1=n^2(\cos^2\rho+\sin^2\rho)=n^2.$$

From which we deduce the result in the text—

$$\cos\iota=\pm\sqrt{\frac{n^2-1}{(m+1)^2-1}}.$$

And for the bow by two reflections:

Violet rays . . $\iota_v = 71° 49' 55''$. $\Delta''_v = 230° 58' 50$. Radius of bow $50° 58' 50''$.
Red rays . . . $\iota_r = 71° 26' 10''$. $\Delta''_r = 234° 9' 20$. Radius of bow $54° 9' 20''$.

From the values of Δ it will be manifest that the rays which produce the bow by one reflection must enter the rain drops above the ray which passes through the centre; and that those which produce the bow by two reflections must enter below the same central ray.

The differences between the values of Δ_v and Δ_r above, show the amount by which the breadths of the bows are increased in consequence of the variability of n. These amounts are, for the first bow, 1° 44′ 40′, and for the second, 3° 10′ 30′. The colors are produced by the want of *conformity* of the bows corresponding to the several elementary rays; and their feebleness is owing to the fact that, notwithstanding this want of conformity, they do, on account of the considerable diameter of the sun, very sensibly overlap, while they are also diluted by the white light reflected from the anterior surface of the drops Were they entirely superposed upon each other the bow would be white.

While the discoveries of Newton and Snellius, just mentioned, were removing old impediments to progress in optical science, observation continued to add new ones more perplexing than those which had disappeared. In the year 1665 there was published, at Bologna, a posthumous work by Francis Maria Grimaldi, an Italian Jesuit, in which were, for the first time, described certain phenomena now very familiar under the name of *diffraction*. He stated that if any very small object be placed in a pencil of divergent light, admitted through a minute aperture into a dark room, its shadow will appear materially larger than it ought if light passes its edges in straight lines; and, moreover, that any opaque object, large or small, exhibits along the edges of its shadow a border of at least three distinctly tinted fringes, the brightest and broadest of which is next the shadow. He also observed that when two minute pencils of light are admitted through apertures very near to each other, the screen on which the blended pencils fall, and which, as he supposed, ought to be uniformly illuminated with a light equal to the sum of the two intensities, is streaked with lines absolutely dark. He was led by this observation to announce the paradoxical proposition that there are circumstances in which the union of two rays of light produces darkness. Bold as this announcement must have originally appeared, the progress of scientific discovery has fully confirmed its truth. This phenomenon, being attributed to the bending of the rays of light in the immediate vicinity of the opaque body, was distinguished by the name *inflection* or *diffraction*. It was carefully studied by Newton and others, and has occupied a prominent place in all the discussions which have since arisen in regard to the nature of light.

Not far from the time of the discovery of Grimaldi, just mentioned, the attention of the scientific world was called to a case of new and extraordinary refraction observed to take place in crystals of carbonate of lime—a species of refraction, which, from the circumstance of its dividing an incident beam into two beams entirely distinct, or of presenting two images of any object seen through the crystal, has been called *double refraction*. The first publication on this subject was made by Erasmus Bartholinus, a physician of Copenhagen, who gave to the mineral the name of Iceland spar, from the circumstance that his specimens had been obtained from that island. It is now known that this property of double refraction is exceedingly common, being possessed by most crystallized bodies, and capable of being produced, transiently or permanently, in any transparent solid whatever, whether organic or mineral, in which it does not naturally exist It is only in Iceland spar, however, that it manifests itself in a degree remarkable enough to attract the attention of a casual observer, and in most cases it can only be detected by special arrangements.

Iceland spar is favorable to observations upon double refraction, not only on account of its wide separation of the refracted rays, but also because of the size of the crystals which can be obtained of this mineral, and of their beautiful transparency. Its primitive crystalline form is the rhombohedron. Whatever may be the configuration of the mass as obtained from its native bed, it will be found to cleave with great facility in directions parallel to the faces of the original rhombohedron, and it is thus easily reduced to a form favorable for experiment. The angles of the rhomboidal faces are 101° 55′ and 78° 5′. The inclinations of the faces upon each other are 105° 5′ and 74° 55′. Two of the solid angles are contained by three of the obtuse angles of the rhomboids, and the other six by two acute and one obtuse each. The diagonal connecting the two exceptional solid angles is the shortest of the diagonals of the rhombohedron, and is called the crystallographic axis. These angles themselves are called the vertical, and the other six the lateral, angles of the crystal.

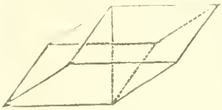

Fig. 4.

If a mark be made with ink upon a sheet of white paper—a small cross for example—and a rhomboid of Iceland spar, two or three inches in thickness, be laid over it, then in whatever position the eye may be placed above the upper surface of the crystal, two crosses will be seen. If the crystal be turned about upon its horizontal face, one of these images will remain motionless, and the other will describe a circle around it. The motionless image will, moreover, appear sensibly nearer to the eye than the other. If, instead of a small mark, we take a straight line ruled entirely across the paper as an object, then, if the eye be placed vertically over the line, and the crystal interposed, it will be seen that the nearer image is always a continuation of the part of the line seen beyond the crystal on each side, while the more distant one is more or less displaced laterally. In revolving the crystal, moreover, this second image will pass from one side to the other of the first, and a position will be found (or rather two positions, differing from each other by 180°) in which the two images apparently coincide, though, as they are differently distant, they are merely superposed.

Until the discovery of this remarkable property in Iceland spar, refraction was supposed to be governed in all cases by the law of Snellius. But it is impossible that this should be true of both the rays in the present case. It is, in fact, true only of that one which produces the nearer and fixed image. This is, for distinction, called the *ordinary* ray; the other, the *extraordinary*.

If the vertical angles of the rhombohedron be truncated perpendicularly to the crystallographic axis, and the artificial faces thus formed polished, it will be found that when the crystal is laid over a small object upon one of these faces, and the eye placed immediately over it, only one image will be visible. This is not an illusion occasioned by the superposition of images differently distant; there is actually but one image. But if the emergent ray coming to the eye, by which the object is seen, be at all inclined to the surface, the image will be duplicated, and the degree of separation of the two images will increase with the inclination. If the lateral edges of the crystal are cut away, so as to form a parallelopipedon, whose faces are parallel to the crystallographic axis, and the crystal be laid on its side, the separation of the images will be at its maximum. In this case, if the emergent visual ray be perpendicular to the surface, the two images will be superposed, but the duplicity will be very perceptible.

It appears, then, that there is one direction in the crystal, in which light may pass without double refraction, and that this direction corresponds with that of the crystallographic axis. This direction is also called the *optic axis;* but the term optic axis, it must be observed, is not intended to denote a particular *line*, but only a particular *direction*, and in the present case it is a line anywhere in the crystal parallel to the axis of symmetry.

Any *plane* parallel to the axis of the crystal necessarily coincides with the optic axis, and every such plane is called a *principal* plane or *principal section.* This term is one of very convenient use. Any plane at right angles to the optic axis (and therefore to all the principal planes) may be called a *conjugate* plane or section, which term will be also found to have its convenience. In every such conjugate plane the separation of the two rays by double refraction is at its maximum; and, what is also important, the extraordinary ray, as well as the ordinary, obeys in this plane the law of Snellius. The indexes of refraction for the two are, however, necessarily different; that of the ordinary ray being 1.6543, and that of the extraordinary, 1.4833. In directions which do not correspond with either a principal or a conjugate plane, the index of refraction of the ordinary ray will be found to be invariably the same, but that of the extraordinary ray will gradually increase from the direction perpendicular to the axis to that which coincides with the axis. In this last case the two indexes become equal, and double refraction disappears. The index of the extraordinary ray at any inclination (denoted by α) with the optic axis, may be found from the following formula, in which $n = 1.6543$, $n' = 1.4833$, and N is the index sought:

$$N = \sqrt{n^2 - (n^2 - n'^2)\sin^2\alpha} = \sqrt{2.7367 - 0.5365\sin^2\alpha}.$$

We see now why it is that one of the images seen through the crystal is apparently nearer than the other. The general effect of refraction by a single plane surface of a body denser than air, is, as has been already illustrated, to bring the object apparently nearer to the surface. This effect must depend for its degree upon the refracting power, and this power is a direct function of the index of refraction. The indexes of the two rays are different; and therefore the apparent distances of their images are different likewise.

One of the most remarkable facts connected with the refraction of the extraordinary ray is that, unless the incidence is in the plane of a principal section, or of a conjugate section, the refracted ray is not in the plane of incidence. And if the refracting surface, whether the natural surface of the crystal or one artificially prepared, be not coincident with a principal or a conjugate plane, the extraordinary ray is bent at the surface, even when the incidence is perpendicular.

In observing through the crystal prepared by truncating its vertices by conjugate planes, in which case we have the advantage of having both refracted rays in all positions in the plane of incidence, we shall see that the extraordinary ray is always the most distant from the normal to the surface. But this normal represents the direction of the optic axis. The extraordinary ray, therefore, has the semblance of being repelled from this axis. As there are crystals in which the apparent effects are reversed, that is, in which the extraordinary ray is nearer to the optic axis than the ordinary, as if it were attracted, these two classes have been distinguished by the terms *negative* and *positive.* In the negative the extraordinary index is less than the ordinary; in the positive, greater.

A curious observation in regard to the paths of the two rays through a crystal of Iceland spar, by which an object at a little distance beyond it is seen, originally made by Monge, may be mentioned here. The object being at O, and the eye being at E, the ordinary image will appear above the extraordinary and nearer, as at O′, O″ being the extraordinary image. The emergent rays are therefore P′E and Q E. But the rays incident on the under side of the crystal from the object must be respectively parallel to these. Draw then OP parallel to P′E, and OQ parallel to Q′E, and join PP′, QQ′. The entire path of the ordinary ray is then OPP′E, and that of the extraordinary is OQQ′E, which lines, when the plane of incidence is a principal plane, necessarily cross each other in the crystal.

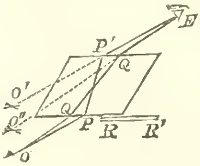

Fig. 5.

If a card be passed along the under surface of the crystal, in the direction R'R, it will cut off the ray OP before interfering with OQ. The image O', which is most distant from the card, is therefore first to disappear—a phenomenon very striking when seen for the first time. The card employed in this experiment should be dull black in order to produce the best effect, otherwise it is too conspicuous itself.

When a ray of light, after having passed through one crystal and having been divided into two distinct emergent rays, is allowed to fall upon another similar and equal crystal similarly situated, the effect, as might naturally be expected, will be to increase the separation of the rays to the same extent as would have occurred had both the crystals been united in one. But if the second crystal be turned around the direction of the ray as an axis, other phenomena make their appearance, the character of which depends on the amount of turning. In speaking of this kind of revolution it will be convenient to employ the term *azimuth.* By this word is meant *direction in space in a plane at right angles to any axial line.* The term is adopted from astronomy and geodesy, in which sciences the assumed axial line is the *vertical,* and the azimuthal plane the *horizon.*

In the case in hand, if we completely reverse the position of the second crystal in azimuth, that is to say, turn it round 180°, it will reverse the refracting effect of the first crystal and reunite the two rays, which will emerge as one. If we turn it only 90° in azimuth the separation of the rays will continue, but that which was the extraordinary ray in the first crystal will become the ordinary in the second, and *vice versa.* Accordingly, if the original incidence is perpendicular, the ray which follows the normal in the first crystal will be bent at the surface of the second, and that which is bent at the surface of the first will follow the normal on entering the second.

At any azimuth differing from the original position more or less than 90° or 180° there will be seen four emergent rays, of which two will usually possess a greater intensity than the other two. When the change of position of the second crystal is but slight, the two original rays will be vivid; but, in a line at right angles to that which connects them, two very faint ones will appear, nearer together than the original two. As the rotation advances these new rays will gain in strength, while the other two grow less intense. At the azimuth of 45° the four will be equal and equidistant. Beyond 45° the original rays go on fading and the new ones increasing in brightness, until, at 90°, the former become entirely extinct and the new ones remain alone. Beyond 90° again another faint pair appear, which go on, as before, increasing in brightness, at the expense of the companion pair, up to the azimuth 135°, when the four are again equal. Beyond 135° this second new pair still continue to gain strength and to approach each other, till, at the azimuth 180°, they reunite into one, and the others in their turn vanish. In the figure following, these successive phases are shown as they appear upon a screen when the experiment is performed in a dark room. They are circumscribed by the outlines of the two rhombs in their relative successive positions.

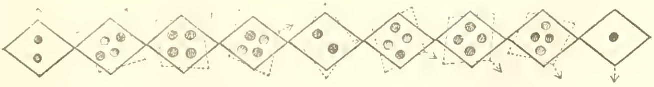

Fig. 6.

The phenomena of double refraction were carefully studied by the celebrated Huyghens, who devised a physical theory for their explanation, which has been pronounced by Brewster to be one of the most splendid of the triumphs of genius which illustrate the history of science. His theory did not, however, extend to the explanation of the remarkable appearances last described, which present themselves when two doubly refracting rhombs are combined—appearances which were observed by him with surprise and perplexity. They are now known to be owing to a remarkable modification of light which always accompanies double refraction, though it may be produced in other ways, and which is called *polarization*. This will occupy much of our attention further on.

Soon after his announcement of the compound nature of light, Sir Isaac Newton made public the results of his ingenious investigations in regard to the colors exhibited by *thin plates* of transparent substances, such as soap-bubbles, films of moisture upon glass and upon polished opaque solids, laminæ of air confined in fissures of transparent minerals, &c. He showed that the tints displayed by such thin plates, when viewed in common light, depend upon three conditions, viz: the thickness of the plate, its refracting power, and the angle of obliquity under which it is viewed. The determination of the relation of the tint to the thickness, was made by means of a very simple contrivance. A double-convex lens, of very long focus, was placed in contact with the plane surface of a plano-convex lens, the two being pressed together by means of screws. In Newton's experiments the double-convex lens was beneath and the plano-convex above. The convexity of the upper surface of the upper lens is advantageous when oblique observations are desired, as tending to reduce the refraction of the incident and emergent rays at that surface. The two touching surfaces have, theoretically, but a single point of contact, and that point is the centre of a thin plate of included air, of which the thickness increases from zero equally in all directions. The law of this increase will be apparent from the figure annexed. MN represents the lower surface of the superior glass, and QR the upper surface of the inferior. Let C be the centre of the sphere of which QR is a superficial section. Put r for the radius CP. Then, if the arcs Pa, Pb, are small in proportion to the whole circumference, we shall have

$$Pa'=Aa=\frac{\overline{aa'}^2}{2r}\text{; and } Pb'=Bb=\frac{\overline{bb'}^2}{2r}.$$

Or, if x stand generally for the thickness Aa or Bb, and y for the corresponding distance from the point of contact, PA or PB, we shall have the variation, $x \propto y^2$.

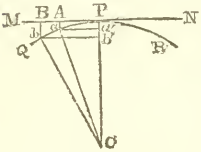

Fig. 7.

This furnishes a law by which, when the thickness corresponding to a single assigned value of y is known, the thickness for all other values may be computed with great facility.

The apparatus being arranged as above described, the colors which are seen by reflected light are arranged in regular rings around a black centre and in successive series, as follows:

1. Black, blue, white, yellow, red.
2. Violet, blue, green, yellow, red.
3. Purple, blue, green, yellow, red.
4. Green, red.
5. Greenish blue, red.
6. Greenish blue, pale red.
7. Greenish blue, reddish white.

These are what Newton calls the successive *orders* of colors, and, in referring to any particular tint, it is designated as the blue, red, green, &c., of the first, second, or third order, as the case may be. Beyond the fourth order the colors become feeble or begin to fade rapidly out into whiteness, and, beyond the seventh, color can scarcely be at all perceived. The cause of this fading may be made manifest by employing *homogeneous* or *monochromatic light;* that is to say, light of a single tint only, obtained by isolating a portion of the rays of the prismatic spectrum whose refrangibility and color are sensibly the same. Then very many more bright rings will be observed, separated by intermediate rings entirely dark. But what is of most importance at present is that those which are formed by the least refrangible rays are larger than any others, and that the diameters of rings of the same order regularly diminish as the refrangibility increases. This difference of magnitude between the rings of different tints occasions the overlapping of one color upon another when white light is used, so that the colors observed are not simple but resultant colors, determined in their tints by the simple colors which happen to be predominant at any point. The other components serve, with some portion of the predominant tint, to produce white light, by which the tint is diluted and rendered more feeble than it would otherwise be. The truth of this explanation will be made apparent by viewing the rings through a prism. The effect will be to make the overlapping on one side more complete than before, and, on the other side, less. The rings will be less highly colored but more numerous and better separated on the side of greatest refraction, and more confused on the other.

From a careful measurement of the diameters of all the bright rings, Sir Isaac Newton ascertained that the squares of these diameters form a regular arithmetical progression, corresponding to the natural series of odd numbers, 1, 3, 5, 7, &c. And the squares of the diameters of the intermediate dark rings were found to constitute another similar progression, corresponding to the series of even numbers, 2, 4, 6, &c. From the law $x \propto y^2$, it therefore follows that the bright rings appear where the thickness of the plate is *once, thrice, five times*, &c., some constant value, and that the dark rings appear where the thickness is *twice, four times, six times*, &c., the same constant value. The next question to be determined is, therefore, what is that constant?

In order to ascertain this, Sir Isaac Newton measured with great precision the absolute diameter of the fifth dark ring. This, with the known radius of the spherical surface of the lens, enabled him to compute the thickness of the plate at that ring, this thickness being the versed sine in a great circle of the sphere of an arc of which the measured diameter is the chord. The result gave him $\frac{5}{89000}$ of an inch, very nearly, for the thickness of the plate at the fifth dark ring. But the fifth number in the series 2, 4, 6, &c., is 10. Hence, the constant sought for is one-tenth of $\frac{5}{89000}$ of an inch, or $\frac{1}{178000}$, and this is the thickness of the plate at the point where the greatest brightness of the first bright ring is seen. Reduced to a decimal, it gives a little more than fifty-six ten-millionths of an inch. If the value of this constant be sought for the several homogeneous rays, it will be found to be, for the violet, a little more than thirty-nine ten-millionths, and, for the red, not quite sixty-nine ten-millionths. As, in the space occupied by the colors of the first order, the thicknesses vary slowly, and as there is a certain range of variation in thickness within which each color may appear, though its greatest intensity is in the middle of this range, it happens that the colors of the first order are dilute, especially toward the centre of the system, and that the middle of the series is white. In the succeeding orders, the differences tell in such a manner that the bright rings of some colors fall more or less exactly upon the dark rings of others, and the tints become stronger. But, as the thicknesses soon begin to vary rapidly, every system of rings becomes crowded, and the separating dark intervals grow narrower and narrower, until there is a complete blending of tints at every point and the resultant is sensibly white. When water is introduced between the glasses, the rings become immediately smaller. If the thickness at which a given tint now appears is compared with that at which the same tint appeared in air, it is found to be reduced in the ratio of n to 1, n being the index of refraction between air and water. This law admits of being generalized. In fact, whatever be the substance of the thin plates in which these tints appear, the thicknesses which produce them are inversely proportional to their indexes of refraction.

When the system of lenses described above is held between the eye and the light, another system of rings makes its appearance, which is formed by the transmitted light. In this case the tints are much feebler, being diluted by the intermixture of a great deal of white light, which, as we shall see hereafter, has nothing to do with their formation. Of these it is remarkable that the diameters of the bright rings correspond with those of the dark rings seen by reflection. Thus the thicknesses at which the bright rings by transmitted light appear form a series corresponding with the progression of even numbers, 0, 2, 4, 6, &c.; and the thicknesses at which the intervening dark rings are seen correspond to the progression 1, 3, 5, 7, &c. Also the tints reflected and transmitted at any given point are complementary to each other, or are such as, united, produce white.

The measurements above given are those which correspond to rings formed by light perpendicularly incident upon the thin lamina. But when the rings are observed obliquely, their diameters are rapidly enlarged with increase of obliquity. Sir Isaac Newton ascertained the law of this increase to be this: that the squares of the diameters are inversely as the cosines of incidence. When the incidence exceeded 60°, it appeared to him that this law no longer held good; and this conclusion, which, up to a recent period, had not been invalidated, has formed a serious difficulty in the way of any theory of light. Recent experiments, however, made by Messrs. Provostaye and Desains, with monochromatic light, and with special arrangements to eliminate the sources of error in measurement which must have vitiated Newton's results at high incidences, have fully established the universality of the law. Their measurements extended to the forty-third ring, and to the great incidence of 86° 14′, beyond which the rings were no longer discernible.

Colors resembling those of thin plates may be produced also, in various modes, by means of thick plates. Sir Isaac Newton employed, in an interesting experiment of this kind, a spherical glass mirror, with truly concentric surfaces, silvered on the back. A very small beam of light (about one twenty fifth of an inch in diameter) having been introduced into a dark room, he received it on this mirror in such a manner as to reflect it back toward the aperture. At the centre of curvature of the mirror he placed a white card pierced, in order to allow the light to pass, with a very small orifice. Around this orifice he saw a series of rings resembling those of thin plates. When the light was homogeneous, the rings were alternately bright and dark as in the other case. The diameters were also observed to follow similar laws. As both surfaces of the mirror are concerned in producing these rings, and as, at the first surface, it is the irregular or scattered reflection only which is necessary to the effect, the experiment succeeds best with a mirror in which this surface is not highly polished. Instead of the perforated card, a lamina of mica, or of slightly tarnished glass, may be employed to receive the rings.

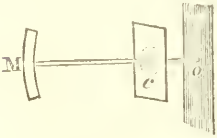

Fig. 8.

When light is transmitted through or reflected by a pair of thick plates of homogeneous glass, with parallel plane surfaces, and placed parallel to each other, colors may appear, if the *difference* of thickness of the two plates is comparable to the absolute thickness at which such colors are produced by thin plates. The figure shows the arrangement. Dr. Brewster produced the same effects with a pair of plates of *equal* thickness, by inclining one of them so that the path of the rays within it should be slightly longer than within the other. There is some similarity between the first of these classes of phenomena and those of diffraction. The second have a nearer analogy to the colors of Newton's rings.

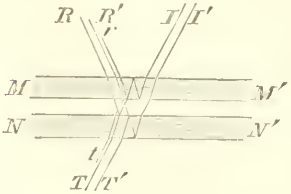

Fig 9.

The next important step in the progress of optical science was the discovery of the progressive motion of light, and the determination of its velocity. Though every theory which had ever been suggested to account for the phenomena of light presumed that there must be a progress from the luminous origin, and therefore that time must be an element in the solution of every optical problem, still so nearly instantaneous are all the effects produced at the distances to which our ordinary observation extends, as apparently to render hopeless any plan for experimentally determining the velocity. This circumstance rendered the efforts made by the celebrated Galileo, and by the academicians of Florence, to settle the question, completely nugatory. The method of proceeding adopted by Galileo was to place himself upon an eminence opposite to an assistant observer something more than a mile distant; both being provided with lanterns which could be darkened by a slide. The lights being arranged, Galileo darkened his lantern; and the assistant, immediately on noticing its disappearance, darkened his also. Apparently both were extinguished at the same instant. The Florentine academicians repeated the experiment, increasing the distance between the stations, but the result was the same. The problem remained unsolved; but its solution came at last, when demanded by the exigencies of a higher branch of science.

In 1675 Rœmer, an astronomer of Copenhagen, in his observations upon the eclipses of the first satellite of Jupiter, became perplexed by irregularities for which he could conceive no means of accounting. It was suggested by Dominic Cassini that these difficulties might perhaps be removed by supposing that the time occupied by light in passing through the vast distance between Jupiter and our planet may be large enough to be appreciable; and therefore that, as our distance varies, this time must vary also. Assuming this hypothesis to be true, and that the epoch on which our computations of future eclipses are founded is the date of some eclipse actually observed when the two bodies were occupying their points of nearest approach, it will follow that if the accuracy of the determinations is affected only by the motion of light, all subsequent eclipses, observed when the distance is the same as at the epoch, will agree with the prediction, and all others will be in retardation by an amount of time equal to that which light requires to pass over the space by which the distance has been increased. In like manner, if the epoch had been an eclipse observed in the position of greatest distance between the bodies, subsequent eclipses would be in advance of the prediction; and if the epoch had been an observation made from some position intermediate between the points of greatest and least distance, the eclipses afterwards occurring would be sometimes in advance and sometimes in retardation. The test of the correctness of the hypothesis would be a careful comparison of the observed irregularities of time with the variations of distance—a comparison involving no slight labor. Cassini, with whom the idea originated, seems to have abandoned it; but Rœmer followed it up with such perseverance as at length conclusively to establish its truth. He demonstrated that the time occupied by light in passing over the entire diameter of the earth's orbit is 16 minutes and 26 seconds. But at that period the dimensions of the earth's orbit were not accurately known, and this determination was insufficient to fix the absolute value of the velocity of light. Assuming the sun's mean parallax to be 8.″6, the mean diameter of the orbit must be about 190,000,000 of miles, and this number divided by 986, the number of seconds in 16 minutes and 26 seconds, gives for the velocity in miles 192,700.

The velocity of light has, since the time of Rœmer, been ascertained, with a probably near approximation to the truth, by other independent methods, and the results tend to confirm the substantial correctness of his original determination. The first of these methods is that which rests upon the measurement of the aberration of the stars, a phenomenon discovered by Bradley, afterwards

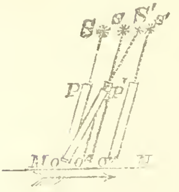

Fig. 10.

astronomer royal of England, in 1728. This aberration consists in an apparent displacement of the star from its true position by the combined influence of the motion of the earth and the progressive motion of light. If, for instance, the line MN be taken to represent a small portion of the earth's path, and S be a fixed star, then while the earth advances in the direction of the arrow from O to O′, O″, &c., if the propagation of light were instantaneous through all distances, the star would be seen in the true direction, OS, O′*s*, O″*s*′, &c., the telescope OP remaining parallel to itself as the earth moves, in consequence of the immense distance of the star. Also, allowing progressive propagation of light, if the earth were without motion, the star would still appear in its true direction, OS, the telescope OP remaining stationary; but if we suppose both the earth and light to move, then a ray entering the centre of the tube OP, at the summit, would not be in the centre of the tube when it reached the lower end, but would be displaced toward the rear by a small space equal to the earth's own motion while the ray is descending the tube. To the observer at O, therefore, the telescope would not appear to be truly pointed at the star, but would require to to be leaned forward in the direction OP′, until the luminous elements which compose the ray (whatever they may be) should follow accurately the axis of the telescope from top to bottom. The star will accordingly seem to be at S′, in advance of its true position, in the direction of the earth's motion. The amount of this apparent displacement will vary with the angle made by the direction of the earth's movement with the direction of the star. When this angle is zero, that is to say when the earth is moving directly toward or from the star, the displacement is zero; when the angle is 90°, or when the earth's motion is directly across the line drawn to the star, it is maximum. For a star in the plane of the earth's orbit, the aberration is apparently an oscillation in a straight line, the duration of the movement in the alternately opposite directions being six months; for a star in the pole of the ecliptic, or in a direction at right angles to its plane, the apparent path would be a very small ellipse similar to the earth's orbit. The major axis of this ellipse would measure the maximum amount of aberration on both sides of the true place, and this is found to be equal to 40″.88; half of this, or 20″.44, is the maximum absolute amount of displacement. The direction of a star, therefore, when its aberration is maximum, deviates from its true direction as the diagonal of a rectangle deviates from the side. If, in such a rectangle, the smaller side be made equal to the velocity of the earth, the larger will be the velocity of light, and the angle between the larger side and the diagonal will be 20″.44. But the earth's velocity per second is known, and is about 18.9 miles; hence the velocity of light is $18.9 \times \cot 20''.44 = 190{,}730$ miles, a number less than that before obtained by about one one-hundredth part.

This coincidence of results is sufficiently remarkable, when we consider the extreme delicacy of such measurements as those by which aberration is determined, and also the difficulty of fixing, by observation, the exact instant of the immersion or emersion of one of Jupiter's satellites; but, these difficulties apart, there is nothing surprising in the agreement, since both depend at last for their absolute values upon our received horizontal parallax of the sun.

Results very nearly similar, however, have been recently obtained by experimental methods founded upon principles entirely different from the foregoing. The first of these methods was devised by Mr. Fizeau, of Paris, and executed by him in the vicinity of that city. Having selected two stations, visible from

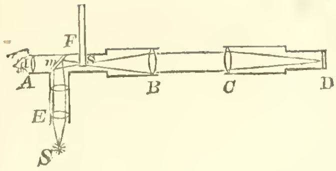

Fig. 11.

each other, and about 5⅓ miles apart, he placed two tubes, something like tubes of telescopes, one at each station, looking towards each other, with their axes in the same straight line. One of these tubes, represented at AB, has a branch tube E furnished with lenses, through which is received the light from a radiant point S. This light is reflected by an inclined transparent plane mirror *m*, forming a bright image of the luminous point at *s*, which is in the principal focus of the large lens B. The rays being made parallel by this lens, are received at the other station upon the lens C, by which they are brought to a focus upon the surface of a plane mirror D. Being reflected back by this mirror, they return to the lens B, and once more form a bright image at *s*, which image may be observed through the transparent mirror *m*, by an eye placed at A. The upper side of the tube at F, just in front of the plane mirror, is cut through in order to admit the limb of a wheel, furnished with teeth, to descend so far into it that the image *s* may be seen between the teeth or cut off by them, according to the position of the wheel. The teeth and the intervening spaces are of exactly equal breadth. By means of connected gear-work a high velocity of rotation may be given to the wheel, while the number of turns per second admits of being ascertained. The velocity may also be retarded by a brake. When the wheel turns slowly the light is intermittent, and the passage of the teeth is perceptible. But when as many as ten teeth pass per second, the light is constant, owing to the duration of the successive impressions upon the eye. By accelerating the movement the brightness of the image may presently be made to fade, in consequence of the interference of the successive teeth with the rays returning from the distant station, after having passed through the last preceding interval between the teeth. As the velocity increases this fading will become an absolute extinction, each tooth in its progress cutting off all the light which passed through the interval before it. When this state of things is reached, it is evident that the time occupied in the passage of light to the distant station and back—that is to say, 10⅔ miles—is equal to the time which it takes for a tooth to advance a distance equal to its own breadth. If there are five hundred teeth and five hundred intervening spaces, this time will be one one-thousandth part of that of a revolution; and if there are eighteen revolutions in a second, the absolute time will be one eighteen-thousandth of a second.

By still further accelerating the velocity of rotation, the light may presently be made to reappear; the rays which pass through one opening to the distant station, returning through the next following opening to the eye. When the full brightness is thus restored, the velocity will be found to have been doubled. By carrying the acceleration still further, the light may be a second time eclipsed and a second time restored; and, in like manner, alternately extinguished and revived, as long as the driving power will allow: the velocities at which the several successive extinctions and revivals occur, constituting a regularly increasing arithmetical series.

We thus are enabled to measure the small fraction of a second required for light to pass over twice the distance between the two stations; and dividing this double distance by this fraction we obtain the velocity of light per second. The result at first obtained by Mr. Fizeau, by means of this apparatus, was about 196,000 miles, being in excess of the results by the astronomical methods by one-sixtieth part nearly.

It is manifest that any mode by which very minute intervals of time can be accurately measured, is capable of being employed as a means of determining the velocity of light. Mr. Wheatstone, in his researches upon the velocity of electricity, employed for this purpose a revolving mirror; and in 1839, Mr.

Arago made an effort, by the use of a similar mirror, to institute a comparison between the velocities of light in passing through water and through air. This was suggested by him as an *experimentum crucis* between the opposing theories current in regard to the nature of light; in one of which light was supposed to consist of material particles actually thrown off by luminous bodies, while in the other it was assumed to be an effect of undulations propagated through an exceedingly subtle elastic medium pervading all space. If the first were the true theory, the velocity of light in a more powerfully refracting medium should be greater than in a less; and the reverse, if the second were true. Mr. Arago did not carry out his design to its completion, but it has since been successfully executed by both Mr. Fizeau, whose original method has just been given, and by Mr. Foucault, well known for his pendulum demonstration of the earth's rotation. The experiment served at once to compare the velocities of light in air and water, and to determine the absolute velocity. The annexed figure may render the method intelligible. Suppose a small beam of parallel rays to be admitted into a room otherwise dark, through an aperture R, and to fall upon an achromatic lens, fixed at C, in the direction of its axis. Let CD be the focal distance of this lens; and at M let the beam be intercepted by a mirror capable of turning around a vertical axis coincident with its plane. At a distance, ME=MD, in any convenient position not very remote from R, let there be placed a spherical mirror having its centre of curvature in the axis of M.

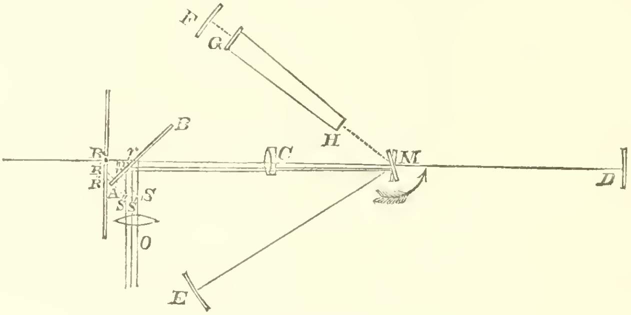

Fig. 12.

Let there be, further, across the aperture R a fine wire exactly vertical; and in front of the aperture R a transparent plane mirror AB, inclined to the beam at an angle of 45°. The mirror M may obviously be turned on its pivot, so that the ray RCM falling upon it may be reflected to E. If it remain stationary in this position, the light incident on E will be returned to M, and so again to the aperture R. But a portion of this returning light, being reflected by AB towards O, will enable an observer at that point to see the image of the vertical wire. To assist the eye, a magnifying eye-piece may be employed, and this may be provided with a spider-line micrometer at its focus. If the mirror be now put very slowly into revolution, the image of the wire will be seen intermittently and momentarily, once in every revolution. The spider-line of the eye-piece is now to be brought to exact coincidence with this image. Accelerating the revolution, when the number of turns per second becomes as great as ten, the image will be permanent. If, now, a very high velocity be given to the mirror, the image seen by the observer at O will no longer coincide with the spider-line of the micrometer, but will be seen at a sensible distance from it in the direction of rotation. Thus, if the arrow represent the direction of rotation, the returning ray which originally met the mirror A B at *r*, will meet it at *r'* or *r''*, and the image which originally appeared at S will be seen at S' or S''. This is evidently owing to the change in the position of the mirror M, while the light is moving from it to the spherical mirror E and back again; and the angular displacement of the returning ray around the centre M will, according to the well-known law of reflection, be double the angular change of position of the mirror. If the mirror makes n turns in a second, the time of one turn will be the nth part of a second, and the time of making the change of position of which the observation gives us the evidence, will be the same fraction of the nth part of a second that half the angle subtended by RR′ or RR″, as the case may be, at the centre M, is of 360°; or as $\frac{1}{2}$RR′ is of a whole circumference. This distance RR′ or RR″, being equal to SS′ or SS″, is directly measured by the micrometer. Let it be put $= \delta$. The circumference of the circle whose radius is RM, (which put $= r$,) is $2\pi r$. Put the space ME$=s$, or 2ME $=$ $2s$, and the time of passing 2ME$=t$. Also let v represent the velocity of light. Then—

$$t=\frac{\delta}{4\pi rn};\ \text{and}\ v=\frac{2s}{t}=\frac{8\pi rns}{\delta}.$$

This expression is, however, true only on the assumption that the returning ray suffers no deviation in passing the lens C. But since, if its original path was, as we have assumed, the axis of the lens, it cannot, if sensibly deviated, return through the axis, it will be bent at C, and the displacement RR′ will be less than we have assumed it to be. If D be the actually observed displacement, and if RC be represented by r' and MC by s', then the value of our assumed displacement in the above formula will become, as may easily be shown—

$$\delta=\frac{Dr(s+s')}{sr'}.$$

Substituting this value, we shall have for the velocity of light—

$$v=\frac{8\pi nr's^2}{D(s+s')}.$$

With a distance $s = 4$ metres, and 800 turns of the mirror per second, Mr. Foucault found a value of $D = 6^{mm}$, whence the value of v is found to be, in English miles, 192,950.*

By placing a second fixed mirror, F, in any other convenient position, and interposing a tube, as GH, filled with water or any other transparent medium, the ends of the tube being closed with plate glass having parallel surfaces, the velocities of light in air and such a medium may be compared. The mirrors E and F will both give images of the wire at R; and if the value of v is the same for both, the two images will be coincident, and appear as one; but if v have different values for the different media, one of the images will be more displaced than the other. Mr. Foucault performed this experiment; and, in order to identify the images, and to distinguish one from the other, he placed before the mirror E a screen having a rectangular opening, such that one-third part of the image from that mirror should be cut off from the top, and another third from the bottom, the central third only being left unobstructed. In the image of the aperture at R, as seen at O, the middle third had, very sensibly, greater brightness than the top or the bottom, and the wire, as reflected from E, was apparently but one-third as long as it appeared reflected from F. The image from F was sensibly the most displaced, indicating lower velocity in water than in air; the displacement, D, in the formula above, being a factor of the denominator.

The next discovery of importance in the progress of optical science was made near the close of the last century, by Dr. Wollaston, in his observations upon the prismatic spectrum. He discovered that, by employing a pencil of light

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* By Mr. Foucault's more recent experiments with this method, the velocity of light is reduced to 190,249.16 miles.

very narrow in the direction of the plane of refraction, but broad parallel to the axis of the prism, five well-defined dark straight lines could be distinguished crossing the spectrum at right angles, and maintaining invariably the same positions relatively to the colors. This number he afterwards increased to seven. These lines may very easily be distinguished by holding a prism near the eye, parallel to any small fissure through which light makes its way into a dark room. The reason they escaped the notice of Newton and other earlier observers is to be found in the fact that those observers employed a pencil so broad in the direction of refraction as to make the actually observed spectrum a compound of many superposed and unconformable spectra, thereby obliterating these very narrow markings. In fact, every point in an aperture of sensible magnitude, through which the light experimented on is introduced into the dark room, produces a spectrum of its own. Moreover, supposing that it is the sunlight which is introduced through the aperture; it may be said that every point of the aperture produces not only *one* spectrum, but as many spectra as there are points in the sun's disk from which lines may be drawn to the assumed point in the aperture. As all these lines, so drawn, would, in the absence of the prism, produce a white circular image of the sun upon the screen in the dark room, having a diameter increasing with the distance of the screen from the aperture, it follows that, when the prism is introduced, the spectrum produced by each point of the aperture will have a breadth equal to the diameter of this white image of the sun, and that its elongated form is due to the lateral unequal displacement of an indefinite number of circles, produced by the several elementary rays of which white light is made up. The interposition of a convex lens between the prism and the aperture may serve to reduce the breadth and sharpen the boundary of the image; but still it is manifest that with a circular aperture, there must, unless the diameter is made too small for convenient observation, be a considerable mixture of rays of different refrangibility in every part of the length. It is therefore best, for the purpose of obtaining a spectrum at once broad and pure, to employ an aperture very narrow in the plane of refraction, and broad in the direction of the axis of the prism. This may be still further improved by the use of a convex lens of long focus, as above described; or better, by the use of a *cylindrical* lens, with its cylindrical axis parallel to the length of the aperture. With an arrangement like this, the lines of Dr. Wollaston may be easily exhibited, and many more. By aiding the eye with a telescope, the number discovered becomes surprisingly great. Mr. Fraunhofer, of Munich, enumerated five hundred and ninety, and Sir David Brewster afterward increased this number to two thousand. Their general appearance under the telescope is shown in the figure annexed.

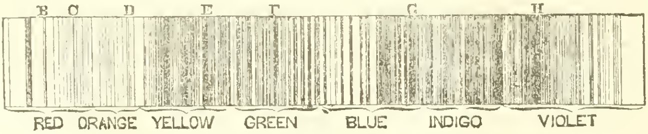

Fig. 13.

The eight principal lines are distinguished by the letters A to H, of which the line A is at the beginning of the red, and the line H about the middle of the violet. The line A does not appear in the figure. The positions of these lines being definitely fixed among the colors of the spectrum, they furnish valuable aid in comparing the refracting powers of different bodies, and have served to reveal the fact that bodies whose *mean* refractive powers are equal, do not always equally refract the several elementary rays. The line A is not among the most easily discernible, but Sir David Brewster has discovered others in the almost imperceptible light below A; and Sir John Herschel, and especially Professor

Stokes, have discovered many others still beyond the violet. By his curious discovery of *fluorescence*, or the property possessed by some substances of rendering sensible to vision rays beyond the limit of the ordinary spectrum, Professor Stokes has in fact quadrupled its length.*

In observing the spectra formed by light from other sources than the sun, as from the fixed stars, from incandescent solids, flames, &c., great differences are found to exist in regard to the lines observed. In the spectra of the fixed stars, dark lines are seen, which, like those of the solar spectrum, are unchangeable in position, but which do not occupy the same positions. In the spectra of flames, lines are observed which are not dark, but bright. Different salts added to the wick of an alcohol lamp produce different systems of lines, always bright. So, likewise, metals burned under the compound blow-pipe. The spectrum of the electric spark exhibits bright lines also, the positions and numbers of which depend on the substances of which the electrodes are formed. A platinum wire made incandescent by an electric current, gives no lines at all; and none are seen in the spectrum formed from the light of the solid carbon electrodes which produce the galvanic arch. Experiments made by Sir David Brewster, by passing solar or artificial light through different colored gases, led him to the conclusion that the dark lines are caused by absorption—an absorption which he supposed to take place in the earth's atmosphere.†

In the year 1808 the French Academy of Sciences proposed the problem of the double refraction of light as the subject of a prize to be awarded two years thereafter. The successful competitor for this prize was Malus. To him is due the discovery of the polarization of light by reflection. He was led to this remarkable discovery by an accident. In observing through a prism of Iceland spar the light reflected to his windows from those of the palace of the Luxembourg, he was surprised to see that, as he turned the prism around the ray, one of the two images vanished at every quarter revolution. By following up the indication thus given, he arrived at the important law that, when light is reflected from glass at an angle of 54° 35′, or from water at an angle of 52° 45′, it possesses all the properties which belong to the pencils into which a ray of ordinary light is divided by a doubly refracting crystal. Accordingly, if such a crystal be placed in the path of such a reflected ray, with the principal plane of the crystal, or a conjugate plane, in the plane of reflection, the ray will not be doubly refracted. But if the crystal be turned in azimuth, two rays will make their appearance, unequal at first in intensity, but becoming equal at the azimuth of 45°. Beyond this azimuth the ray which was previously most intense fades gradually away, while the other gains in strength, until, at 90°, the former disappears entirely, and the latter remains alone. These phenomena are repeated in every quadrant.

If the ray which has been reflected as above described be incident upon a second surface of glass, at the same angle, (54° 35′,) as at first, the plane of second reflection corresponding with that of the first, it is in part reflected and in part transmitted, as is the case with common light; but if the second plane of reflection be at an azimuth of 90° with that of the first, no reflection at all will occur, but the whole ray will be transmitted.

A ray of light therefore, which has undergone the modification which is produced by transmission through a doubly refracting crystal, or by reflection at an incidence of 54° 35′ from glass, or at that of 52° 45′ from water, seems to possess dissimilar physical properties on the sides which are at right angles to each other, and similar ones on the sides which are diametrically opposed. This

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* The important researches of Kirchhoff and Bunsen on the chemical relations of the fixed lines of the spectrum have been published since the preparation of these lectures.

† Kirchhoff and Bunsen have demonstrated that this absorption takes place (at least in case of many of the lines) in the atmosphere of the sun.

circumstance, from a sort of fanciful analogy which it presents with the relations of the poles of the magnet, has suggested the name *polarization*, to distinguish this condition of light.

An interesting experiment of Malus, illustrating the identity of the phenomena of polarization by reflection, and polarization by double refraction, is the following: Let a ray of light pass, at a perpendicular incidence, through a crystal of Iceland spar, undergoing division into two rays; and afterwards let these rays fall at an incidence of 52° 45′ on water, or of 54° 35′ on glass. Let then the crystal be turned in azimuth until the principal section coincides with the plane of reflection. The extraordinary ray will cease to be reflected altogether, though the ordinary ray undergoes reflection as usual. Turning the crystal once more in azimuth, until the principal section is 90° from the plane of reflection, the ordinary ray will, in its turn, wholly cease to be reflected, and the extraordinary ray will revive.

Another interesting and very curious experiment by Brewster, analogous to the foregoing, may be performed thus: Let the light of a candle or other luminous object be polarized by reflection, and afterward received, at the polarizing angle, upon a plate of plane glass, which has its plane of reflection in azimuth 90° from the plane of polarization. It will, as we have just seen, be wholly transmitted, so that, to an eye placed anywhere in the direction in which reflection would ordinarily occur, the radiant will be invisible. The eye remaining in this position, let now another person breathe upon the glass plate, and instantly the luminous object will appear, and will continue to be seen until the film of moisture left by the breath has evaporated. This is because the polarizing angle for water is not the same as that for glass.

The experiment may be varied and made still more striking by placing a second plate by the side of the first, and adjusting this one to the polarizing angle for water. The radiant will then be visible in the second plate, but not in the first. In this state of things, if both plates be breathed on simultaneously, the light in the second plate will be extinguished and that in the first revived by the same breath.

It is only at the angles which have been mentioned that polarization by reflection is complete. But partial polarization takes place in reflection at any angle; being zero at the incidences 0° and 90°, and increasing from those incidences up to the polarizing angle.

Light is polarized by reflection from all polished surfaces: but it is only in the case of bodies whose indexes of refraction are in the neighborhood of 1.4 that the modification which it undergoes has the simplicity which belongs to the examples we are considering. The index of water is 1.336, and that of crown glass 1.48 to 1.53.

It was the conclusion of Malus that the angle of polarization of a given body is independent both of its refractive and of its dispersive power. Dr. Brewster, however, demonstrated that this angle depends on the refractive power; and is connected with it by the law that "the index of refraction of any body is the tangent of the angle of polarization."

From this law we derive one or two interesting consequences; first, at the angle of polarization the reflected ray is perpendicular to the refracted ray, for, putting ι for the angle of incidence, ρ for the angle of refraction, and n for the index, the law of Snellius gives us $n\sin\rho=\sin\iota$; and the law of Brewster, just mentioned, gives $n=\tan\iota$. Hence—

$$\tan\iota\,\sin\rho=\frac{\sin\iota}{\cos\iota}\,\sin\rho=\sin\iota;\ \text{or, } \sin\rho=\cos\iota, \text{ and } \iota+\rho=90^\circ.$$

Secondly, when light falls upon a transparent plate having parallel surfaces, if the angle of incidence at the first surface is the polarizing angle, the angle of incidence at the second surface will also be the polarizing angle for that surface

In this case ρ is the angle of incidence and ι the angle of refraction for the second surface, the index of refraction being $\frac{1}{n}$. And we have—

$$\tan\rho\sin\iota = \frac{\sin\rho}{\cos\rho}\sin\iota = \sin\rho;\ \text{or, } \sin\iota = \cos\rho, \text{ and } \iota + \rho = 90°$$

We have seen that when the two polarized rays into which a single ray of common light is divided by double refraction in passing through a rhomb of Iceland spar fall upon a second similar rhomb, they are both of them subdivided in most of the positions of the second rhomb; but that the intensities of the rays of each pair are unequal, except when the principal planes of the rhombs differ in azimuth 45°, and that one member of each pair disappears entirely when the principal planes are coincident or normal to each other. The inequality of intensity is variable, and is dependent on the angle between the principal planes. If one ray of either pair be observed through all its variations, it will be found to begin from zero of intensity, to increase regularly in brightness for 90°, and then to diminish through the second 90°, to zero again. The other member of the same pair passes through a similar series of changes, but its maxima correspond in azimuth to the minima of the first, and its minima to the maxima of the first.

A ray which has been polarized by reflection possesses the same character as those which have been produced by double refraction in Iceland spar; and accordingly, if such a ray be transmitted through a doubly refracting rhomb which is turned in azimuth in the manner just described, it will be divided into two rays which will alternately increase and diminish in intensity; and of which one will become zero in the azimuth 0° or 90° between its plane of polarization and the principal section of the rhomb. Assuming the united intensities of the two rays into which a single one is thus divided by double refraction to be equal to the total intensity of the original ray, Malus inferred that their several intensities should vary as the squares of the sines and the cosines of the azimuth. Thus, if I be put for the total original intensity and α for the azimuth, reckoned from the position of coincidence of the plane of polarization with the principal section of the rhomb, then the ordinary ray would have the intensity $I\cos^2\alpha$; and the extraordinary, $I\sin^2\alpha$. These values fulfil the condition of constancy of sum; since—

$$I\cos^2\alpha + I\sin^2\alpha = I.$$

If a ray which has been polarized by reflection fall, at the polarizing angle, upon a second mirror of transparent glass with parallel faces, it will be divided into two rays; one of which will be reflected and the other transmitted. When the second mirror is turned in azimuth around the incident ray, these two derivative rays will undergo changes of intensity somewhat resembling those which have just been described as produced by double refraction. When the two planes of reflection are coincident, the intensity of the reflected ray will be maximum, and that of the transmitted ray, minimum. This minimum will not, however, be zero. When the two planes differ in azimuth 90°, the intensity of the transmitted ray will be maximum, and that of the reflected ray, minimum. This minimum *will* be zero; and the simultaneous maximum of the transmitted ray will be equal to the total intensity of the incident light. The alternations in this case resemble, therefore, to a certain extent, those previously described as produced by double refraction; but they are not represented by the law of Malus.

The *plane of polarization*—an expression which we have just used without defining it—is the plane in which a polarized ray is capable of being reflected at the polarizing angle. Accordingly, when a ray of common light is polarized by reflection, the plane of incidence and reflection is itself the plane of polarization.

In the arrangement of two mirrors, as above described, when the second mirror is rotated in azimuth, its plane of incidence and reflection is constantly changing its inclination to the plane of polarization of the ray incident upon it. Suppose the incidence upon the second mirror *not* to be at the polarizing angle. It is found that after reflection in an oblique azimuth, the plane of polarization is nearer to the plane of reflection than it was at incidence. If the azimuth at incidence be represented by a, and that after reflection by a', there will be found to be a constant ratio between tan$a$ and tana'; tana' being always less than tan$a$. By many reflections, with the same azimuth between the mirrors, the plane of polarization may be brought indefinitely near to the plane of reflection; but it can never be made, in this way, absolutely coincident with it.

When common light is reflected from any surface at an angle greater or less than the polarizing angle it is found to be partially polarized: that is to say, it is made up of a mixture of polarized light with common light. By repeated reflections at the same incidence the polarization may be made sensibly complete. The number of reflections necessary for this purpose will be greater as the angle of incidence is further from the polarizing angle.

It must not be overlooked that, though at the angle which we have called the polarizing angle, all the light that is reflected is polarized, yet that this is after all but a small portion of the incident light. From a single surface of glass it amounts to less than eight per cent. The manner of determining this ratio will be seen hereafter. When, for purposes of experiment, it is desired to obtain a large and intense beam of polarized light, it has accordingly been found useful to employ many reflecting plates placed one upon another, forming a *bundle* or *pile*. It is obvious that the thinner these plates are made, (so that they are not so thin as to produce color,) the more convenient they will be in use, and, from the diminution of absorption, the more economical of light. Not fewer than sixteen are usually employed.

The amount of light reflected at different angles of incidence goes on increasing from 0° to 90°. The amount which is polarized in the reflected beam also goes on increasing, but not throughout the quadrant. For glass having the index 1.5, the incidence of maximum polarization is 79°. At this incidence the total intensity of the reflected light is expressed by the decimal 0.355, the intensity of the incident light being 1. The amount which is polarized in the reflected beam is, however, only 0.1518, which is still about double of that which is reflected at the polarizing angle. But, comparing this value with the foregoing 0.355, we shall see that it is less than half the total light reflected, (forty-four per cent.,) and accordingly it is not suited to exact experiments in polarization.

When a transparent reflector is employed as a polarizer the transmitted beam will be found to contain light which is polarized in a plane perpendicular to the plane of refraction. The amount of light so polarized is exactly equal to the amount polarized at the same time by reflection, and in the plane of reflection. And as the maximum amount polarized by reflection from one surface of glass having the index 1.5, is 0.1518, this also is the maximum amount which can be polarized at one surface by refraction. But since, at this angle of maximum polarization, the total reflection is only 0.355, the total transmission will be 0.645 and of this amount the polarized portion will be but twenty-three and a half per cent. But if this light, already partially polarized, be transmitted through other refracting surfaces, though it will continually lose in total intensity by reflection, it will gain in the *proportion* of the polarized light which it contains; and if the incidence is that of the polarizing angle for reflected light, the quantity transmitted *which is polarized*, will continue to increase *in absolute amount*, notwithstanding the decrease of *total intensity*, until polarized light only is transmitted. Moreover, if the number of refracting plates employed should happen to be greater than is necessary to produce complete polarization, the supernumerary plates will not reduce the amount of polarized light transmitted; since, at the incidence supposed, they are incapable of reflecting light polarized transversely to the plane of reflection. This statement presumes, of course, that the refracting surfaces are perfect, and that no light is lost by absorption in the media.

It is a curious fact, resulting from the polarizing power of a pile of glass plates, that the pile is more transparent when held at an obliquity greater than the angle of polarization than it is at that angle; and that the transparency increases with the obliquity. This is owing to the fact that the light which has been polarized by the first few laminæ undergoes very little loss by reflection on increasing the obliquity; but the amount *polarized* in those first refractions increases as the obliquity increases, more rapidly than the loss by reflection of the natural light falling on the same surfaces is increased. The intensity of the transmitted beam, therefore, becomes actually greater as the obliquity is greater: a fact which is the reverse of what happens with a single plate.

A remarkable fact in regard to the condition of light emitted at great obliquities from luminous solids or liquids, was discovered by Mr. Arago. Whenever the light of an incandescent body of either of these classes is examined as it proceeds directly from the body and with no great inclination to the luminous surface, it is found to be unpolarized. But when the rays whose obliquity to the surface is very considerable are the subject of examination, they are found to be partially polarized. The inference is, that these rays have been polarized by refraction; and hence that they must have originated beneath the surface of the luminous body. From the law of equality between the quantities of light simultaneously polarized by refraction and by reflection, it follows that there is a reflection toward the interior of such bodies, of some of the light which they generate. The light of flames and incandescent gases exhibits no such polarization.

The light of the sun is always unpolarized, whether it be examined at the limb or at the centre of the disk. From this observation, Arago was led to consider the luminous envelope of the sun to be gaseous, and not liquid or solid. An incidental corroboration of the ingenious suggestion of the elder Herschel in regard to the constitution of the solar photosphere, is thus derived from optics; and although that hypothesis is by no means universally received, and though there seems recently to have been manifested an increasing disposition among men of science to call it into question, it will be found difficult to reconcile the optical properties of the solar light with any supposition which implies that the luminous surface which we see is either liquid or solid.

In observations upon polarized light, there are some inconveniences attending the use of a mirror, which, when turned in azimuth, obliges the observer to change his own position; or of a doubly refracting prism or crystal, which presents two images often not sufficiently separated. Both these disadvantages are obviated by means of a prism invented by Mr. Nicol, which is now in almost universal use. This contrivance is represented in the figure. It is an elongated rhomb, formed of Iceland spar, its length being about three times its breadth. Having been brought into this shape from the natural crystal, it is carefully sawed asunder in the plane which divides it symmetrically through its shortest diagonal, AD, and then reunited by means of Canada balsam. This substance is perfectly transparent, and has a refracting power whose index is 1.532, intermediate between those of the ordinary and extraordinary rays, viz., 1.654 and 1.488. The relative index between the crystal and the balsam for the ordinary ray is 1.0796, and the limiting angle of emergence from the former to the latter is 68°. The ordinary ray from R meets the surface, AD, at a greater angle than this, and is totally reflected at O. The extraordinary ray passes through. The sides of the prism are blackened to prevent a second reflection.

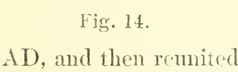

Fig. 14.

This ingenious contrivance is invaluable to the observer in this interesting branch of optical investigation. Its advantages are, however, in some respects limited. The necessary length of the prism, as compared with its lateral dimensions, renders it difficult to employ light of any considerable convergency or divergency. The cost of the construction of such prisms increases also very rapidly with their magnitude; and few have been made at all which measured more than an inch on the side. Those commonly found with opticians are much smaller than this.

The eye-piece of Mr. Delezenne is a very good substitute for Nicol's prism; although it affords a less intensity of light. In this, the surface CD is a polished mirror of black glass; ABD is a prism of transparent glass. R*r*, R*r*, rays of light falling at the polarizing incidence upon CD, are reflected at a perpendicular incidence upon the first surface of the glass prism; and after being totally reflected on AB, emerge at right angles to the surface AD, meeting the eye of the observer at O.

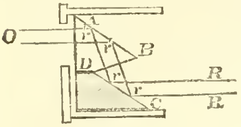

Fig 15.

Another convenient eye-piece, which may also serve, like Nicol's prism, as a polarizer for small beams, is formed of a lamina of tourmaline cut parallel to the axis. This mineral possesses the very remarkable property, when not in exceedingly thin laminæ, of suppressing one of the rays into which incident common light is divided by it, and transmitting the other. The ray transmitted, as in Nicol's prison, is the extraordinary ray. Cut perpendicularly to the axis a plate of tourmaline is opaque. Two equal plates, cut parallel to the axis, are opaque when crossed upon each other.

The disadvantages of the tourmaline eye-piece are, first, the color of the crystal, which mars the beauty of the tints exhibited by polarized light, and to some extent neutralizes them. It is rather unfortunate that the crystals which are least colored are usually bad polarizers. In this respect different crystals very much differ. Some, which are light green, transmit a notable amount of the ordinary ray even when quite thick. Those which polarize best are usually brown or yellowish brown. Occasionally one of this kind will be found which polarizes well without being very disagreeably dark. But an equal if not greater disadvantage of the tourmaline is the great brittleness of the crystal and the rarity of specimens in which fissures do not naturally exist. It is difficult, therefore, to obtain clear plates of any considerable size. Finally, the supply seems, of late years, not to have kept pace with the demand; and opticians intimate that it is almost impossible to obtain specimens fit for optical purposes at all.

A few years since Dr. Herapath, of London, announced the discovery of a property like that of the tourmaline, in artificially prepared crystals of the iodo-disulphate of quinine. These crystals are but slightly colored; and could they easily be prepared and made permanent would probably come into general use. Dr. Herapath succeeded in obtaining specimens half an inch across.

The peculiar property of the tourmaline was also early observed by Sir David Brewster, in agate; but that substance is not sufficiently transparent for the purposes of optical experiment.

When large polarizers are needed, resort must be had to reflection from mirrors made of black glass, which reflect only from the first surface, or of transparent glass whose surfaces are truly parallel. If great purity in the polarized beam is not an object of importance, bundles of thin plates may be employed as heretofore described, to polarize either by reflection or by refraction.

In the year 1811 Mr. Arago communicated to the Academy of Sciences of Paris, one of the most remarkable and beautiful discoveries which has ever been made in the history of optics. Upon examining thin plates of certain transparent crystals, such as mica, selenite, or quartz, by means of transmitted polarized light, he found that when the light was received upon the eye through a prism formed of Iceland spar, the richest conceivable colors made their appearance, which were complementary to each other in the two images, and which varied in intensity with the azimuth of the laminæ or of the prism.

When the principal plane of the prism coincides with the plane of polarization of the light, and the azimuth of the lamina is varied, the maximum brilliancy of coloring is found in the azimuth of 45° between the principal section of the lamina and the plane of polarization. When the azimuth is 0° or 90°, the color entirely vanishes, and the light appears entirely unchanged. At intermediate azimuths the color has an intermediate intensity, regularly increasing and diminishing between the positions of minimum and maximum. These variations, as well as the thickness of the laminæ themselves in which the phenomena appeared, satisfied Mr. Arago that the colors could not be owing to the same causes which produce the colors of Newton's rings. Still they had evidently some relation to the thickness; for it was not difficult to remove them entirely, either by considerably increasing the thickness or by excessively diminishing it. In the rotation of the lamina as just described, the colors which appeared between the successive positions of minimum were always the same in the same image. But when the lamina itself remained fixed, while the prism at the eye was rotated in azimuth, the two images interchanged their colors in passing each successive position of minimum.

If, instead of a doubly refracting prism as an eye-piece, a mirror, presented to the ray at the polarizing angle, be employed, only one of the images is reflected; but the other, if the mirror be transparent, will be seen in the light transmitted. In consequence of this separation of effects, Mr. Arago was led to distinguish the mirror when used in this way as the *analyzer*. In observations with the analyzer, then, it appears that when the lamina is rotated in azimuth, the same colors come and go in the successive quadrants; but when the analyzer itself is rotated, the colors in the alternate quadrants are complementary to each other.

The colors thus seen in crystalline laminæ recur in several successive series, as the thickness of the laminæ is increased. Accordingly, if in a plate of selenite we hollow out a spherical cavity of very large radius, we shall find it to exhibit several orders of rings resembling those of Newton, and following the same laws; though the thicknesses at which the colors of the same order occur are very much greater. According to the determinations of Biot, the comparative thicknesses at which the same colors appear in air, in Iceland spar, in quartz, in selenite, and in Siberian mica, are as the numbers 1, 13, 230, 230, and 440; the thickness for selenite and quartz being sensibly the same.

The limits of absolute thickness below which crystalline plates fail to give colors in polarized light, are, for selenite, 0.017 in.; for mica, 0.0323 in.; and for Iceland spar, 0.001 in. The *maximum* thickness for this last crystal is but six or seven one-thousandths of an inch. Mica and selenite are therefore prepared with facility for this class of chromatic experiments; but this is not equally true of Iceland spar. If a lamina of selenite—a mineral which is very easily wrought—be secured by transparent cement of any kind to a plate of glass, very fanciful effects may be produced by grinding it away unequally in different parts according to any definite pattern. Figures of various kinds, images of insects, flowers, gothic windows, &c., &c., may thus be prepared, which will come out in polarized light in very brilliant colors.

When laminæ are presented obliquely to the polarized ray and the inclination varied, the colors change with the obliquity; sometimes ascending in *order* with an increase and sometimes with a decrease of obliquity, according to the character of the crystal and the direction in which the lamina has been taken from it. For these experiments it is best to cut the laminæ parallel to the optic axes of the crystals.

If two laminæ, either or both of which exceed the limits at which colors are seen, but whose *difference* of thickness is within those limits, be placed one upon the other with their principal sections *crossed*—that is to say, placed at right angles to each other—colors will be seen corresponding to those of single laminæ whose thicknesses are the *differences* of those employed in the experiment. This supposes that the laminæ are of the same kind. If they are not, the actual thicknesses are not to be employed in the calculations, but what may be called the *bi-refringent equivalents of thickness*—that is to say, their measured thicknesses divided by the numbers expressing their chromatic relation to the plates of air which give Newton's rings—which latter numbers may be called their *chromatic equivalents.* If, then, the difference of these quotients, multiplied by the chromatic equivalent corresponding to the greater quotient, is within the limits at which the crystal to which the greater quotient belongs gives colors, the combination will give the color belonging to the value of that product.

If the laminæ belong to crystals of which one is positive and the other negative, they are not to be *crossed* in this experiment, but their principal sections must be parallel. This furnishes an easy test for determining whether a given crystal is positive or negative. Having prepared a lamina of the crystal to be examined, (which may be of any convenient thickness,) apply it upon laminæ of Iceland spar of different thicknesses, with the principal sections successively parallel and crossed. If the colors appear when the planes are parallel, the signs are opposite, since, either plate being too thick to produce color alone, the sum of their effects cannot, of course, do so. If the crystals are of similar sign, the colors will appear when the planes are crossed.

Another class of chromatic effects produced by crystalline plates viewed in polarized light was first observed by Dr. Wollaston in Iceland spar, in which the display is, perhaps, the most brilliant. In these cases, the crystal is cut perpendicularly across the axis. The arrangements for observation are the same as in the experiments already described. If a mirror be employed as an analyzer, and be turned to azimuth 90° before the introduction of the crystalline plate, no light will, of course, be reflected to the eye. But the moment the crystal is introduced a system of concentric rings will make its appearance, colored with the richest conceivable tints, and marked by a black cross, whose arms are in the plane of reflection, and at right angles to it, passing through the centre.

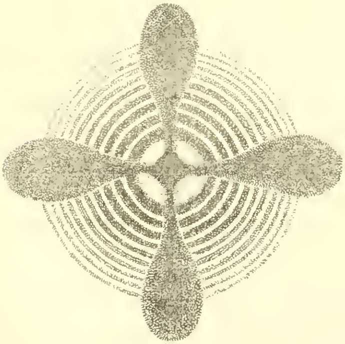
Fig. 16.


Fig. 17.

The ends of these arms are enlarged, and have the appearance of brushes. If the analyzer is transparent, another set of rings may be seen by the transmitted light, in which the colors will be complementary to the former, and the cross will be white. As the analyzing mirror is revolved in azimuth, the colors fade and a new set of rings gradually appears with colors complementary to the first, and distinguished by a white cross. In short, in this case, the colors before transmitted are reflected, and those before reflected are transmitted. The annexed figures exhibit the two aspects of the rings which have been just described.

These rings make their appearance at thicknesses much greater than those which produce color in laminæ parallel to the axis.

In examining plates of quartz cut across the axis as above described, Mr. Arago observed a peculiarity of a remarkable kind, which is scarcely found in any other natural crystal. The centre of the field was not dark in any position of the analyzer, but was deeply and uniformly colored with a tint which varied as the analyzer was turned. When a bi-refringent prism was employed as an analyzer, the two images seen were constantly complementary in color, and as the analyzer was turned they ascended in tint, in the order of Newton's scale, from red to violet. Mr. Biot, in subsequent experiments, discovered that in some crystals the ascent of the tints in the scale is produced by a right-hand rotation, (the ordinary direction of a screw,) and in others, by a left-hand rotation. These classes of crystals have been distinguished by the names right-handed and left-handed crystals, or *dextrogyre* and *lævogyre*. Sir John Herschel, at a later period, made the remarkable observation that these optical peculiarities of the crystals are associated with a geometrical or crystallographic peculiarity. The tetrahedral angles where the prism and terminal pyramid of the crystal meet, are sometimes replaced by planes which encroach more on the neighboring planes of one side than on those of the other. The same occasionally happens with the lateral edges of the crystal. These faces are called *plagihedral*. If, as the crystal is held in the hand horizontally, with the pyramidal vertex toward the observer, the plagihedral faces lean to the right—that is, if they encroach most upon the faces to the right of them—the crystal will be found to be optically dextrogyre, and the analyzer will have to be turned in the direction of the movements of the hands of a watch, in order that the tints may ascend.

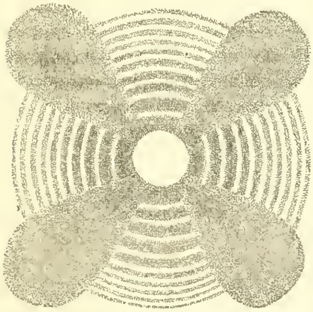
Fig. 18.

Sir David Brewster's observations on these crystals led to the discovery that, when the crystal is not very thick the uniformly tinted field is confined to the centre, and is surrounded by a system of rings resembling those seen in Iceland spar, but in which the cross is imperfect. The figure exhibits the appearance. He also found in that remarkable species of colored quartz called amethyst, veins of right-handed and left-handed crystallization alternating with each other in many parallel layers, and producing at their surfaces of contact lines of neutral character. In some specimens the layers were found to be so extremely thin as to neutralize the rotatory power of the whole crystal, and in these instances the ordinary system of rings with a perfect cross makes its appearance.

In all these observations upon crystals in the direction of their optic axes the number of rings is greatly increased by the use of monochromatic light. The intervals between the rings are also, in such light, intensely dark. In the case of quartz crystals, monochromatic light presents appearances in the centre very little different from those seen when the crystal is not present—that is to say, it exhibits, as the analyzer is turned, a succession of maxima and minima, separated from each other in azimuth 90°. But the absolute azimuths of these maxima and minima are no longer what they were before the introduction of the crystal: in other words, the plane of polarization has been turned to the right or to the left, according to the nature of the crystal, through an angular distance proportioned to the thickness of the crystal. The peculiar kind of polarization produced by quartz has, on this account, been called *rotatory* polarization.

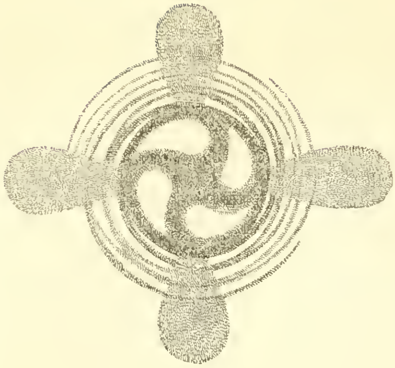

Fig 19

It will be easily conceived that a right handed and a left-handed crystal of equal thickness, superposed upon each other, will produce a resultant rotation equal to zero. But two such plates so superposed, examined in polarized light, exhibit a remarkable spiral cross, such as is seen in the figure annexed. These spirals were first observed by Mr. Airy, and are commonly known as Airy's spirals.

Two contrary plates of *unequal* thickness, superposed as above, produce an amount of rotation proportional to their difference of thickness.

The power of rotation of the same crystal is different for the different colors, being, on the undulatory theory of light, an inverse function of the length of the undulations. By employing the successive colors of the spectrum separately, Mr. Biot determined the absolute rotatory power of a crystalline plate of quartz one twenty-fifth of an inch in thickness, for each, as follows:

	°		°
Extreme red	17.4964	Limit, green and blue	30.0460
Limit, red and orange	20.4798	Limit, blue and indigo	34.5717
Limit, orange and yellow	22.3138	Limit, indigo and violet	37.6829
Limit, yellow and green	25.6752	Extreme violet	44.0827

This property of rotatory polarization does not exist in plates of quartz cut parallel to the axis. In such plates ordinary double refraction exists; but it is the extraordinary instead of the ordinary ray whose velocity is least, or the crystal is a *positive* one.

The physical cause of rotatory polarization is unknown. Mr. Biot supposed it to belong to the ultimate molecules of the substance; but this hypothesis Sir David Brewster believed to be disproved by the fact that the property ceases to appear in quartz whose crystalline structure has been destroyed by fusion. This argument seems, nevertheless, not to be conclusive. If the property belongs to the ultimate molecules, the fact that it does not appear when the crystals are examined across the axis, proves that a regular arrangement of them, presenting their similar sides in a common direction, is necessary for its display. Fusion breaks up the regular arrangement, and thus destroys this essential condition. The fact, however, that different crystals turn the ray in different directions, is apparently decisive against the hypothesis of Mr. Biot; and the connexion of this difference of property with difference of crystallographic modification, seems to indicate that the phenomenon is an effect of the structural arrangement of the molecules. Indeed, it is observed, in the fracture of quartz crystals, that there is occasionally something actually resembling a spiral arrangement of parts.

The double refraction of quartz along its axis was experimentally analyzed by Fresnel by means of a very ingenious arrangement. The difference of velocity of the two rays being so slight as to render their separation by ordinary expedients difficult, he devised and constructed a compound prism by which to double their divergency. In the annexed figure, ABF and CDF represent similar triangular prisms of right-handed quartz, with the faces AB, CD, cut perpendicularly to the axis. The obtuse-angled prism BFD, having the angle BFD equal to the supplement of 2AFB, has its base, BD, parallel to the axis of a crystal of left-handed quartz. The incident ray II′ falling perpendicularly upon AB, is separated into two, whose

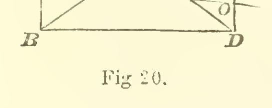

Fig 20.

velocities differ, but which pursue the same path, which is the axis. At the surface BF their paths become different, the velocity of one of them passing from — to +, and that of the other from + to —. At the surface FD this divergency is increased, the velocities again interchanging their relations. At final emergence from the face CD, the divergency will be further slightly increased in consequence of the inclination of the emergent rays to the surface. By this arrangement a sufficient separation of the two rays is obtained to make it possible to examine them singly. And it is obvious that a duplication of the system of prisms here shown, or a still greater increase in the number of elements employed, would, if necessary, make the separation still wider.

If quartz were like other uniaxial crystals in the law governing refraction along its axis—that is, if the velocities of the two rays were in that direction equal in this crystal as they are in others—the system of prisms just described would produce no separation of the rays. The fact of the separation proves quartz to be in this respect an exceptional case. When the separated rays are examined, however, the extent to which quartz is exceptional is discovered to be much greater than is implied in the difference just indicated. The peculiarities are the following, and are true of either of the separated rays.

Examined with a doubly refracting prism, two perfectly equal images appear in all azimuths of the prism. Received upon a mirror at the polarizing angle, equal reflection takes place in all azimuths of the mirror. In these respects the rays resemble ordinary unpolarized light.

But in the following particulars they differ: Transmitted through thin crystalline plates they display, on being analyzed, tints like those produced by polarized light, only they are such tints as ordinary polarized light produces in thicknesses of crystal greater or less, by a determinate amount, than those used in the experiment.

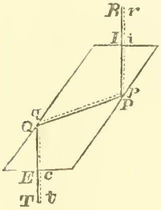

Fig. 21.

Transmitted through a rhomb of glass, like that represented in the figure, of which the acute dihedral angles are 54½, they emerge, after two internal total reflections, at Q and P, polarized in planes, one in azimuth 45° on the right, and the other in azimuth 45° on the left, of the plane of reflection. If both are transmitted through the rhomb simultaneously, so as to emerge together, they will form a single ray polarized in the plane of reflection.

Rays in this condition are said to be *circularly* polarized And as it appears that a circularly polarized ray becomes *plane* polarized by two internal reflections in glass, at an angle of incidence of 54° 30′, the resultant plane of polarization being in azimuth 45° from the plane of reflection, it follows that a plane polarized ray may be circularly polarized by causing it to make two similar reflections, the plane of its original polarization being 45° in azimuth from that of the first reflection. This is effected by the use of a rhomb such as has been just described, and which, from its originator, has been called Fresnel's rhomb. It is obvious that, if a plane polarized ray be thus passed through *two* of Fresnel's rhombs successively, it will emerge plane polarized.

Mr. Fresnel was led to the discovery of the remarkable property of the rhomb which bears his name, by theoretic considerations. When light is passing from a denser to a rarer medium, the angle of refraction is greater than the angle of incidence, and the law of Snellius,

$$\frac{\sin\iota}{\sin\rho}=n,$$

gives a value for n, the index of refraction, less than unity. Now as 1 is the greatest possible sine, if we put $\sin\rho=1$, we shall have $\sin\iota=n$; and therefore ι itself less than 90°. For an incidence greater than this value of ι there can be no emergent ray; and hence this is called the *limiting angle*. For all incidences from ι to 90° the whole of the light is reflected; and this is what is meant by *total reflection* at second surfaces.

Mr. Fresnel found that the mathematical formulæ which he had deduced from his theory of light, to express the intensity of reflection at different incidences, became *imaginary* in the case of total reflection; and in reasoning on the probable causes of their failure, he was led to predict that a rhomb of glass, having the angles above stated, would produce precisely the effect which has just been described. Experiment proved the truth of this anticipation. The nature of the modification which light undergoes in these circumstances will be more fully explained further on.

Reflection from metals presents characters which resemble those of reflection from the second surface of transparent media. There is this difference: that common light *totally* reflected exhibits no traces of polarization; but common light reflected from metallic surfaces *is* partially polarized. When the incident light at second surfaces is polarized in an azimuth between 0° and 90° the modifications which it undergoes resemble those produced by metals. This subject was first systematically investigated by Sir David Brewster. He first discovered that polarized light, after having undergone one total reflection in an azimuth between 0° and 90°, produced colors, when examined with an analyzer, analogous to those produced by thin crystalline laminæ. He afterwards ascertained that a polarized ray which has undergone successive reflections from plane metallic mirrors placed parallel to each other, when the original azimuth of reflection is 45° from the plane of polarization, will exhibit similar tints. The angle of incidence at which the effect is best produced varies with different metals, but is in all, or nearly all cases, above 70° and below 80°. The brightness of the tints increases with the number of reflections.

Sir David Brewster also found this analogy between the effects of such a pair of parallel metallic mirrors and a pair of Fresnel's rhombs: that at a certain angle of incidence, different for different metals, the effect of the reflection on the first mirror would be exactly compensated by that on the second, and the ray would emerge plane polarized. But he found also this difference between the cases: that while (the azimuth of incidence being + 45°) the ultimate plane of polarization with the rhombs was — 45°, that with the metallic mirrors was always less than this, being for silver, in which it was greatest, — 39° 48′, and for galena in which it was least, no more than — 2°. There is also this additional and very remarkable difference: In the case of the rhombs, after the light has undergone reflection in the first, it will be restored to its original condition by the second, no matter what be the azimuth between the planes of reflection in the two rhombs. But in the case of the mirrors, if the second be turned in azimuth, it will no longer restore the ray, unless the *angle of incidence* be changed also. If it be turned quite round, the angle of incidence required to effect restoration will pass through a series of regular variations between determinate limits, which variations may be represented by the varying radii of an ellipse. It was on this account that the term *elliptical* polarization was originally applied to light in this physical condition. We shall see, further on, that the propriety of the term may be established on other grounds.

Common light reflected from metallic surfaces is more or less elliptically polarized. In fact, the recent investigations of Mr. Jamin and others have proved that there are very few substances which furnish by reflection from their surfaces absolutely pure plane polarized light. None are capable of doing so whose indexes of refraction exceed or fall short of 1.414. Water and glass do so sensibly; but in this respect they are nearly exceptional.

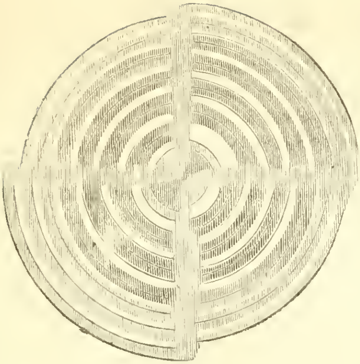

Fig. 22

The rings seen in crystals cut across the axis, when examined in circularly polarized light, exhibit some singular peculiarities. They are divided into quadrants by a cross which is neither very dark nor very bright, and which does not change in intensity with the revolution of the analyzer, but turns with it. The rings in the alternate quadrants are *unconformable*, those in one opposite pair being nearer to the centre, and those in the other more distant from the centre, by a quarter of an interval, than the corresponding rings in plane polarized light. This singular arrangement is shown in Fig. 22.

Mr. Airy found that light may be circularly polarized by *refraction*, in passing through laminæ of crystals which doubly refract; provided the thickness of the laminæ used is such as, on the undulatory theory of light, is just sufficient to effect a retardation of one of the rays produced by the double refraction, one-quarter of an undulation behind the other, or to advance it one-quarter of an undulation before the other. The mineral employed by him for this purpose, and which is more conveniently prepared of suitable thickness than most others, is mica; of which the laminæ are easily separable, and cleave in large sheets without breaking. A lamina reduced to a thickness proper to produce circular polarization is commonly called a "quarter-wave lamina."

For some time after the discoveries had been made of which a brief account has here been given, it was supposed that all doubly refracting crystals have but a single optic axis. In the year 1817, however, Sir David Brewster announced the remarkable fact that most crystals have two optic axes instead of one. The rings seen in crystals of two axes are elliptical, when the axes are so far apart that only one can be observed at a time; and they form *lemniscate* curves, or curves resembling the figure 8, when they are near together. In topaz the axes form an angle with each other of 65°, and the rings present the appearance shown in Fig. 23, when the analyzer is crossed upon the polarizer, the plane of the axes of the crystal being in azimuth 0° or 90°. This crystal possesses the peculiarity of showing its own rings without the help of an analyzer when the plate subjected to experiment is cut across the line intermediate between the axes, the opposite surfaces being parallel. In such a plate, in order that the ray may follow the line of one of the axes within the crystal, its angle of incidence must be 62½°. The angle of refraction will then be 32½°. The incident angle at either the first or the second surface will, therefore, be very nearly equal to the polarizing angle for the substance, since the reflected and refracted rays make an angle of 85° with each other; whereas, according to the law of Brewster, at the polarizing angle they should be at right angles. If, therefore, instead of observing the light transmitted through the plate, we

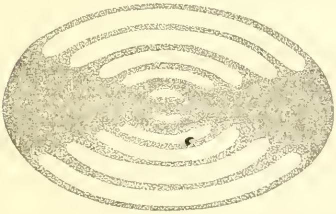

Fig. 23.

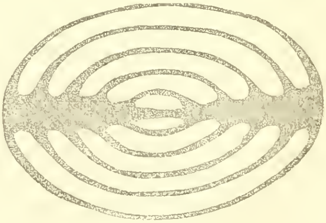

Fig. 24.

receive upon the eye the rays reflected from the second surface and emergent from the first, the reflecting surface itself forms an analyzer sufficiently perfect to exhibit the rings. But as the angle of reflection is not truly the polarizing angle, when the crystal is in azimuth 90° the dark band will not be as large as is the case in the rings seen with a better analyzer by transmitted light. Fig. 24 exhibits the appearance of these reflected rings.

In Figs. 25 and 26, which follow, are seen the appearances presented when the subject of examination is saltpetre, (nitrate of potash,) in which the axes are inclined to each other 6°. The plane of the axes of the crystal being brought into coincidence with the plane of polarization of the incident light, and the analyzer being crossed upon the polarizer, a system of lemniscate curves is seen, like that shown in Fig. 25, intersected by a dark cross, of which the bar coinciding in direction with the plane of the axes is longest. If the analyzer be turned 90°, the colors become complementary, and the cross becomes white; but if, the analyzer and polarizer remaining fixed, the crystal itself is turned in azimuth, the cross will break at the centre, forming two curves, which, when the rotation becomes forty-five degrees, assume the form of two opposite hyperbolas. This appearance is exhibited in Fig. 26.

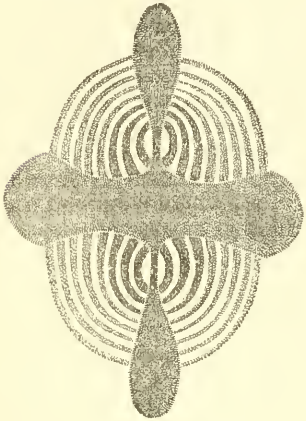

Fig. 25.

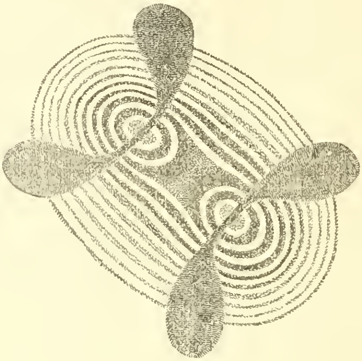

Fig. 26.

In the prosecution of his investigations, Sir David Brewster arrived at the discovery that the polarizing structure could be artificially produced in glass by heat or by rapid cooling; that this effect is transient when the heat is below the point of softening or fusing the substance; but that when it is carried beyond that point, and cooling rapidly follows, as in glass which is not annealed, the structure is permanent. He found that the same structure could be produced by pressure, by torsion, by tension, or by flexure; and traced the transient condition of the same kind produced by heat to the mechanical effects of unequal expansion. Any solid transparent substance, organic or mineral, was found by him to be capable of receiving this structure transiently or permanently. Among these may be named horn, indurated jellies, tortoise shell, gums, resins, the crystalline lenses of fishes or animals, &c., &c.

When cylinders, tubes, rhombs, or other geometrical forms of well-annealed glass are subjected to a sudden increase of temperature acting upon all their surface, as by immersing them in hot water or hot oil, there will be seen within them, by polarized light, systems of symmetrical figures, circular and concentric in cylinders, and dependent on the form of the solid for their shape in other cases, bearing a striking resemblance to the rings seen in crystals. Like those rings, these figures are marked by a cross, which changes from black to white with the rotation of the analyzer. But these figures will alter their forms if the glass be broken, which is not true of the rings formed in crystal. When the heat has fully penetrated the glass, and the interior temperature is uniform, the figures cease to be seen. At this time, if the heated glass be removed from the bath, and allowed to cool rapidly, a new system of figures will spring up within it. This is related to the former one, as the rings of a positive crystal are to those of a negative one; and, therefore, if two similar solids, in one of which the former set of figures is seen, and in the other the latter, be superposed when the intensities are equal, they will neutralize each other's effects, and the rings will disappear. This structure may be made permanent in the glass solids we have been considering by heating them nearly to the point of fusion and then suddenly cooling them. Many common articles of glass are so imperfectly annealed as to display the doubly refracting structure in a striking manner. The stoppers of bottles, if cut across the axis and polished, will invariably show it; so will the stems of wine glasses, the stirring-rods of the chemist's laboratory, and many, if not all, glass tubes.

The effects of heat are also remarkable in altering the doubly refracting character of crystals. Mr. Mitscherlich discovered that heat expands crystals unequally in different directions. Iceland spar is expanded in the direction of its axis, and slightly contracted at right angles to the axis. Its doubly refracting power is thus diminished. In sulphate of lime, which is a crystal of two axes inclined to each other 60°, he found that the inclination diminishes with elevation of temperature, until the two axes unite in one; after which, with further increase of heat, they open out in a plane at right angles to the first. Dr. Brewster discovered an example even more remarkable in *glauberite*. At the freezing point, this crystal has two optic axes for every color of the spectrum, the inclination of the axes of the red being greatest, and that of the violet being least. At ordinary temperatures it has two axes for red and one for violet. When heat is applied, the other axes approach, as in the case just described, and, after successively uniting, successively open out in the transverse plane.

In comparing the crystals which possess the power of double refraction,(being by far the greater number of the whole,) there is found to be a certain relation between the optical character of the crystal and the crystallographic structure. All crystals whose primitive form is the cube, the regular octahedron, or the rhomboidal dodecahedron—figures whose geometrical axes are all equal—are destitute of the property. All crystals which have one axis greater or less than the others are crystals of one optic axis. All crystals whose geometric axes are all three unequal have two axes of double refraction.

In the year 1815, Mr. Biot made the remarkable discovery that many liquids possess the power of rotatory polarization—a discovery which was independently made by Mr. Seebeck; the effect was first observed in oil of turpentine, but has since been found in most essential oils, in solutions of sugar, dextrine, the vegetable alkaloids, camphoric and tartaric acid, and the tartrates. In some of these substances the plane of polarization is turned to the right and in others to the left. Their relative rotatory forces are estimated by a comparison of the amount of angular change in azimuth produced upon a polarized ray in passing through a column of given length; but as yet there has been no universal agreement upon a standard length. The statements of experimenters, therefore, usually embrace both the angular rotation and the length of the column by which it has been produced, rendering a reduction to a common length necessary before a correct comparison can be instituted. It would perhaps be most convenient to adopt as a standard length, the length of the tube introduced by Mr. Soleil into his *saccharimeter,* or instrument for measuring the rotation in solutions of sugar, which is twenty centimetres. With this length the dextro-gyration of the oil of bitter oranges is, for red light, 157°.89, which is the maximum observed in this class of liquids. The laevo-gyration of narcotine, in alcohol and ether, is 151°.4; that of sulphate of quinine, in water acidulated with sulphuric acid, is 192°.95 in the same direction. Solution of crystallizable cane sugar is dextrogyre; that of uncrystallizable cane sugar, or molasses, is lævogyre. Solution of sugar of grapes is also dextrogyre when prepared from the juice, and before solidification; but if evaporated to dryness and redissolved, it is lævogyre. Crystallizable cane sugar is made uncrystallizable by heat, and its rotatory power is accordingly reversed by the same cause. In many solutions the introduction of an acid modifies the rotatory power. Narcotine, from being −151°.4, becomes, after the addition of hydrochloric acid, +83°. Cane sugar has its rotatory power inverted in the same way. Upon this principle is founded the construction of Soleil's saccharimeter just mentioned. A solution of the sugar to be examined is made of a definite density, and its rotatory power observed in a tube twenty centimetres in length. There is then added to the solution a measured amount, one-tenth of its volume, of strong hydrochloric acid, and a heat of about 150° F., applied for ten minutes; after which it is cooled and observed again in a tube one-tenth longer than before. Its rotation will now be wholly negative. The original observation will give the difference between the rotatory effects of the crystallizable and uncrystallizable sugar present, and the second observation will give the sum of the same effects. From these data the relative quantities of the two kinds contained in the solution may be deduced. For convenience, tables to accompany the instrument are prepared in advance, from which the values sought may be found by inspection. A saccharimeter has also been contrived by Mr. Mitscherlich.

Mr. Pasteur has made a very elaborate examination of the salts of tartaric and paratartaric acid in their relations to polarized light. All the tartrates are dextrogyre; the paratartrates have no rotatory power at all. Mr. Pasteur made the interesting discovery that paratartaric acid which is the same as racemic, and which differs from tartaric acid only in having an additional atom of water, is composed of two acids, one of which has a positive and the other a negative rotatory power. The dextro-racemic acid is simply tartaric acid, and the dextro-racemates are tartrates. Paratartaric acid and its salts owe their neutral character to the balance of opposite forces belonging to their components.

In considering the crystalline forms of these different salts, Mr. Pasteur detected a relation between them and their polarizing properties, such as has already been described to exist in quartz; that is to say, the salts which possess rotatory power have plagihedral faces leaning in the direction of rotation. The crystals are all of the kind called by Mr. Weiss *hemihedral;* that is to say, not in all respects symmetrical. Mr. Pasteur observed that there are two kinds of hemihedral crystals, which he has distinguished as the *superposable* and the *non-superposable.* When a crystal, or any solid, or surface is such that another may be conceived or constructed like it in every particular as to form and dimensions, yet incapable of being made to occupy the same matrix or mould, such a crystal, or solid, or surface belongs to the class of the *non-superposable.* The image of the face in a mirror, as compared with the face itself; the left hand or the left foot, as compared with the right, and many analogous objects natural and artificial, may serve to illustrate this conception. Mr. Pasteur found that all the crystals whose salts possess the rotatory power are hemihedral and *non-superposable;* and, conversely, that all salts whose crystals are non-superposably hemihedral have the power of circular polarization, with two exceptions only thus far known, which are formiate of strontian and sulphate of magnesia. In the latter case the crystal is so very nearly superposable, that it is hardly surprising that it should not sensibly conform to the law. In the instance of the formiate of strontian, Mr. Pasteur thinks that the hemihedrism does not depend on the arrangement of atoms in the chemical molecule but on that of the physical molecules in the entire crystal; so that, on solution, the structure on which the rotatory power depends, disappears in the same manner as it is known to do in quartz on fusion. It is impossible within the limits to which we are here confined to pursue this interesting subject further.

Mr. Arago early made the discovery that the light which comes to us from the atmosphere is polarized. Observations made in the vertical plane passing through the sun show sensible polarization in that plane up to about 150° from the luminary—a point which can only be observed, therefore, when the sun is low. The polarization at this point becomes zero, and it is hence known as Arago's neutral point. Below this point down to the horizon, polarization is found in a horizontal plane. Mr. Babinet discovered a second neutral point 17° above the sun, and Dr. Brewster a third, 8° 30′ below. Neither of these is easy of observation, in consequence of the proximity of the sun himself and his great light. Between them the light is probably polarized horizontally; but the fact, for the reason just mentioned, has not been verified. The plane of polarization in the vertical between the neutral points of Arago and Babinet is easily accounted for by ascribing the polarization itself to direct reflection of the sun's rays from the molecules of the atmosphere. The polarization in a horizontal plane below Arago's point is a less simple phenomenon. It is believed, however, to be occasioned by rays which have undergone two reflections from the atmospheric molecules. Of the rays of this class those which will come most effectively to the eye of the observer will be such as are reflected in the lower parts of the atmosphere in planes nearly parallel to the horizon. These will, of course, be polarized in planes nearly horizontal, and if in force sufficient to overcome the light polarized vertically, will produce a resultant in their own direction. At an altitude at which the two opposite polarizations balance each other, will be found a neutral point, and this is the point of Arago.

Regarding atmospheric reflection of the sun's rays as the cause of atmospheric polarization, it will follow that every plane passing through the sun (in the superior portions of the atmosphere at least) must be a plane of polarization. This will therefore be true of the *hour-circle* or meridian in which the sun happens at any time to be. And as all hour-circles pass through the pole of the heavens, it results that a delicate polariscope, directed toward the pole, may follow the horary motion of this plane. Such a polariscope, furnished with a dial and index, becomes a chronometer. This is the principle of an elegant little instrument invented by Wheatstone, called the *polar clock*. When accurately adjusted, it will indicate, in the hands of a practiced observer, the apparent solar time within a very few minutes. It will operate even when the sky is overcast with clouds, provided there be an unobscured spot at the pole, through which the blue sky may be seen.

In the foregoing very succinct outline of the history of optical discovery, the object kept in view has been to present simply facts, without entering into any discussion of the physical causes to which they are to be attributed. It is now proposed to consider in what manner these facts may be most satisfactorily explained.

THEORIES OF LIGHT.

Two theories have been maintained in regard to the nature of light, either of which is supported by the authority of very illustrious names. According to the first of these, light is a material emanation thrown off by the luminous body, and its particles constantly traverse and fill the entire illuminated space, so long as the source continues unexhausted. According to the second, there is no transfer of *matter* from the source of light to the surrounding region, but there *is* a transfer of *force* through the medium of an elastic fluid which fills all space, and whose molecules in contact with the luminous body, being disturbed by that body, transmit the disturbance to those more remote, by means of undulations which succeed each other uninterruptedly until the cause which produced them ceases to act. The first of these two hypotheses seems to have been of very early origin. It received the sanction of Newton, and was made by him the basis of his reasonings in regard to optical phenomena. It is hence commonly called the Newtonian theory. Until an advanced period in the present century it may be said to have been the generally accepted theory. Laplace, in his great work on celestial mechanics, has founded all his investigations in regard to aberration and astronomical refraction upon it.

Yet it must be admitted by its advocates, if there remain any who adhere to it still, that it presents, even before we follow it into its applications to the explanation of the phenomena we have described, many serious difficulties. In the first place, if light consist of material particles, these particles must be of inconceivable minuteness, or their living force would be sufficient to destroy every structure, no matter how solid or how tenacious it might be, which they should encounter in their flight. A single grain of matter, moving with the velocity of light, would have a quantity of motion equal to that of a cannon ball of 100 pounds weight, moving with the velocity of 1,500 feet per second. But since destructive power is proportioned, not to the quantity of motion, but to the *living force*, which varies as the square of the velocity, a single grain of matter moving with the velocity of light would have a destructive power equal to that of a mass of 3,350 tons moving with the velocity of 1,500 feet. If light be material, therefore, its particles must be many millions of times less in weight than a single grain. We have no instruments sufficiently delicate to detect a weight so minute. Still it would be possible, by optical arrangements, to concentrate many millions of particles upon a single point. Attempts have been made to test the question by the use of such expedients. Dr. Priestley, in his History of Light and Colors, describes an experiment in which he directed the light of the sun, by means of a concave mirror having four square feet of surface, upon a balance of exceeding delicacy, without producing any sensible impression. The conclusion is expressed in his own words, as follows: "Now the light in the above experiment was collected from a surface of four square feet, which, reflecting only about half what falls on it, the quantity of matter contained in the rays of the sun incident upon a square foot and a half of surface in one second of time, ought to be no more than the twelve hundred millionth part of a grain."

Dr. Priestley does not consider that, in such an experiment, it is the moment, and not the weight, of the particles of light that would be measured. The amount of inertia in any balance, however delicate, is sufficient to render it an instrument not very well adapted to the purpose in view. The presence of the air is also a disadvantage, both on account of its own resistance to motion and on account of the currents created by the heat which attends the direction of the solar focus upon any solid. The following experiment by Mr. Bennet avoids these objections. This brief account is taken from Professor Lloyd's Essay on the Undulatory Theory, edition of 1857. "A slender straw was suspended horizontally by means of a single fibre of the spider's thread. To one end of this delicately suspended lever was attached a small piece of white paper, and the whole was enclosed within a glass vessel from which the air was withdrawn by the air-pump. The sun's rays were then concentrated by means of a large lens, and suffered to fall upon the paper, but without any perceptible effect." These results are negative, it is true, but it must be admitted that they are such as to render the truth of the material theory of light in the highest degree improbable.

Another difficulty in the way of this theory is found in the uniformity of velocity with which light reaches us from distances all but infinitely unequal, and from luminous bodies of every magnitude. This equality of velocity in the propagation of the light of the stars is evinced in the universality of the law of aberration. But it might be inferred from the equality of the refraction which all light, whether natural or artificial, undergoes in passing from medium to medium. Now, if light be material, it must be regarded as subject, like all other projectiles, to retardation by the gravitating power of the body from which it is emitted. And, moreover, it is a phenomenon inconceivable that so perpetual a shower of projectiles, so infinite in number, should all be thrown with the same initial velocity, and that this initial velocity should be the same for every source. The only hypothesis upon which it is possible to meet this last objection is to assume, according to a suggestion of Mr. Arago, that the eye is insensible to luminous impressions, except for a certain definite velocity of the luminiferous particles, or for that narrow range of variation of velocity, within which are embraced the velocities which we attribute to the different colors in refracting media.

In regard to the retardation of the particles by the attracting power of the luminous body itself, it may be observed that, with our present means of measurement, this would not be appreciable for distances so small as that which separates us from the sun, or even for distances no greater than the extreme dimensions of the solar system; at least without supposing an enormous increase in the mass of the luminous body beyond that of any aggregated form of matter known to us. An attracting body can destroy, in a projectile thrown from it, no greater an amount of velocity than it can impart to a material mass falling toward it. And this limit is reached if we suppose the falling body to commence its motion at an infinite distance. Now, the velocity acquired by a body falling from an infinite distance to the sun's surface, under the influence of solar attraction, would be less than four hundred miles (391 miles) per second; and of this velocity about fourteen-fifteenths (365.1 miles) would be acquired after passing the limit of the earth's orbit. But the body would be twenty-seven and a half days in reaching the sun after passing this limit, while light is only eight minutes and thirteen seconds in traversing the same immense space. The effect of an accelerating or retarding force being as its time of action, and, in this case, the two times to be compared being in the ratio of about one to four hundred and eighty, it may easily be shown that the retardation of light by solar attraction, during its transit from the sun to the earth, could not be so much as a mile per second in its velocity.

But the light of stars coming from distances so vast as to require years, and many years, to reach us, must undergo such retardation as to render aberration a phenomenon exceedingly variable, unless we admit Mr. Arago's assumption just mentioned in regard to the sensibility of the retina. Moreover, in cases in which the rays, in their long travel, had become reduced to velocities comparatively moderate, the gravitating power of heavy bodies near which they might pass, ought to produce a sensible deflection of their course, and modify, in a remarkable manner, the phenomena of occultations. Nothing of this kind is observed. It is here assumed that there may be suns much more massive than ours.

Laplace has examined the question, what ought to be the mass of a luminous body, in order that its gravitating power may be great enough to destroy the velocity of the particles of light entirely, at some distance less than infinite—the initial velocity being assumed to be that which observation has determined in the sunlight as it reaches us. The expression for the velocity acquired in falling from an infinite distance to the sun's surface—his mass being assumed to be unaltered, is—

$$v=\sqrt{\frac{2mgr^2}{R}},$$

in which m is the sun's mass, that of the earth being unity; g is the measure of the force of gravity at the earth's surface, being the velocity it is capable of imparting in one second, or $32\frac{1}{6}$ feet; r is the earth's radius, and R the radius of the sun, both expressed in feet. If we put $v = 192{,}700$ miles, (reduced to feet,) and make m indeterminate, we shall find that the mass must be increased 860,000,000,000 fold to be capable of creating, and therefore of destroying, a velocity equal to that of light. This supposes the bulk of the sun to be unaltered. But if the mass is increased without altering the density we shall have—

$$v=\sqrt{\frac{2mgr^2x^2}{R^3}},$$

in which x is the radius of the sun under its supposed enlargement; whence—

$$x=\frac{rR^{\frac{3}{2}}}{r\sqrt{2mg}}.$$

Replacing the symbols by their values, we find that the sun must be enlarged to nearly five hundred times his present diameter in order to possess the power of entirely arresting the progress of light, considered as material, at any distance. The surface of such a sun would extend more than seventy millions of miles beyond the orbit of Mars. That there may be bodies in the universe so large as this is possible, but we may esteem it hardly probable. If there are, and if light is material, they may be invisible to us.

A final objection to the material theory of light is found in the phenomena of refraction and reflection. This, though it seems to have been overlooked, is really the most serious of all. We have seen that the effect of the immense power of solar gravitation is insufficient to produce more than an inappreciable variation in the velocity of light, during the nearly eight minutes and a quarter which is occupied in its passage over the space between us and the sun; and yet, if the hypothesis we are considering be true, there is a force residing in the superficial stratum of transparent bodies—a stratum so thin that no attempt has ever been made, or can be made, to measure it—which is capable of instantaneously doubling and, in some instances, almost tripling this velocity. Thus light which has passed the surface of glass of antimony or chromate of lead must, if this theory is true, have its velocity raised, in the instant of passing, from 192,700 miles to 574,000 miles per second. In common glass the velocity becomes 289,000 miles. In ordinary reflection, also, the reflecting force has first to destroy the original velocity, and then to impart an equal velocity in the opposite direction. This is more easily conceivable than the acceleration produced by refraction, as it corresponds with the ordinary phenomena of elasticity. But refraction, on the theory we are considering, is only explicable on the hypothesis of attraction; and the immensity of an attracting force which is capable of accomplishing in so short a time what gravity is totally unequal to in a time greater beyond measure, is totally inconceivable.

But, if objections of this weighty description to the material theory of light did not exist, the impossibility of finding in it any satisfactory explanation of the remarkable phenomena which have presented themselves in the later progress of optical discovery, would be conclusive against it; while the opposing theory finds in these very phenomena its strongest recommendation to acceptance. It is to that theory, therefore, that attention will be confined in the following attempt to connect the facts which have been detailed with their probable causes. To repeat the imperfect explanations which have been founded on a hypothesis which is now generally abandoned, would be an unprofitable waste of time and space.

PART II.

UNDULATORY THEORY.

§ I. VIBRATION.

In order to understand the mode in which undulations are propagated through an elastic medium, it is necessary to attend first to the subject of vibration. If a body be so held in equilibrium between opposing forces, as that, when displaced from its position of repose, it is urged to return by a force always proportional to its distance from that point, then the time occupied in returning, supposing it to be left at liberty to obey this impulse, will be the same, whatever may be the magnitude of the displacement. Moreover, if the extent or amplitude of the displacement be taken as a radius, and a circle described about the point of repose as a centre, the velocity of the body in its returning movement will be proportional, at every point of its path, to the perpendicular to the path drawn from that point and cut off by the circular arc. Thus, if C be the point of repose, and CA the amplitude of displacement, the force urging the body to return being proportional to CA, CF, CH, &c., when the body is at the points A, F, and H, then the velocities at these several points will be proportional to zero at A, to EF at F, and to GH at H.

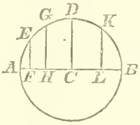

Fig 27.

These elementary propositions in physics, which admit of easy demonstration,*

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*The demonstration referred to is the following: Put CA $=a$, CH $=x$. Put also t for the time from the beginning of movement, v for the velocity, and p^2 for the force drawing the body toward C at the distance unity. Then if dx be the small space described in the small time dt, $vdt=-dx$; dx being negative because, when v is increasing, it diminishes x.

Also, if dv be the small increase of velocity in the time dt, we shall have the force, at the distance x, equal to p^2x, and $p^2xdt=dv$. Whence,

$$vdv=-p^2xdx;\text{ and } v^2=-p^2x^2+C.$$

But when $v=0$, $x=a$: consequently,

$$v^2=p^2(a^2-x^2);\text{ or, } v=p\sqrt{a^2-x^2}=\text{(in the figure) } p\sqrt{CA^2-CH^2}=p.\overline{GH}.$$

Therefore v is proportional to GH. Also, the time of vibration is constant, irrespective of the value of a. For, substituting in the first equation the value of v just found,

$$p\sqrt{a^2-x^2}.dt=-dx;\text{ or, } dt=\frac{-dx}{p\sqrt{a^2-x^2}}.$$

This gives $t=-\frac{1}{p}\sin^{-1}\frac{x}{a}+C$.

When $t=0$, $x=a$: whence $t=\frac{1}{p}\left(90^\circ-\sin^{-1}\frac{x}{a}\right)=\frac{1}{p}\cos^{-1}\frac{x}{a}$.

Now, taking t' and t'' for two particular values of t, one at the beginning and the other at the end of a complete double vibration, $t''-t'$ will be the duration of the vibration, and will be measured by the increase which the arc $\cos^{-1}\frac{x}{a}$ undergoes during one complete series of all the possible values of $\frac{x}{a}$ in diminishing and in increasing order—that is, from $x=+a$ to $x=+a$ again. Hence, putting $\tau=t''-t'=$ the duration of a vibration, we have,

$$\tau=\frac{1}{p}\left(2\pi\,(m+1)-2\pi m\right)=\frac{2\pi}{p},\text{ which is constant.}$$

The symbol a disappears from this expression, showing that the duration of the vibration is independent of the amplitude.

We have here also a proposition essentially the same as that demonstrated by a different may be assumed as established. When the body in its return arrives at C, it will accordingly be moving with the velocity represented by CD, the radius of the circle, and its inertia will carry it forward in the direction CB. It will now be resisted by forces similar in degree but contrary in direction to those which urged it from A to C, and its velocity will decrease as it before increased, until it is brought to rest again at B, when it will once more return. Supposing no forces or resistances to be called into action but those embraced in our hypothesis, there is no reason why this reciprocating motion should not continue indefinitely.

We have an approximate illustration of the case under consideration in an ordinary pendulum. When the pendulum is drawn from the vertical position, the component of gravity which urges its return is a force very nearly proportional, at every instant, to its distance from the position of rest. Were its path a cycloidal instead of a circular arc, this proportionality would be rigorously exact. Its beats are therefore sensibly equal in time, whether it swing through twenty degrees or through only one.

If we suppose the pendulum to be so suspended that its vibrations are not of necessity restricted to a single plane, we shall be able to conceive, without much difficulty, what must happen in another case important to be considered, viz: that in which a body, already in a state of vibration, is acted upon by a second disturbing force, not directed in the same plane with the first. To simplify the supposition as much as possible, let us imagine that, at the moment when the body, in its return from A, is passing the centre C, it receives an impulse in the direction D, at right angles to its actual movement, capable of giving it, instantaneously, the same velocity towards D, which it already has towards B. By the law of the composition of forces, it will take the direction CM, which is the diagonal of the rectangle formed upon CB, CD; and its subsequent vibrations will be represented in extent and direction by the line NM. It will be seen that the extent of its excursions in the direction AB remains unaltered, since the lines MB and AN are parallel; but it performs, at the same time, an equal vibration in the direction DP, since DM and NP are also parallel.

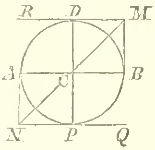

Fig 28.

Let us suppose, however, that the second impulse takes effect on the body not at the point C, of its greatest velocity, but at A, where its motion is null, and at the instant when it is about setting out on its return to C. It will vibrate, as before, between the parallels NP and MDR; but it will reach the limits of its vibration in this direction when it is at the middle of the vibration in the other. At the end of half a vibration, therefore, it will be found at D instead of at C, as in the former case; at the end of the next half at B; at the end of the third half at P; and at the end of the fourth, or of a complete double vibration, at A, the point of starting. Apparently, therefore, under these circumstances, the orbit of the body is a circle. We shall see that this is really so.

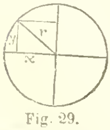

Fig. 29.

In fact, if we represent by r the radius vector of the body, and by x and y its co-ordinates to the axes AB and DP, we shall have the equation $x^2+y^2=r^2$. Whence, taking the differential—

$$xdx+ydy=rdr. \qquad (a)$$

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method, a little further on in the text, in regard to the measure of the *time elapsed since the beginning of the vibration*, at any given moment, and in any position of the vibrating body. Since t varies as $\cos^{-1}\frac{x}{a}$, it is obvious that, if a circle be described on the path of the vibrating body as a diameter, and if an ordinate to the circumference be drawn from the position of the vibrating body at any moment, the arc of the circle intercepted between the origin and this ordinate will be the measure of the time elapsed since the vibration began. The arc must be taken always in the same constant direction around the circumference, and the ordinate must be positive for the advancing and negative for the returning movement. In like manner, the arc intercepted between *two* such ordinates, will measure the time intervening between the moments when the vibrating body occupied the points from which the ordinates are drawn.

Now, by the law of velocities above given, if a be put for the maximum velocity, or that which the body has in passing C, and if φ be put for the arc of the circle on AB which is included between the origin, A, and y, the velocity in the direction AB, will be $a\sin\varphi$; and that in the direction CD, $a\cos\varphi$. Hence, for the differentials of the co-ordinates x and y, we have, putting t for the time—

$$dx = -a\sin\varphi dt; \text{ and } dy = a\cos\varphi dt.$$

But by construction $x = r\cos\varphi$, and $y = r\sin\varphi$.

Substituting these values in equation (a) there results—

$$-ar\sin\varphi\cos\varphi dt + ar\sin\varphi\cos\varphi dt = rdr.$$

$$\text{Or, } \frac{dr}{dt} = 0.$$

From which it appears that the radius vector is constant and the orbit a circle.

Also the motion in the circle is uniform. For if $d\varphi$ be the increment of the arc,

$$d\varphi^2 = dx^2 + dy^2.$$

And substituting the values of dx and dy, given above, we have,

$$d\varphi^2 = a^2\sin^2\varphi dt^2 + a^2\cos^2\varphi dt^2 = a^2dt^2.$$

$$\text{And } \frac{d\varphi}{dt} = v = a.$$

That is to say, the velocity of the movement in the circle will be uniform, and will be equal to the maximum velocity of the plane vibration.

Hence it follows that if, at the moment when a body, vibrating in a rectilinear path, is at the limit of its movement, a second body sets out from the same point, in a circle of which the path of the first is a diameter, with a uniform velocity equal to the maximum velocity of the first, the line which joins the two will move parallel to itself, and will always be perpendicular to the path of the plane vibration.

We hence obtain a convenient measure of the time which has elapsed since the beginning of vibration, when the body is at any point, as H, Fig. 27, of its path. For, taking as the unit of time the duration of a complete double vibration, and employing the ordinary symbol, 2π, to denote the circumference of a circle whose radius is 1, $2\pi t$ may express the entire space passed over by a body making its revolutions in such a circle isochronously with the movements of the vibrating body, and t (whether its value be integral or fractional) will then denote at once the number of vibrations which have taken place and the number of units of time which have elapsed since the beginning. Assuming the starting point to be at the commencement of a double vibration, every integral value of t will denote a return to the original position, and every fractional excess will denote a corresponding progress in a new vibration. Thus if the body be at H, the fractional part of $2\pi t$ will be the arc AG, and this will have the same ratio to the entire circle which the time of describing the portion of path, AH, has to the total time of double vibration.

Let us now suppose that the second impulse (still normal to the first) is not equal to the first, but greater or smaller; and that the vibration which it would independently produce has an amplitude (measured from the centre) represented by a'. The figure of the orbit will, in this case, be an ellipse, with the greater or lesser axis in the direction of the original vibration, according as a is greater or less than a'. The velocities in the direction of a will still, as before, be represented by $a\sin 2\pi t$, while those in the direction of a' will be represented by $a'\cos 2\pi t$; and these expressions will also stand for the ordinates of the orbit, y and x. $2\pi t$ here takes the place of the former symbol, φ.

Let us now suppose that the second impulse, though still normal to the first, is not imparted either at the limit or in the middle of its path, but at a point corresponding to $2\pi t$, where t may have any value whatsoever. The velocity of the body at the time t will be $a\sin 2\pi t$. The velocity produced by the second impulse necessarily commences with the maximum:—, that is, with the velocity belonging to the time $t=\frac{1}{4}$:— and hence is $a'.\sin\frac{1}{2}\pi$, or $a'.\sin 90°$. Then the difference of the two, in respect to *phase*—that is, to the *degree of their advancement* in their respective vibrations—will be $2\pi t-90°$, or $90°-2\pi t$; which, for convenience, put equal to θ. After a further time t', the two velocities will be—

1. $a.\sin(2\pi t+2\pi t')=a.\sin(90°\pm\theta+2\pi t')=a.\cos(2\pi t'\pm\theta)=y.$
2. $a'.\sin(90°+2\pi t')=a'.\cos 2\pi t'=x.$

Expanding y, and eliminating $2\pi t'$, there results the equation,

$$a'^2y^2+a^2x^2-2aa'xy.\cos\theta=a^2a'.^2\sin^2\theta. \qquad [1.]$$

This is the general equation of the ellipse referred to its centre. It follows, therefore, that any two impulses, applied in directions at right angles to each other to a body susceptible of vibration, will cause the body to describe an elliptical orbit, whatever be the interval between the impulses.

If, however, we suppose the second impulse to be in the direction of the original vibration, and not at right angles to it; and, as before, that there is a difference of time corresponding to the arc θ, then the body will be impelled by two conspiring or conflicting forces, capable of producing the simultaneous velocities, $a\sin\varphi$, and $a'\sin(\varphi+\theta)$.

Let us put $(a+a'\cos\theta)^2+(a'\sin\theta)^2=\mathrm{A}^2$.

$$\text{Or, } \frac{(a+a'\cos\theta)^2}{\mathrm{A}^2}+\frac{(a'\sin\theta)^2}{\mathrm{A}^2}=1=\cos^2\omega+\sin^2\omega.$$

the symbol ω denoting a determinate angle. Then,

$$a+a'\cos\theta=\mathrm{A}\cos\omega\,;\ \text{and}\ a'\sin\theta=\mathrm{A}\sin\omega.$$

Let the first of these equations be multiplied by $\sin\varphi$, and the second by $\cos\varphi$: their sum, added member for member, will be—

$$a\sin\varphi+a'\sin\varphi\cos\theta+a'\cos\varphi\sin\theta=\mathrm{A}\sin\varphi\cos\omega+\mathrm{A}\cos\varphi\sin\omega.$$

$$\text{Or, } a\sin\varphi+a'\sin(\varphi+\theta)=\mathrm{A}\sin(\varphi+\omega). \qquad [2.]$$

The first member of this equation is the expression for the velocity which the body will possess at any time answering to φ, after the commencement of the vibration a, which is least advanced in phase, and the second member shows that this velocity is that which would exist in the body at the same time, had it been acted on by one impulse only, capable of imparting the velocity A, and applied at a moment earlier than a by the time corresponding to ω, and later than a', by the time corresponding to $\theta-\omega$.

The value of the velocity A, in terms of the original velocities a and a', may be obtained by developing the assumed equation above, when it takes the form,

$$\mathrm{A}^2=a^2+a'^2+2aa'\cos\theta\,;\ \text{or, } \mathrm{A}=\pm\sqrt{a^2+a'^2+2aa'\cos\theta}. \qquad [3.[$$

This expression is remarkable, as being the value of the diagonal of a parallelogram, of which the sides are a and a', and the angle of their inclination $=\theta$. In the figure annexed, let AB $=$ DC $=a$, and AD $=$ BC $=a'$. Also let BCD $=$ ADE $=\theta$. AE being drawn perpendicular to CD produced, we have DE $=a'\cos\theta$, and AE $=a'\sin\theta$. Then,

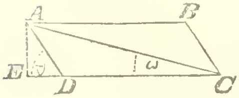

Fig. 30.

$$\overline{AC}^2 = \overline{EC}^2 + \overline{AE}^2 = (DC+ED)^2 + \overline{AE}^2 = (a + a'\cos\theta)^2 + (a'\sin\theta)^2 = A^2.$$

$$\text{Also, } AE = a'\sin\theta = A.\sin ACD. \quad \text{And } CE = a + a'\cos\theta = A.\cos ACD.$$

Therefore, ACD $=\omega$, or the angle between the diagonal and the side denoting the force corresponding to the component which is least advanced in phase, is the measure of the interval of time between that component and the resultant, or the time earlier than a, at which A must be applied in order to produce alone precisely the same succession of vibrations in phase and in force which is produced by the combined action of a and a'.

If the second impulse be oblique to the first, it may be decomposed into two components, one acting in the direction of a, and the other at right angles to a. The effects of these may be successively estimated according to the principles already illustrated. And the same principles may be applied to the determination of the resultant of any number of impulses, acting in all possible directions. The important conclusion is that, though the form of the path and the amplitude of the oscillations of the body may be altered, yet they are, in all cases, capable of determination, and the time of the vibration will be invariable.

§ II UNDULATION.

Hitherto we have supposed that the vibrating body imparts none of its motion to surrounding bodies. This is a case which can never be experimentally realized, and, if it could, it is not the case which concerns us at present. We wish to show the connexion between *vibration* and *undulation*, and, to this end, we must suppose the body in vibration to be immersed in an elastic fluid, whose particles are set in motion by it. Such a fluid is the atmosphere; and it is matter of common knowledge that *sound* is an effect of undulations produced in the air by vibrating bodies. It is also well known that sound does not attend all movements in the air, not even all movements of vibration. A certain rapidity is required, for a reason which will presently appear.

An elastic fluid may be described as one whose particles tend to recede from each other, or have the same mechanical relations as if they possessed the property of mutual repulsion. The distances between the particles, in the case of such elastic fluids as actually exist, are probably very great compared with the magnitude of the particles themselves. When the fluid is at rest, each particle is held in equilibrium by the repulsions of its neighbors. If a slow movement be excited among these particles, they will not, to any material degree, alter their distances from each other; but, if any one particle, or any stratum of particles, be driven toward those adjacent, with such suddenness that the *inertia* of the latter is brought sensibly into play, then the distances will be momentarily diminished, or there will occur a local compression of the fluid at that place. This impulse, it is evident, must come from some body foreign to the fluid, for by the definition it must appear that the fluid is incapable of unequally compressing itself. Suppose, then, the compressing body, after making its sudden advance, to stop with equal suddenness. The repelling force between the first and second strata will exceed that between the second and third; but the first stratum cannot recede on account of the obstacle. The second, therefore, will advance, diminishing its distance from the third, and so calling a greater repulsive force into activity between those strata also. The third will then advance, then the fourth, then the fifth and so on. It thus appears that the movement originally communicated to the first stratum will pass from stratum to stratum through the whole fluid. Each stratum, moreover, will come to a state of permanent rest the instant the next has taken up the movement. This is in accordance with the well known law of impact of equal elastic bodies. There is, therefore, no *vibration* in *this* case. But the progress of the movement is *uniform* throughout the medium, and the velocity of transmission is the same, (or sensibly so,) whether the molecular movement be large or small. This last fact we learn experimentally, though it may be deduced from considerations entirely theoretical, as will presently be shown.

A molecular movement, like what has just been described, may be called a *tremor*. A succession of tremors, swelling and sinking in magnitude, each so excessively brief that their successive differences are comparatively null, constitutes an *undulation*. Such a succession of tremors must be produced in the air by a vibrating sonorous body. Take, for instance, one of the steel springs of a musical box. It traverses, in its vibration, but a very minute portion of space, and its duration occupies an equally minute portion of time. But as, at the end of its path, its velocity is, for an instant, zero, while, in the middle, it is very great, we know, from the general principles laid down above, that, at intermediate points, it must have as many intermediate values as it is possible to imagine points, and we are able, moreover, to give a definite mathematical expression to those values. These various velocities, beginning and ending with zero, and passing through an intermediate maximum, will be successively imparted to the stratum of particles of air which is in contact with the spring. From this stratum they will be transferred to the second, from the second to the third, and so on.

And here, to the clear understanding of this subject, it is necessary to take into consideration a very important fact, viz., that, however rapid may be the motion of the spring, the tremors which the air takes up from it advance with a velocity vastly greater. Let us suppose, for instance, that the spring makes a thousand simple vibrations, or five hundred double vibrations, in a second, which is somewhat below the number corresponding to the tenor C, and that the amplitude of each vibration, measured between the extreme limits of its excursions in both directions from the position of equilibrium, is one twentieth of an inch. In one second after beginning to vibrate it will have described only fifty inches, but in the mean time the tremor generated by its first movement will have passed over eleven hundred and thirty feet. In this case, accordingly, the tremor will have more than two hundred and seventy times the mean velocity of the vibration. Thus, while the spring is advancing from its first zero of velocity to its maximum, a distance of one fortieth of an inch, the first and feeblest tremor which it excited will have reached a point nearly seven inches in advance of it, and the stratum next before it will be receiving the most energetic tremor which it is capable of imparting. When it reaches the second zero the first tremor will be fourteen inches in advance, the middle or maximum will be seven inches in advance, and the stratum next it will have just received a minimum impulse corresponding to the first. Were the spring to remain in this position, the undulation would pass on, leaving the air in its vicinity at rest. But as it instantly commences its return, it withdraws from the stratum in contact with it the support it afforded against the repulsive forces of the more advanced particles; and accordingly that stratum follows the spring, producing a rarefaction, or increase of distance, between the first and second. The repellent forces between the first and second strata will thus be diminished, so that those between the second and third will predominate. The second stratum will consequently follow the first, and in like manner each successive stratum throughout the fluid will successively move backward toward the one before it; or a tremor will affect every stratum of particles, as in the case before considered, but will differ from that in this particular, viz: that, in the latter case, the particles are moving in *one direction*, while the tremor is advancing in *the contrary direction*, whereas in the case we first considered the movement of the particles is *the same* in direction with that of the progress of the tremor.

It is easily seen that the whole series of tremors produced by the return vibration will generate an undulation equal in length to the former, and differing from it only in the *direction of movement* of the particles. This is called the *wave by rarefaction;* the former, the *wave by condensation.* The two are complementary to each other, and both are to be understood as included when in optics we speak of "an undulation."

A visible illustration of the relation between vibration and undulation, which we have been endeavoring, in what precedes, to explain, may serve a useful purpose in facilitating the formation of correct conceptions. Let AB represent

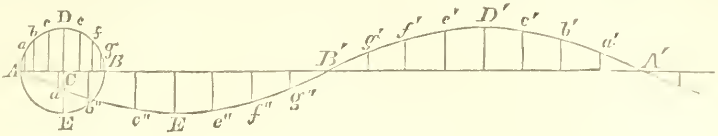

Fig. 31.

the range of movement of a vibrating body, C being the centre of force and the position of equilibrium. Upon AB describe the circle ADBE. Divide the semi-circumference A D B into any convenient number of equal parts, say eight, and from the points of division drop perpendiculars upon AB. Take AA' equal to the entire length of a wave, and bisect it at B'. Divide AB' and B'A' each into eight equal parts. From the points so found draw perpendiculars, upward on B'A', and downward on AB'. Cut off these perpendiculars to equality with those before drawn in the circle, in reversed order for A'B', and in the same order for AB', and through their extremities, as thus determined, draw the curve AE'B'D'A'. The ordinates to this curve will represent the velocities of the molecular movements which will be in activity along the line AB'A', at the end of one complete double vibration of the exciting body in AB.

The point in which this illustration fails to convey a correct impression is in the immense exaggeration of the extent of the range AB, of the vibrating body, as compared with AA', the length of the undulation. In sonorous undulations this latter usually exceeds AB some hundreds of times. Of course, no figure which could be introduced here could preserve these proportions without making AB imperceptible.

Let us now return for a moment to the assumption, which, as we have said, experiment, in the case of sound, confirms, that the velocity of wave propagation will be uniform. This assumption, antecedently to any experiment on the subject, might be justified by the consideration that each successive molecule, or stratum of molecules, commences its motion under precisely the same circumstances as those of the molecules before it; taken along with the ascertained fact that the forces of recoil of elastic bodies in general are proportional to the extent of disturbance. There is no circumstance which could make the time occupied by one stratum of particles in transmitting the movement to the next, longer or shorter than that similarly occupied by any other; unless it should be the decreasing amplitude of the molecular excursions, as, in the expanding wave, the motion is divided among a greater number of particles; and this circumstance, under the observed law of elastic force just stated, can have no effect upon the time of completing the oscillation.

But the proposition admits of a more direct and not a difficult proof. If AB CDEF be a tube containing an elastic fluid—say air—open at CD, but with a movable piston at AF closing the tube completely; and if, in a very minute portion of time t, this piston be advanced from AF to A'F', propagating a tremor which just reaches BE when AF reaches A'F', then, for the instant, the fluid between A'F' and BE will be more dense than before, in the same ratio as its volume is less, or (the diameter of the tube being constant) in the ratio of AB

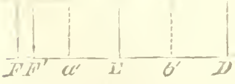

Fig. 32.

to A′B. Let the natural elasticity of the air be called e. By this natural elasticity we mean that force with which the air would tend to expand into a vacuum, or crush in the sides of a vessel from the interior of which the air has been removed. It is measured by the weight upon a square inch, of the column of mercury required to balance it, and this is compared with gravity by considering that it would move a body of a unit in bulk and density (say a cubic inch of water) as much faster than gravity alone, as it is greater than the weight of such a unit. If, then, g represent the velocity which gravity can impart in a second, if h be put for the height of the barometric column, and if D stand for the density of the mercury in the column,

$$e = ghD.$$

Employing e' to denote the increased elasticity of the compressed air between A′F′ and BE, and putting $AB = x$, $A'B = x'$, and $AA' = x''$, we have

$$e' = e\frac{x}{x'};\ e' - e = e\frac{x - x'}{x'} = e\frac{x''}{x'}.$$

This difference, $e' - e$, is the effective force by which BE is acted upon; for the space between BE and CD is filled with uncompressed air, which opposes the movement of BE by the force e, while the compressed air between A′F′ and BE urges it with the force e. Now, the velocity of movement imparted to a body by a given force is dependent not on the time and force only, but also on the mass to be moved. And it will evidently make no difference in the rapidity of propagation of movement, whether—mass remaining the same—we suppose BE to be the stratum of molecules which is nearest to AF, or whether we suppose it to be the middle stratum of a number filling the space half way between it and AF on the one hand, and CD on the other. In either case there will always be the same mass (m) which will be proportional to the length of the column x, and its density, which we may represent by d:—or m will always be as $x \times d$. Now the velocity v, which any force (as $e' - e$) will generate in the minute time t in this mass, will be

$$v = \frac{e' - e}{m}t;$$

or, substituting the values of $e' - e$ and m,

$$v = \frac{ex'\,t}{xx'd}.$$

And the velocity of wave-propagation (which may be denoted by V) multiplied into the time t, will give x, the distance the tremor has advanced in that time; or $x = Vt$. Moreover, since x'' is the distance moved by the molecules in the time t, it must have the same ratio to x, or to x', (for the difference is so slight as not sensibly to affect the ratio) that v has to V.

$$\text{Hence, } \frac{v}{V} = \frac{x''}{x'}.$$

Substituting these values, we have

$$v = \frac{evt}{dV^2t}:\ \text{or, } V^2 = \frac{e}{d};\ \text{and } V = \sqrt{\frac{e}{d}}. \qquad [4.]$$

But e and d are constants. Hence the velocity of the wave is uniform.

In this demonstration it has been tacitly assumed that the expansive force of a confined body of elastic fluid is inversely proportional to its bulk. This is called the law of Mariotte; and it is true if the temperature of the fluid, when in a state of dilatation, is the same as when it is compressed. But compression developes heat, and the expansive force increases directly as the temperature. On this account it is necessary to introduce into the expression for the value of V another constant, which is the quotient which arises from dividing the capacity of air for heat when it is expanded under a constant pressure, by its capacity when its pressure is raised under a constant volume. These capacities being represented by c' and c, the velocity becomes

$$V=\sqrt{\frac{ec'}{dc}}; \qquad [5.]$$

in which all the factors are still constant, and the velocity is uniform.

It will be further observed that the demonstration is independent of the amplitude of the vibration, and also of the length of the wave. The distance AA′ has no necessary relation to either the one or the other of those dimensions. The waves will be longer as the *time* occupied in a vibration is greater, and shorter as the time is less; but this will be only because, in the first case, a larger number of tremors by condensation occur, before the tremors by rarefaction commence, than in the second. These tremors, in longer and shorter waves, are arranged in larger or smaller groups, but every tremor, of whichever species, advances with the same velocity.

Observation proves that in sonorous waves this theoretic influence is true. In the range of musical tones, the waves corresponding to the deepest notes are two or three hundred times longer than those belonging to the highest; yet at any distance at which music is audible all the notes of a melody come to the ear without the slightest perceptible disturbance of their order.

As we shall presently have to apply the undulatory theory to the explanation of optical phenomena, this seems to be the proper place to anticipate an objection which has been made to such an application of it, founded upon this presumed constancy of velocity of propagation for waves of all lengths. The *dispersion* of light by refraction, or the separation of the elementary components of light by the prism, must, upon any theory, be regarded as irrefutable evidence that the velocities of these components are unequal.* If the undulatory theory be the true one, it is demonstrable that the undulations of the more refrangible rays are shorter than those of the less refrangible; and it is also necessary, on that theory, to admit that in point of fact the velocities of the same rays are less. This fact so directly conflicts with the proposition just now demonstrated, that for a long time it was regarded, and by some continues still to be regarded, as an almost insuperable objection to the wave theory of light. If we refer once more, however, to the demonstration, we shall see that it involves one assumption which is not strictly true—the assumption that AA′ is too insignificant a quantity to be regarded when subtracted from AB, and in therefore making AB=A′B. In the case of acoustic waves this assumption is admissible, and observation proves that it introduces no sensible error; but if the wave theory of light be the true one, the undulations themselves must be excessively minute; so that it is not only quite possible, but even probable, that the amplitude of the molecular excursions may have a very sensible ratio to the undulation length. Observe, however, that the admission of this supposition does not draw after it the consequence that the velocity of wave propagation must, *as a necessity*, vary with the varying lengths of the waves; it only vitiates our previous demonstration that this velocity is constant for waves of all lengths. The same constancy may continue to exist, but we must establish the truth, if it is one, by different methods of proof. It is even possible that it may exist under certain circumstances—that is, in certain media, and not in others. This last is the conclusion which was reached by Mr. Cauchy after an examina-

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* This must at least be true after refraction if not before.

tion of the question in its most general form. If we put λ for the wave length and τ for the time of vibration, and assume—

$$k=\frac{2\pi}{\lambda}, \qquad s=\frac{2\pi}{\tau}$$

the relation between these quantities is found by him to be expressed by the formula—

$$s^2=a_1k^2+a_2k^4+a_3k^6+a_4k^8+\&c.$$

in which the constants a_1, a_2, a_3, &c., are the same for all rays in the same medium, but differ for different media.

Now the velocity of every uniform movement is expressed by dividing the space passed over in any given time by the time of passing, and assuming a wave length for the space, and V for the velocity—

$$V=\frac{\lambda}{\tau}=\frac{\frac{2\pi}{\tau}}{\frac{2\pi}{\lambda}}=\frac{s}{k}. \quad \text{Hence, } k=\frac{s}{V}, \text{ and } k^2=\frac{s^2}{V^2},$$

and, by substitution,

$$V^2=a_1+a_2k^2+a_3k^4+\&c.$$

The velocity in the same medium is therefore a function of the length of the wave. But if, in any medium, the coefficients of the terms beyond the first are insensible, the equation will become $V^2=a_1$, or the velocity will be constant for all the rays. This he supposes to be true in a vacuum and in media which, like the air, do not sensibly disperse the light.

By reverting the first series it becomes—

$$k^2=A_1s^2+A_2s^4+A_3s^6+\&c.,$$

which is convergent like the first, and in which all the terms after the first three may be neglected.

Now, as $k=\frac{s}{V}$, if the velocity in vacuo be put equal to unity, we shall have, by substitution and reduction,

$$\frac{1}{V^2}=\mu^2=A_1+A_2s^2+A_3s^4. \qquad [6.]$$

in which μ is put for the index of refraction. This index is therefore a function of the time of vibration in vacuo, which time is necessarily unaltered by refraction; since the period in the second medium is determined by the period of the impulses, which impulses are the vibrations in the first. But the time of vibration in vacuo is inversely as the length of the wave. Hence, if we determine by observation three wave lengths in vacuo, and three corresponding indexes of refraction, we shall have the data for determining the three unknown constants, A_1, A_2, and A_3. For all other wave lengths the indexes may then be computed.

These theoretic conclusions can only be thoroughly tested by comparing the values deduced from the formula with the results of observation upon media of high dispersive powers. A very elaborate series of such comparisons was made by Prof. Baden Powell, which exhibits a general agreement between the computed and observed values quite within the limits of the probable errors of observation. We are therefore justified in saying that the different velocities of propagation of luminous undulations of different lengths no longer constitute a serious objection to the undulatory theory.

It will now be easily seen that so long as the movements of the vibrating body continue, undulations will continue to flow off from it through the air; but that the moment they cease, the undulations will cease also. Also that during the existence of any undulation there are *actually and simultaneously* existing, *somewhere in its length*, all the various velocities, positive and negative, which belong *successively*, and *one at a time*, to the *vibrating body*. Also that, as the progress of the undulation is uniform, its fractional parts are proportional in length to the times of performing them; and finally that the *molecular* velocity at any point may be represented by observing to what fractional part of a whole undulation that point corresponds, and taking the sine of the same fraction of an entire circle multiplied by the maximum velocity.

It must not be understood, because, in what has been said, mention has only been made of tremors in the direction of the original vibration, that therefore undulations are *confined* to that direction. It is impossible to disturb the equilibrium of the molecules of an elastic fluid, at any point or in any direction whatever, without disturbing that of all the adjacent molecules, and giving rise to tremors in all directions. At a distance from the vibrating body which is considerable compared with the size of the body, therefore, the front of the wave will be sensibly spherical, and when the body is small it may be regarded as strictly so.

It is furthermore manifest that the molecular movements need not be, as we have hitherto supposed them, *linear*, nor directed to and from the centre of the wave. As they may be derived from any of the forms of vibration which have been described, they may be circular or elliptical; and they may be performed in the tangent plane of the wave itself, instead along its radius. In the case of circular movements performed around the radius as an axis, an undulation will then consist of a chain of particles occupying positions less and less advanced, in the direction of progress, in their respective circles, until the last differs from the first by 360°. At any instant, therefore, the particles in such undulations, or series of undulations, will present the exact counterpart, direct or reversed, of the thread of a screw.

At a very great distance from the centre of disturbance the curved front of the wave will become sensibly plane. If a solid obstruction be placed in the way of such a plane wave, having in it a small aperture through which a minute elementary portion of the movement may be propagated, while all the remaining part is arrested, then this aperture will become the centre of a new spherical wave on the other side of the obstruction. If the solid be perforated by a great number of apertures, each of these will generate its own independent wave; and all these waves, as, in their progress, they encounter each other and become blended, will ultimately reproduce anew the plane wave from which they originated. By supposing the number of these apertures infinitely great, and the spaces between them infinitely small, we shall arrive at the conclusion that *a plane wave is equivalent to an infinite number of spherical waves, whose centres are infinitely near to each other in that plane*, and of which the plane wave is the resultant; so that if, at any time, we intercept any number of these component waves, either such as are contiguous to each other, or such as are separated by determinate intervals, the consequences of the proceeding may be calculated *a priori* by finding the resultant of those which remain unobstructed. This principle, which is of the highest importance to the physical theory of undulation as applied to optics, was first laid down by Huyghens. We shall presently see how it was applied by him to explain the phenomena of the reflection and refraction of light.

§ 3. REFLECTION AND REFRACTION.

If light is to be regarded as an effect of undulation, the elastic fluid in which its undulations are propagated must be inconceivably more rare than the air. This fluid must be supposed not only to fill all space, but also to occupy all the intervals which separate the particles of ponderable bodies; as well of those which are opaque as of those which are transparent. It is furthermore necessary to suppose that within these bodies it is possessed of a density or of an elasticity different from that which belongs to it in free space; or that the molecules of the bodies themselves, in some manner, retard its movements among them. Assuming for the present the first of these suppositions to be the true one, it will follow that when a luminous wave encounters the surface of a ponderable medium, its velocity of progress must undergo a change. The nature of the change may be best understood by referring once more to the laws which govern the impact of elastic solids. If a non-elastic ball impinge upon another equal to it at rest in the line which joins their centres, it will divide with the latter its motion, and the two will go on together with half the original velocity of the first. But if the balls are elastic they will be in a state of *compression* at the instant in which the motion is equally divided, and the recoil from this compression, acting with a force equal to that of the impact which produced it, will destroy the remaining half of the velocity of the first, and communicate an equal addition to the velocity of the second, so that the latter will proceed with the entire velocity which belonged to the impinging ball, and this will remain at rest. But if the impinging ball be heavier or lighter than the other, the motion will not be equally divided at the moment when the impact brings them to a common velocity—that is to say, at the moment when, if non-elastic, they would begin to move together; and the recoil, which doubles the effect of impact, will destroy less or more than the entire motion of the first and will add an equal amount to the motion of the second. If less than the entire motion of the first is destroyed, that body will continue to advance; if more, it will move in a contrary direction, or rebound. If the second, at the moment of maximum compression, has received less than half the motion of the impinging body, (in which case it is the lighter of the two,) the joint velocity with which both tend to advance together will be greater than half the original velocity; and as this will be doubled for the body originally at rest, by the recoil, the second body will go on with a velocity greater than that of the impinging body. But if the second in the collision receives more than half the motion of the impinging body, (in which case it will be the heavier,) the joint velocity will be less than half the original velocity, and accordingly the final velocity of the second will not be so great as that with which the first encountered it.

Regarding the successive strata of the ether in a luminous wave as elastic bodies, it will follow that so long as they are equal in mass (which will be the case when the density is uniform) the passage of an undulation or tremor will leave all the ether behind it at rest. But if, at any given point, a stratum of greater mass (that is, an ether of different density) be encountered, there will be a movement of return as well as a movement of advance; that is to say, there will be a reflected undulation which, in the view we are now taking, will be an undulation by condensation. If the change of density be from denser to rarer, then the advance movement of the impinging tremor will not be entirely arrested, and there will be a reflected undulation by rarefaction. We have here supposed the impinging wave to be in that phase in which the molecules are moving in the same direction as the wave itself. But the consequences will be entirely analogous if we suppose it to be in the opposite phase: only that, on that supposition, the phases of the reflected waves will also be the reverse of what is stated above.

It appears, therefore, that at any surface at which the luminiferous ether undergoes a change of density, an impinging wave will, in general, be divided into two parts, one of which will be propagated beyond the surface, while the other will be reflected from it. It remains to be considered what should be the laws governing the directions in which these two waves will proceed.

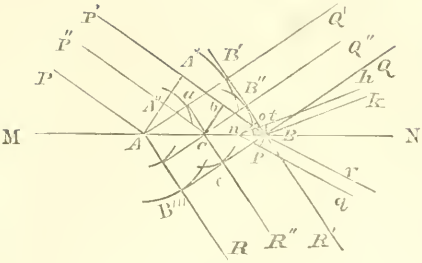

Fig. 33.

For this purpose let us assume a plane surface, as MN, bounding a transparent medium more dense than air. Let AA′ be the point of a plane wave advancing in the direction PA, P′A′, and passing from the air into the medium between the points A and B. We have seen that this wave will be partially reflected from AB. In order to determine the front of the reflected wave let us suppose, according to the principle of Huyghens, that every point of the surface of AB is the origin of a new spherical wave propagated in every direction from that point. It will be sufficient for the illustration to attend to a few only of these points, as, for instance, A, C, and B:—C being taken half way between the other two. By hypothesis AA′ is perpendicular to PA. Draw CA″ parallel to PA. The velocity of the reflected wave being equal to that of the incident, when the wave front AA′ reaches the position C*b*, the elementary reflected wave from A will have travelled to a distance equal to CA.″ When A reaches B the reflected wave from A will have reached a distance AB′ equal to A′B. Also, at the same instant, the elementary wave from C will have travelled a distance CB″ equal to *b*B=A′*b*. With A and C as centres describe the small circular arcs shown at B′ and B″, and from B draw BB′ tangent to the first of these arcs. It will also be tangent to the second. For if AB′Q′ be drawn from A through the point of contact B′, and CB″Q″ parallel to it, then, because AB is bisected in C, CB″ is half of AB′; and it is also perpendicular to B′B, consequently B″ is a point in the spherical wave front which originates from C. And as similar reasoning may be applied to all the elementary waves reflected from the points between A and B, it follows that the tangent BB′ is the resultant reflected wave front moving in the direction AQ′ or BQ. Also, from the similarity of the triangles AA′B and AB′B, it is evident that the inclination of the wave front AA′, or of the ray PA to the reflecting surface MN, is equal to that of the wave front BB′, or of the ray BQ to the same surface. The incident and reflected rays are therefore equally inclined also to the *normal* to the surface: or, in other words, the angle of reflection is equal to the angle of incidence.

If we turn our attention to that portion of the incident wave which is propagated into the medium beyond the surface, the construction which determines the refracted wave front is analogous to the foregoing. Only, in the second medium, as it is the denser of the two, the velocity of propagation will be diminished. Suppose it to be so in the ratio of *n* to 1, the circles described from A and C must have their radii, AB‴ and C*c* reduced below A′B, and *b*B in the same proportion.* The wave front of the refracted wave will then be the common tangent BB‴ of these circles. Also, if we observe that A′B = AB sin. A′AB, and AB‴ = AB sin. B‴ BA, we shall obtain the proportion,

$$\sin A'AB : \sin B'''BA :: A'B : AB''' :: n : 1.$$

But these angles, being the inclinations of the wave fronts to the refracting surface, are also the inclinations of the rays themselves to the *normal* to the

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* A diminished velocity might be a consequence of a change in the *time* of a vibration without a corresponding change in the undulation length; or it might be caused by a change of both. But, in the case in hand, we are not at liberty to assume any change in the time of the vibration, because the impulses which produce the molecular movements in the second medium are the vibrations themselves of the first. The undulations in the two media are, therefore, isochronous; and it is only possible to explain the difference of velocity of undulation progress by supposing a change of undulation length.

surface; that is, they are the angles of incidence and refraction. The law of Snellius is thus very simply deduced from the theory of undulation. It also appears from the foregoing proportion that the velocity before refraction is to the velocity after refraction as the sine of incidence is to the sine of refraction, or as the index of refraction is to unity.

It was not until after Huyghens had perfected his theory of refraction, that he became acquainted with the remarkable case of Iceland spar. In order to reconcile the phenomena presented by this crystal with his theory, it was necessary to suppose that the incident wave is divided at the refracting surface into *two* waves having unequal velocities. But, inasmuch as one of these waves must, on this hypothesis, and, in order to meet the phenomena, have a velocity greater in some directions than in others, it occurred to him that the second wave must probably be *spheroidal*, and not *spherical*. Following out this ingenious idea, he presently discovered that it contained a perfect explanation of all the apparent anomalies of double refraction; and by generalizing the method which has just been given for finding the direction of a ray after refraction, and extending it to embrace spheroidal as well as spherical wave surfaces, he contrived a geometrical construction by which the path of the extraordinary as well as of the ordinary ray may, in all cases, be exactly determined.

In order to understand this, let it be observed that the greatest and least axes of the spheroidal wave will evidently be proportioned to the greatest and least velocities of the ray in the crystal; and these velocities are inversely as the corresponding indexes of refraction. Also, as the least index, in the case of Iceland spar, is that which is found in the plane perpendicular to the optic axis, and as the refraction is there conformable to the law of Snellius, it is manifest that this is the plane of greatest velocity, and is the equatorial plane of the spheroid. The polar axis of the wave corresponds in direction, therefore, with the optic axis of the crystal. Let it also be remembered that the least velocity of the extraordinary ray is the constant velocity of the ordinary ray; or is, in other words, the radius of the spherical wave.

If, now, a plane wave of light fall obliquely upon the surface of a crystal of Iceland spar, intersecting it in a straight line, any point of this line of intersection may be assumed as the common centre of a spherical and a spheroidal wave, having one diameter in common, parallel to the optic axis of the crystal, which common diameter is the axis of revolution of the spheroid. Moreover, as this supposition may be made of every other imaginable point of the line of intersection, it follows that there will be an infinite number of elementary waves, spherical and spheroidal, simultaneously generated. As the incident wave advances, the line of intersection will advance along the surface of the crystal; and in every position of this line a new set of elementary waves will in like manner originate. By reasoning similar to that which was employed in the illustration of ordinary refraction, it may be shown that, in any position of the incident wave, all the elementary spherical waves will be touched by one and the same tangent plane; and all the elementary spheroidal waves will be touched by one plane likewise. These two tangent planes must intersect each other in the line in which the incident wave intersects, at the moment, the surface of the crystal, or the plane of that surface; but they will not coincide except in the single case in which the velocities of wave progress are equal; that is to say, when the movement of progress within the crystal is in the direction of the optic axis.

The geometrical problem of determining the path of the extraordinary ray reduces itself, therefore, to this. With the point of incidence as a centre describe a sphere. Upon that diameter of the sphere which coincides with the optic axis describe a spheroid of revolution, whose revolving axis is to its fixed axis as the greatest index of refraction is to the least. In the plane of incidence lay down the path of the ordinary ray according to the law of Snellius. Through the intersection of this path with the surface of the sphere pass a plane tangent to the sphere; and through the intersection of this tangent plane with the plane of the refracting surface pass another plane tangent to the spheroid. The radius of the spheroid drawn to the point of contact will be the path of the extraordinary ray.

The following is the construction usually given in this case. It has the advantage of involving only the angle of incidence. Let the velocity before refraction be represented by unity; and after refraction let the velocity of the ordinary ray be v, and that of the extraordinary ray perpendicular to the optic axis be v'. With the point of incidence as a centre let there be described a sphere and a spheroid, as before, the radius of the sphere being $= v$, and the revolving axis of the spheroid being $= v'$. On the plane of the refracting surface and in the plane of incidence take a distance (in the direction of progress) equal to the cosecant of the angle of incidence, and through the point so determined draw a perpendicular to the plane of incidence. The plane passing through this perpendicular and touching the spheroid, determines the direction of the extraordinary ray, which, as before, coincides with the radius to the point of contact.

To illustrate by a comparatively simple case. In figure 34, let MN be the surface of the crystal, and CD the direction of the optic axis. Let also the plane of incidence (represented by the plane of the figure) be a principal section. RC being the direction of the ray and C the point of incidence, make RC = unity, draw CG perpendicular to RC, and RG parallel to MN, cutting CG in G. Draw GQ parallel to RC. Then GQ being made radius, QC is the cosecant of QCG = PCR = the angle of incidence. Then if, with the centre C, a sphere be described whose radius is CD = v, and also a spheroid whose polar radius is CD also, and whose equatorial radius is made = v', the tangents QE, QF, drawn through Q to the sphere and spheroid respectively, will determine the directions of CE, CF, the ordinary and extraordinary rays.

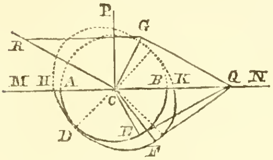

Fig. 34.

The hypothesis of Huyghens completely determines the geometrical law of double refraction, but it leaves the physical cause of the phenomenon unexplained. It is easy to understand how the disturbance at a single point of the molecules of an ether of uniform density and elasticity should produce a spherical wave; and it is also easy to comprehend how a similar disturbance in an ether of variable density or variable elasticity should produce a wave having a surface not spherical; but, as undulation was understood in the time of Huyghens, it was not easy to comprehend how waves of both these descriptions should be generated in the same ether simultaneously. We have already seen that Huyghens himself was very greatly astonished to observe that, when the two rays into which a single incident ray is divided by double refraction in a crystal of Iceland spar, fall, after emergence, upon a second similar crystal, they are, each of them, in some, and in fact, in most positions of this second crystal, again divided, so as, of the single original ray, to make four; while in other positions they are not so subdivided, but remain two only. But, in fact, this new phenomenon presents no really new cause of astonishment. The thing which ought to have surprised him, the point which involved, in truth, all the difficulty of the conception, lay in the actual coexistence of dissimilar waves in the same ether. No mode of explaining this fact could well have failed to explain the other at the same time.

It was only, however, after the entire inadequacy of any theory of light which had been suggested, to explain the multiplied and brilliant phenomena of polarization and double refraction had been universally felt and acknowledged, that an idea presented itself (and this simultaneously, or nearly so, to two distinguished physicists) which contained within itself the key to the original and to all succeeding difficulties. In undulation, as understood by Huyghens, Newton, and others, the molecular movements were supposed to be always in the direction of wave-progress and the contrary. Though this is the case with the atmospheric undulations which produce sound, it was ingeniously suggested by Fresnel and Young that it is probably not so with the ethereal undulations which produce light. Polarized rays are differently affected to reflecting surfaces on their different sides. Suppose the movements of the molecules to be *normal to the direction of progress*, and this fact is easily explained. And not only that, but a whole class of perplexing, and previously, in fact, entirely inexplicable phenomena, to which we shall have presently to attend, are, by this simple supposition, rendered, not merely intelligible, but so necessary consequences of the hypothesis, that they might have been predicted (as some of them actually were) before having been ever observed.

Though the idea of transverse vibrations occurred, as just remarked, to Young as well as to Fresnel, it is, nevertheless, to Fresnel alone that the credit is due of having made it the basis of a theory fully wrought out and established, as well upon the basis of a thorough mathematical analysis, as of an elaborate and extensive experimental verification. To apply it at present to explain the possible coexistence of two independent waves of different curvature and different progressive velocity, originating simultaneously from a common centre in a doubly refracting medium, we have but to make the two assumptions following, viz:

1. The molecular movements, being in both of the two waves at right angles to the direction of progress, are performed in planes which are at right angles to each other.

2. The elasticity of the ether which determines the velocity of a tremor is not the same in all directions within the medium.

It will presently be more particularly shown in what manner those two assumptions serve to explain all the phenomena which appeared so unaccountable to Huyghens, as well as many others which were not known to him.

§ IV. INTERFERENCE.

We will now proceed to apply briefly the theory of undulation to the explanation of some of the phenomena which we have heretofore detailed without accounting for. Many, or most of the phenomena depend on the mutual influence of different undulations conspiring or conflicting in consequence of the superposition of one upon the other. A gross illustration often employed in explaining this idea is to refer to the appearances presented by the intersecting rings formed in water into which two pebbles have been thrown. The elevated rings and their intervening depressions are undulations; the molecular movements are vertical, while the undulation progress is horizontal. When the rings intersect, the points where two ridges cross are doubly elevated; the points where two hollows cross are doubly depressed; while the points in which a ridge in one system crosses a hollow of the other are neither elevated nor depressed. The term applied to this influence of one undulation upon another is *interference.*

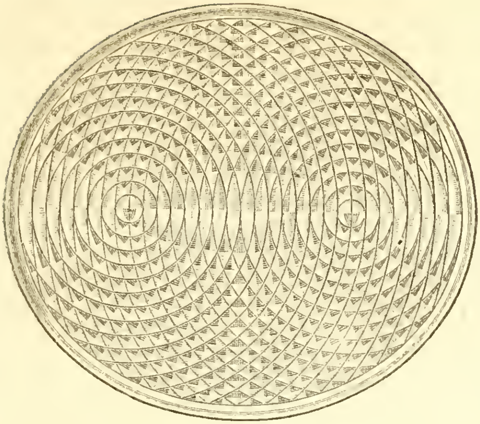

Fig. 35.

The interferences of liquid waves are finely illustrated in the undulations of mercury contained in a vessel of elliptical figure. If a disturbance be produced at one of the focal points of the ellipse, the circular waves proceeding from it will, by reflection from the sides of the vessel, form a second similar system having for its centre the other focus. If the corresponding points of inferference be connected, they will form, as the figure shows, two sets of curves, elliptical and hyperbolic, having for their common foci the foci of the original ellipse.

The interference of waves of sound is often very perceptible. It is observed only in musical sounds because it *can* only be observed in those whose undulations are continuous and uniform; and such sounds are musical. It is best observed when the waves are long—as in the case of the grave tones of the heavier organ-pipes. The sinking and swelling of the sound, called by musicians the *beat*, is owing to one of the interfering waves being slightly longer or shorter than the other. In many repetitions this slight difference of length accumulates until it reaches half an undulation, when, if the two waves originally conspired—that is, (to borrow again an illustration from the water,) if their two crests were originally superposed—they will, after this difference has crept in, be in conflict; or the crest of one will fall upon the hollow of the other. During this interval a sinking of the sound will have been observed; but immediately after, as the difference of path goes on increasing from a half to a whole undulation, the sound will swell again as the two crests once more approach superposition. We need hardly remark that the interference of waves of sound of *perfectly equal length* would not be perceptible to us; for, in that case, the resultant sound would be a *constant*. If we endeavor, by moving about while two bodies of precisely similar pitch are sounding, to pass from the points of conspiring to those of conflicting undulation, we shall not find it easy to detect these points for several reasons

In the first place, when the molecular movements are normal to the wave front, as in the case of sound, there is no *complete* interference, or approach to complete interference, except when the waves are tangential, or approximately so, to each other; except, therefore, in or near the line of the centres, and except, it may be added, when the distance between the centres is an exact number of half undulations. Again, at the *intersections* of sonorous waves, whether the molecular movements conspire or conflict, their resultant is never so great as the sum, nor so small as the difference of the two components. The difference of intensity between the maxima and minima of sound in such cases will not be striking, unless they succeed each other with brief intervening intervals of time, as in the case of the *beats*.*

It is, however, by this second method that we detect the interferences of light, and not at all by the first. That is to say, we discover these interferences by moving the eye through the space where they exist, when the points of maximum and minimum brightness are easily observed; or we let fall the interfering

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* Mr. Despretz has succeeded in this rather difficult experiment of *localizing* the interferences of sound from two pipes in perfect unison.

rays upon a white surface, when the same points will become manifest by their difference of illuminating power. The first method is best, especially if the eye be assisted by a lens, but the second is that which was used by the earliest observers.

We cannot detect the interferences of light by observing periodical maxima and minima, like the beats in music, because of the almost inconceivable shortness of the undulations. But if the waves of light were as long as the waves of sound, interferences might easily be made to manifest themselves, something in the manner of the scintillation of the stars, though with a regularity which that phenomenon does not possess.

Before proceeding now to a more particular inquiry into the laws of the interference of luminous waves, it is proper to make two or three preliminary explanations. The phenomena compel us to the assumption that the molecular movements in these waves are normal to the direction of progress; that is, to the direction of vision. In other words, they are in the plane of the wave itself, and at right angles to the ray. If we suppose that all ethereal tremors have this character, we must account for the fact by presuming that the ether is nearly incompressible. If this is the case, the vibrations of the luminous body, at its surface, may move *laterally* the whole stratum of ethereal particles which is most nearly in contact with it, though they produce little if any motion perpendicular to the surface.

If MN, for example, be the surface of the luminous body, A,A,A, &c., the row of ethereal particles next it, A′,A′,A′, &c., the row beyond this, and so on, the arrangement of these particles will, on the principle of equilibrium, be such that the distances of adjacent particles shall be equal. If the molecular vibrations of the surface MN are incapable of driving the particles A,A,A, &c., directly outward toward the plane of A′,A′,A′, &c., on account of the very difficult compressibility of the ether, they may, nevertheless, move them all sideways in the direction AB. Let the entire force of this movement be represented by AB. Join AA′, and draw BC perpendicular to it The force AB may be resolved into the two forces CB and AC, of which the second is directed toward the centre of A′. This again may be resolved into the two, AD and DC, of which the first is normal to MN, and, by hypothesis, produces no sensible movement. But DC is parallel to MN, and, as all the other particles in the stratum A′,A′, and are simultaneously acted upon by similar forces, they will all move in the direction of DC, without changing their distances from each other.

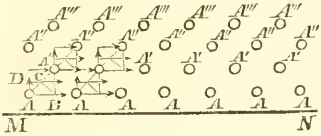

Fig. 36.

It is not necessary to suppose that the ether has absolutely no compressibility. In fact, if it had none at all, it could have no elasticity; or, what is, singularly enough, practically the same thing, its elasticity would be infinite. But its compressibility must be esteemed very slight, and its elasticity accordingly very high, not merely because of the necessity of admitting lateral or transverse vibrations, but because of the immense velocity of light. It is easy to see that, if the ether were totally incompressible, the velocity of light (if in such circumstances there could be any such thing as light) would be infinite; that is to say, any movement in the ether, if it could be produced at all, must be produced simultaneously through the whole extent of the ether. In proportion as compression is easy, the rapidity of the propagation of a disturbance (density remaining the same) must be less. The immense velocity of light affords, therefore, a strong ground for believing that the compressibility of the ether is very small. Still, it is hardly conceivable that there should exist absolutely no molecular movements normal to the wave at all; and, in fact, the existence of such vibrations is now generally admitted, though they are usually assumed to be incapable of impressing the organs of vision.

But while the molecular movements in luminous waves are assumed to be at right angles to the direction of progress, or of the ray, there exists no natural necessity to determine them in azimuth toward one direction rather than toward another. It is accordingly capable of easy demonstration that ordinary light has no determinate plane or azimuth of vibration, but that its successive undulations assume every variety of azimuth. There is no proof, however, that changes of azimuth are incessant; in other words, that many undulations, in fact, many thousands or perhaps millions, do not follow each other usually, in the same azimuth, between the changes. This, indeed, is probable, since the ethereal vibrations take their character from those of the luminous body, and these may reasonably be presumed to have a certain persistence in their modes of vibration, or at least not to undergo incessant and abrupt changes. Beyond a certain limit, however, this persistency could not continue; nor could there, among the changes which occur, be a predominating disposition to return to one azimuth oftener than to another, or to remain in it longer, without imparting to the light, more or less decidedly, the character of polarization. If five hundred millions of the mean undulations of white light were to follow each other in a single azimuth, they would occupy less than the millionth part of a second; and, accordingly, if five hundred millions of such undulations should take place in each of a million different azimuths successively, the whole would be performed in one second, and no instrumental test could detect polarization in the aggregate beam.

The polarization of light consists, therefore, in the determination of all its vibrations to a single plane. The effect of double refraction is to do this with both the rays into which the incident common light is divided; and the effect of reflection at certain definite angles, from certain bodies, as heretofore explained, is to do the same with the reflected ray.

Prof. Dove, of Berlin, has illustrated in a very ingenious manner the physical relation of common to polarized light. A Nicol's prism having been mounted in such a manner as to admit of being rapidly rotated about its axis, he transmitted through it a ray of common light, which gave, of course, an emergent polarized ray capable of traversing a crystal of Iceland spar (having its principal plane coincident with the plane of polarization) without double refraction. On setting the prism into rotation double refraction instantly appeared, and the ray was equally divided by the crystal in all azimuths.

When two polarized rays follow each other in the same path or intersect under a very acute angle, it is obvious that, if their planes of polarization agree in azimuth, they are in condition to interfere. If in phase of undulation they are perfectly accordant, the two waves will be superposed, and the molecular velocity of the resultant wave will be equal to the sum of the velocities of the two components; but if there is a difference of phase between them amounting to exactly half an undulation, then the crest of one wave will fall on the hollow of the other, and the resultant molecular velocity will be equal to the difference of velocities of the components. If the difference of phase is any other fraction of an undulation, the circumstances of the resultant are determined by precisely the same equation as that which has been given for the resultant motion of a vibrating solid, (equation [3],) in the same case. If a vibrating solid derive its motion from two impulses which are not synchronous, we have seen that its phases of vibration will be somewhere between those which the impulses would have separately produced. Its actual vibration will therefore produce an undulation or series of undulations, which will occupy the same situation in space relatively to those which the separate impulses would have produced, as the generating vibration occupies relatively to the component vibrations, in time. And it matters not to the result, whether we suppose two component systems of undulations to be first generated by independent vibrations and then combined; or suppose the two vibrations to be first combined, and then to generate a single resultant system of undulations—the resultant system is the same in both cases. On the first hypothesis, we allow two forces to work out their effects separately and then unite the effects; on the second, we unite the forces themselves, and make them unite their effects from the start.

Referring to the equation just cited, we see that the resultant molecular velocity, when the movements are in the same plane, takes every value according to the difference of phase of the components, from the sum to the difference of the two component velocities. Thus, if θ be put $= 0°$, the equation

$$A = \sqrt{a^2 + a'^2 + 2aa' \cos\theta}, \text{ will become } A = \sqrt{a^2 + a'^2 + 2aa'} = a + a',$$

$$\text{Or, if } a = a', \ A = 2a.$$

$$\text{If } \theta = 90°, \ A = \sqrt{a^2 + a'^2}; \text{ or, if } a = a', \ A = a\sqrt{2}.$$

$$\text{If } \theta = 180°, \ A = \sqrt{a^2 + a'^2 - 2aa'} = a - a'; \text{ or, if } a = a', \ A = 0.$$

$$\text{If } \theta = 60°, \ A = \sqrt{a^2 + a'^2 + aa'}; \text{ or, if } a = a', \ A = a\sqrt{3}.$$

$$\text{If } \theta = 120°, \ A = \sqrt{a^2 + a'^2 - aa'}; \text{ or, if } a = a', \ A = a.$$

It may aid in obtaining clear conceptions of this subject to employ a graphic illustration. Such notions are very desirable at this point of our progress, if we would understand the application of the theory of undulation to the explanation of optical phenomena; and especially of those of highest interest. In the annexed figure, let the two curves PHA, QMN, represent two undulations, whose molecular velocities are the ordinates drawn to the common axis, MNAC, and whose maxima velocities are PP′, QQ′. The undulation PHA is the more advanced in position; but, referred to any common intersecting line as LA, the undulation QMN is the more advanced in phase.

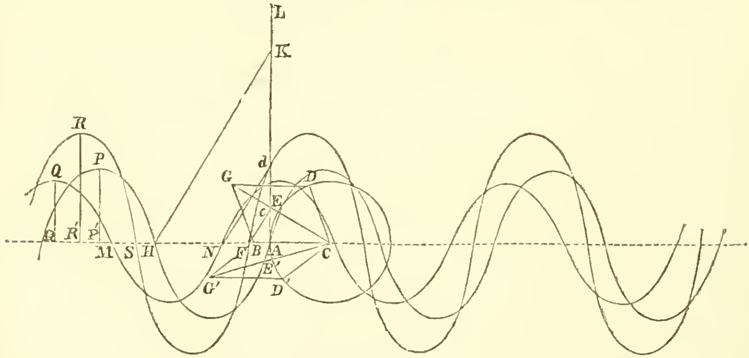

Fig 37.

Now we have seen that the resultant maximum molecular velocity, when these undulations are combined, will be the diagonal of a parallelogram, of which PP′ and QQ′ are the sides, and of which the angle of inclination of the adjacent sides shall be equal to the difference of phase between the components. Accordingly, from the point A, where the curve representing the undulation least advanced in phase crosses the axis, measure off AC, in the direction of progress, equal to QQ′, the maximum velocity of the wave most advanced in phase. From C, measure backward, CB, equal to PP′, the other component maximum velocity. From the centre C, with the radius CA, describe the circle AED, &c. At the point C lay off the angle DCA, equal to the difference of phase of the two components, thus:

Draw AL perpendicular to the axis at A. Upon this take AK, the development of the *semicircle* AED, &c. Join KH, and draw Nd parallel to it. Ad is the development of the arc AED, which measures the angle DCA, the difference of phase. For HA is to NA as a half undulation is to the difference of phase:—that is, as a *semicircumference* is to the *arc which measures the difference of phase.* But AK is a semicircumference, and Nd being parallel to KH, we have.

$$HA : NA :: KA : Ad : AC \times \pi : \text{arc } AED (= Ad.)$$

Join therefore CD, and complete the parallelogram CDGB, drawing the diagonal CG. Then from what has been before demonstrated, the angle ACE, or the arc AE, is the measure of the interval in phase between the resultant and the component CB = PP'. The curve of that component crosses the axis in A. Let, then, Ae be the development of AE, and draw eF parallel to KH. F is the point at which the curve of the resultant undulation will cross the axis in ascending. Making FS = AH, S is the point where the same curve crosses in descending. And making SR' = HP', R' is the point of maximum resultant velocity. Draw R'R perpendicular to the axis, and make it equal to GC, the diagonal of the parallelogram; R is the vertex of the resultant curve. Any other points of this curve may be found by taking the sums of the ordinates of the two components corresponding to the same absciss or point of the axis, with like signs when both components are above or both below the axis, and with unlike signs when one is above and the other below, for the ordinates of the resultant curve. The curve itself may then be drawn through the points so determined.

This construction enables us visibly to verify the analytical results which were just now presented. Let the radius, CD, revolve round the point C, the parallelogram changing its figure as the revolution advances, and the variations in the value of CG may easily be conceived.

Thus, when $\theta = DA = 0°$, the point D will fall upon CA, and the point G upon BH. CB and BG will then be in a straight line, and CG will equal $CB + BG = a + a'$. When $\theta = 90°$, DCA and GBC are right angles, and $CG = \sqrt{BC^2 + BG^2}$, or, $A = \sqrt{a^2 + a'^2}$. When $\theta = 180°$, CD falls on the axis to the right of C, and BG falls on BA. Hence, $CG = a - a'$. In this case, if $a = a'$, $A = 0$; or if equal waves differ in phase by half an undulation they destroy each other. The two curves intersect the axis in the same points, but the convexity of one of them corresponds in position to the concavity of the other. Also, if equal waves differ in phase in any manner, the crest of the resultant will fall half way between the crests of the two components.

When θ exceeds 180°, or the waves are more than half an undulation apart the angle of the parallelogram must still be measured from A through D round to D', and the inclination of the diagonal must be taken in the same way, from A through ED and D' round to E'. These arcs being developed on AL produced, will give the position of the movable component and of the resultant by drawing parallels to cut KH, as before. It will be seen in this case that, in effect, the wave which we have regarded as the *preceding* wave becomes the *following* wave, and *vice versa;* for if we consider the crests that nearest agree in position as forming *pairs,* these pairs will be broken up by a discordance of more than half an undulation, and new pairs will be formed—the lagging crests ceasing to agree with those crests of the other component which are *before* them, and beginning to agree with those *behind* them. Hence, as the resultant crest must fall *between the two components of a pair,* it in this case goes further back than the wave which our construction makes the *following* wave. This construction, therefore, embraces all possible cases.

But if the rays which are thus brought together are polarized in planes at right angles to each other, then it will be manifest that the movements of neither can interfere with those of the other; but, as in the case of the vibrating solid again, they may produce a resultant of which the character may vary from a plane vibration through every form of ellipse to a circle. Equation [1] expresses the circumstances of this case.

Thus, if, in that equation, we assume the difference of phase represented by θ to be 90°, $\cos\theta$ becomes $=0$, $\sin\theta=1$, and the equation is

$$a'^2y^2 + a^2x^2 = a^2a'^2,$$

which is the equation of the ellipse when the axes of figure coincide with the axes of co-ordinates. If we make $a = a'$, then we have

$$y^2 + x^2 = a^2,$$

which is the equation of the circle.

If the difference of phase is 0°, then

$$a'^2y^2 - 2aa'xy + a^2x^2 = 0, \text{ or } a'y - ax = 0,$$

which is the equation of a straight line; and if a once more be put $= a'$, $x = y$. or the straight line makes equal angles with the directions of the original molecular movements.

If the planes of the two undulations are neither normal to each other, nor coincident, there will be an interference which will be more or less complete, as the inclination of the planes is less.

Rays of common light, if the difference of their paths be not very great, will interfere, notwithstanding the fact that their undulations are confined to no determinate azimuth. This fact proves, what has been above assumed, that the changes of azimuth in common light cannot be incessant. But there is one condition absolutely indispensable to produce interference in any case; it is that the rays shall have a common origin.

If the light subjected to experiment be unpolarized, the necessity of the condition is easily explained. The changes of the azimuth of vibration in two such rays could not, except upon a supposition which has an infinity of chances against it, take place at the same intervals and in the same order; and if they did, the chances would be equally great against the coincidence of those planes. It appears, however, to be true, as well of *polarized* rays as of common light, that they will not interfere unless from the same origin. We are obliged, therefore, to resort to the supposition, which has *a priori*, moreover, strong probability in its favor, that there are irregularities at the very origin of the undulations, or at the surface of the luminous body, which are propagated with the undulations, and which will prevent the permanent coincidence or conflict of two sets of undulations, unless both are equally affected by the same irregularities. Thus, if we observe the flame of a candle, we shall see that its wavering motion will make the *point of departure* of the undulations it generates unsteady. But a difference of a single one hundred-thousandth part of an inch in the position of the origin of two successive sets of undulations, would put them into entirely opposite phases. Considering the activity and the energy of the forces at work at the surfaces of incandescent bodies, it is impossible to believe that the luminous waves which they generate can have their origins absolutely invariable in position.

These things premised, we are prepared to apply the theory of undulation to the explanation of all the phenomena of diffraction, polarization, and the colors of thin or thick plates, in regard to which we have heretofore stated only the facts. It is worth while, however, in the first place, to give a moment's attention to an experiment first suggested by Fresnel upon purely theoretic grounds, and afterward made by him with complete success; in which the circumstances preclude the application of any of the special hypotheses which had been previously conceived, for the purpose of accounting for the phenomena, while the observed effects are consequences absolutely necessary of the undulatory theory.

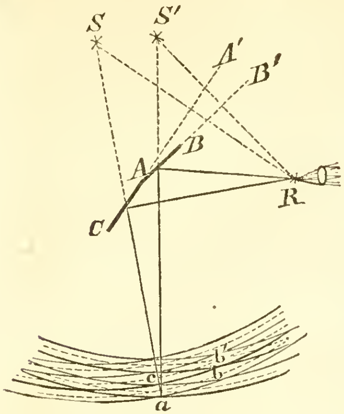

Fig. 38.

Two mirrors, AB and AC, meet at a very obtuse angle at A. R is a minute radiant point. The best radiant for this purpose is the concentrated light from a small solar beam introduced into a dark room and brought to a focus by a lens of small focal distance. Rays of light from R, reflected by the mirrors, proceed as from the points S and S′, which are in the perpendiculars let fall from R on the planes of the mirrors severally, and as far behind them as R itself is before. If the mirrors AB and AC were in one and the same plane, the points S and S′ would coincide, or there would be but one image of R formed by the reflection; and the two images will be nearer to each other, just in proportion as the angle CAB approaches to two right angles. The spherical waves proceeding from R as a centre would be reflected from a single mirror in a system entirely similar to the original one, proceeding apparently from the image behind the mirror. When there are two mirrors and two such images, there will be two such systems of spherical waves, which will intersect each other in arcs more and more nearly coincident, as the images which are their centres are nearer together. In the figure there are drawn, with equal radii, from S and S′, a succession of equidistant circular arcs, which may be considered to represent the crests of the successive waves. The intermediate dotted arcs may be taken for the hollows. According to the principles already laid down there should be increased energy of movement—that is an increase of brightness—wherever two ridges or two hollows cross each other; and diminished movement, or a diminution of light where a hollow crosses a ridge. And as it is obvious, on inspection, that the intersections will be more widely separated from each other in proportion as the centres are nearer to each other, it follows that these theoretically predicted effects will be more conspicuous and more decided in proportion as the planes of the two mirrors approach coincidence. If the light reflected from such a pair of mirrors be received upon a screen, it will be obvious that, whatever be the distance from S and S′, there will be a point, as *a*, where the radii S*a* and S′*a* will be equal; and as this will be true of all points in a line through *a* parallel to the intersection of the planes of the mirrors, there should here be a coincidence of movements, and accordingly a bright stripe. At a little distance on each side of this stripe there will be found parallel lines, in which the radius from one of the centres will exceed that from the other by the length of one entire undulation; and in these lines the movements will be once more in coincidence, and the light will again be in excess. But between them and the central line there will be found other positions in which the radii will differ by only half an undulation; and, as the movements in these positions will be directly opposed to each other, the light should disappear. Extending this reasoning, we should look for a series of stripes alternately dark and bright on each side of the central bright stripe, at distances sensibly equal to each other. These conclusions are fully confirmed by experiment.*

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* Prof. Potter, of London, affirms, in opposition to all other experimenters, that the central stripe in this experiment is often seen dark instead of light; and in fact more usually so than otherwise.—[*London Journal of Science*, XVI, 1840. Also, *Physical Optics*, London, 1856.] Prof. Baden Powell, on the other hand, states that he has endeavored to verify this assertion with every possible attention to the conditions prescribed by Prof. Potter, but entirely without success.—[*A General and Elementary View of the Undulatory Theory*, &c., London, 1841.]

By means of a movable eyepiece, provided with a micrometrical apparatus, Fresnel accurately measured the distances of these stripes from each other, and thus deduced the lengths of the undulations by which they are produced. In fact, as the *locus* of the central bright stripe is in the line of intersections, of which a is one, and that of the adjacent bright stripe is in the line of intersections, of which b is one, we have a small triangle abc, whose sides are severally perpendicular to those of the triangle $a\mathrm{SS}'$; and accordingly,

$$a\mathrm{S} : \mathrm{SS}' :: bc : ac; \text{ or } ac = \frac{bc.\mathrm{SS}'}{a\mathrm{S}}. \qquad [7.]$$

But ac is the length of the undulation, whence it appears that this length is equal to the distance between two adjacent similar stripes multiplied by the distance between the two radiant centres, and divided by the distance of either centre from the screen. As the radiants in this experiment are merely virtual and not actual, the values of SS′ and aS cannot be conveniently measured. But it may be observed that the fraction

$$\frac{\mathrm{SS}'}{a\mathrm{S}} = 2\sin \tfrac{1}{2}\mathrm{S}a\mathrm{S}' = 2\sin \mathrm{BAA}'.$$

$$\text{Hence, } ac = bc.2\sin \tfrac{1}{2}\mathrm{S}a\mathrm{S}' = bc.2\sin \mathrm{BAA}'. \qquad [8.]$$

The angle SaS′ may be directly measured by an instrument placed at a, or the angle BAA′, which is the inclination of the mirrors, may be otherwise determined.

Putting λ for the length of the undulation, φ for the angle $\frac{1}{2}\mathrm{S}a\mathrm{S}'$, and δ for the distance between the stripes, the foregoing equation gives

$$\lambda = 2\sin\varphi \times \delta; \text{ or } \delta = \frac{\lambda}{2\sin\varphi}; \qquad [9.]$$

Whence it appears that the distance between the stripes will be greater as φ is less, or as the radiant centres are nearer together. The same process applied to the distance from the middle stripe to the second one on either side will give—

$$\delta' = \frac{2\lambda}{2\sin\varphi}: \text{ and for the third, } \delta'' = \frac{3\lambda}{2\sin\varphi}, \text{ \&c.}$$

So that the successive stripes are equidistant from each other.

Grimaldi's case of diffraction, in which the radiant centres were two minute apertures very near to each other through which light was introduced into a dark room, was manifestly analogous in principle to this. To that case the first of the formulæ just given may be directly applied.

The mirrors in the experiment of Fresnel require very careful adjustment. If, at the edges where they meet, one or the other projects, however slightly, the effect will be sensibly impaired. A prism of glass having two adjacent faces very slightly inclined to each other might be used to produce the interferences by total reflection from these inclined surfaces, without being subject to this objection. The other faces of the prism would require to be so adjusted that the light might enter and emerge through them sensibly at right angles. The disadvantage would be that the angle of the reflecting faces would be invariable. The experiment admits of being performed, and has been performed, by the help of a single mirror, placed almost but not quite parallel to the original rays, so as to cause a portion of the wave very slightly to deviate, and thus to interfere with the portion which is not reflected. In this case it is obvious that the system of fringes produced can embrace only one-half of those which are seen in the experiment of Fresnel.

In place of Fresnel's mirrors Mr. Arago employed a glass prism to produce interference by refraction instead of by reflection. Arago's prism has a cross section of the form of a very obtuse angled isosceles triangle; the light being received in the experiment perpendicularly upon the base, and emerging at the obtuse vertex in two interfering waves. The effects correspond in all respects with those produced by Fresnel's mirrors.

Mr. Arago also introduced a modification of the experiment, which, though simple, is very interesting in the bearing upon theory, of its results. In the path of one of the interfering rays he interposed a thin lamina of mica. As mica is transparent, it was to be expected that fringes would continue to appear after the interposition as well as before; and this expectation is realized. But as the undulation length cannot be the same in the mica as in the air, since the refracting power of mica exceeds that of air, it was also to be expected that the fringes would change their place; and this expectation also is fulfilled. The direction of the displacement will depend upon the question, which of the two waves, after the lamina is interposed, will be found, when they reach the position of the originally luminous central stripe, to be advanced beyond the other in its phase of undulation. This will of course be true of that which has the least average length of undulation. If the undulations in mica are of less length than in the air, (a necessary supposition, as we have already seen,) the average length of undulation on the side of the mica will be less than that on the other side; and accordingly the phase at the central line of meeting will be most advanced on the side of the mica. We must therefore assume a line upon the screen parallel to the central line, such that the length of path from the radiant on the side of the mica shall be as much less than the length of path from the other radiant to the same line, as the thickness of the lamina of mica is less than that of a lamina of air embracing the same number of undulations would be, in order to find the position of the bright stripe which is central in the displaced system. The whole system is of course moved toward the side of the mica.

If homogeneous light be employed in the experiment with Fresnel's mirrors or Arago's prism, equation [7.] or [8.] furnishes the means of measuring the undulation lengths in different parts of the spectrum. In the following table are embraced the results of such a measurement, made by Fraunhofer and expressed in decimals of an inch, for fourteen different positions determined by their relations to the colors or to the fixed lines of the spectrum. The undulation-lengths in this table are taken from Fraunhofer: the numbers per second are computed on the supposition of a velocity of light of 192,700 miles to the second.

Undulation-lengths and numbers per second.

Place in spectrum.	Length of undulations in parts of inch.	Number of undulations in an inch.	Number of undulations per second.
Line B	.00002708	36,918	451,000,000,000,000
Line C	.00002583	38,719	473,000,000,000,000
Middle red	.00002441	40,949	500,000,000,000,000
Line D	.00002319	43,123	527,000,000,000,000
Middle orange	.00002295	43,567	532,000,000,000,000
Middle yellow	.00002172	46,034	562,000,000,000,000
Line E	.00002072	48,286	590,000,000,000,000
Middle green	.00002016	49,609	606,000,000,000,000
Line F	.00001906	52,479	641,000,000,000,000
Middle blue	.00001870	53,472	653,000,000,000,000
Middle indigo	.00001768	56,569	691,000,000,000,000
Line G	.00001689	59,205	723,000,000,000,000
Middle violet	.00001665	60,044	733,000,000,000,000
Line H	.00001547	64,631	789,000,000,000,000

If we compare the numbers of undulations per second in the foregoing table with the numbers per second of acoustic undulations corresponding to a given pitch, we shall observe that, if these vibrations had power to affect the sense of hearing, the middle yellow would produce a tone forty-one octaves above the fundamental C, or C between the staves; and the middle red would be forty octaves above the "Stuttgard pitch," or normal A, taken at 440 complete vibrations. The entire interval covered by the visible spectrum would be about a major fifth; between line B and line H, or the part easily visible, a major fourth.

§ V. DIFFRACTION.

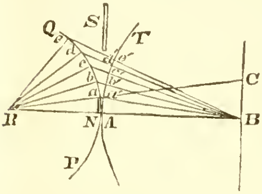

Fig. 39.

We are now prepared to understand the causes which produce the stripes or fringes observed by Grimaldi, bordering the shadows of opaque bodies introduced into a divergent pencil of light from a minute radiant point. Let R, Fig. 39, be the radiant centre, and PQ the spherical wave front, at any determinate distance from R, as RA. In this case, as in the former, and generally in all analogous experiments, the best radiant for the purpose is obtained by concentrating a small solar beam, (introduced into a dark room,) by means of a lens of short focus. Suppose an opaque screen S to be advanced to A, so as to intercept the half wave AQ. The light which reaches the point B, in the line RA produced, will be the resultant effect of the unobstructed half wave PA. Let AQ be divided, at *a*, *b*, *c*, &c., into parts, such that the lines B*a*, B*b*, B*c*, &c., drawn from the point B on a screen BC, to the points of division, may successively exceed each other by the length of one-half an undulation; or such that, drawing the arc AT with B as a centre, the intercepts aa', bb', cc', &c., may have the successive values $\frac{1}{2}\lambda$, λ, $\frac{3}{2}\lambda$, &c. Now, if the screen S be drawn upward from A to *a*, the light which reaches B will be the resultant effect of the half wave PA, combined with the resultant effect of the small additional wave surface A*a*. This latter resultant will be compounded of the molecular movements produced at B by the infinite number of minute elementary waves, which may be supposed to originate from all the points of the given wave front between A and *a*. Since all these elementary waves originate simultaneously, their relative phases, when they reach B, will depend on the differences in the lengths of their paths; and as these differences are the intercepts between the arcs AQ and AT, there will be none, until we reach *a*, which will differ from the wave proceeding from A by so much as half an undulation. Assuming, then, that their several intensities are equal, there will be no complete conflict between any of the elementary waves within these limits; and accordingly their resultant effect must be positive, or must add to the intensity of the light at B. If, however, we raise the screen S higher, the intercepts will begin to exceed half the length of an undulation, and some of the elementary waves originating just beyond *a* will neutralize the effect of some of those near A. Raising it to *b*, there will be a complete series of waves originating between *a* and *b*, which will be in absolute conflict with the series which originate between A and *a*; so that, if A*a* and *ab* were exactly equal, and their separate intensities, as above supposed, equal also, their resultant effect at B would be zero. A*a* is, however, a little larger than *ab*, both because of the inclination of B*a* to BA, and because of the curvature of AQ. The intensities of the elementary derivative waves are also presumed to be greater in the direction of the radius of the original wave than in directions inclined to it, though the law of such variation of intensity is not known. These causes of difference will, nevertheless, exist to no very marked degree in the immediate vicinity of the line RB, and consequently the total effect at B of the portion of wave front A*b* will be sensibly null. If, now, the screen S be further raised to *c*, the elementary waves originating between *b* and *c* will be in complete conflict with those between *a* and *b*. Thus the power of *ab* to interfere with A*a* will be nearly neutralized, and the point B will receive, once more, nearly all the illumination which A*a* is capable of sending to it. And in like manner, if the screen be successively raised to the points *d*, *e*, &c., similar alternations of diminished and increased brightness may be inferred. After passing the fifth or seventh division, however, these successive maxima and minima cease to be perceptible in white light: a consequence partly due to the unequal lengths of the undulations of the different colors, and partly to the diminishing length and increasing obliquity of the successive divisions of the wave front.

Dr. Lloyd has illustrated this case in the following felicitous manner. Let the light received at B from the half wave PA be represented by 1, and that from the total unobstructed wave by 2. Represent the effect of A*a* by $+m$, that of *ab* by $-m'$, that of *bc* by $+m''$, and so on. Then we have,

$$2=1+m-m'+m''-m''', \text{ \&c.}$$

Now as each of the successive literal terms is greater than that which follows it, if we cut the series at any point the value of all the terms which succeed on the right will have the same sign as the first of them; and the sum of the remaining terms on the left will be less than 2 if the value cut off is positive, and greater than 2 if the value cut off is negative.

Should these popular illustrations of a somewhat difficult subject appear unsatisfactory, it may be observed that analysis leads to the same results, although the processes are complicated. Without going into details, we may remark that the intercept *aa'* is evidently a function of the angle AR*a*. Put h for the intercept, ω for the angle, and V for the resultant molecular velocity at B. Then, if all the derivative waves begin simultaneously in the arc AQ, the component molecular velocity at B, due to any elementary wave will be expressed by $v\sin 2\pi\frac{h}{\lambda}.d\omega$; v being the maximum molecular velocity of the derivative wave, and λ the length of an undulation. Hence—

$$\frac{dV}{d\omega}=v\sin 2\pi\frac{h}{\lambda}=v\sin 2\pi\frac{f(\omega)}{\lambda},$$

since h is a function of ω which may be represented by $f(\omega)$.

If $f'(\omega)$ be the differential coefficient of $f(\omega)$, we shall have—

$$f'(\omega)dV=v\sin 2\pi\frac{f(\omega)}{\lambda}.f'(\omega).d\omega.$$

$$\text{And } \int f'(\omega)dV=V.F(\omega)=-\frac{v\lambda}{2\pi}\cos 2\pi\frac{f(\omega)}{\lambda}+C.$$

$$\text{Or } V.F(\omega)=\frac{v\lambda}{2\pi}\left(1-\cos 2\pi\frac{f(\omega)}{\lambda}\right).$$

As $f(\omega)$, which is the intercept, is always increasing with ω, and its differential coefficient also, this expression makes it evident that the value of V must pass through a series of maxima and minima, since the expression $1-\cos 2\pi\frac{f(\omega)}{\lambda}$ undulates between the values 0 and 2. These maxima and minima become, moreover, less marked as ω increases, since $F(\omega)$, whatever it may be, must increase also.

This illustration, however, excludes an important consideration, which is, that, owing to the constantly increasing obliquity of the arc ω to the direction of the intercept, the value of $\frac{dV}{d\omega}$ as above given should be multiplied by the cosine of the sum of the angles AR*a* and AB*a*; which sum is a function of ω

and of the intercept itself. When ω has any considerable magnitude, this factor rapidly reduces the value of $\frac{dV}{d\omega}$; indicating a similarly rapid diminution in the fluctuations of value of the integral. On the other hand, when ω is very small, this factor may be regarded as a constant, and assumed, without sensible error, as equal to unity.

The distance of these fringes from the boundary of the shadow may be determined as follows. Suppose the screen S to be at a, and let the straight line RaC be the boundary of the geometrical shadow. Draw aN perpendicular to RA. Call aN, y, and AN, x. By construction, $\mathrm{B}a - \mathrm{BA} = \frac{1}{2}\lambda$. Put $\mathrm{RA} = r$, $\mathrm{BA} = s$, and $\mathrm{B}a = q$. Then,

$$q - s = h = \sqrt{(s+x)^2 + y^2} - s = s + x + \frac{y^2}{2(s+x)} - s = x + \frac{y^2}{2s} = \tfrac{1}{2}\lambda\,;$$

disregarding inappreciable terms of the root, and omitting x in the denominator where its effect on the value of the fraction is insensible. Also, in the circle whose centre is R, x is the versed sine of Aa, and is sensibly equal to $\frac{y^2}{2r}$. Whence—

$$h = \frac{y^2}{2r} + \frac{y^2}{2s} = \frac{(r+s)y^2}{2rs} = \tfrac{1}{2}\lambda\,; \quad \text{or } y^2 = \frac{rs\lambda}{r+s},$$

And putting δ, as before, for the distance BC, we have,

$$y^2 = \frac{r^2\delta^2}{(r+s)^2} = \frac{rs\lambda}{r+s}\,; \quad \text{and finally } \delta = \sqrt{\frac{(rs+s^2)\lambda}{r}}. \qquad [10.]$$

From this expression, which is the equation of an hyperbola, it appears that, if the screen B move toward A, the *locus* of all the points in space corresponding to B will be a hyperbolic curve, of which R and a are the vertices. A similar inference may be drawn from considering that, in all positions of the screen B, $\mathrm{B}a - \mathrm{BA}$ is constant and equal to $\frac{1}{2}\lambda$; or $\mathrm{BA} - \mathrm{B}a = -\frac{1}{2}\lambda$, whence $\mathrm{RA} + \mathrm{BA} - \mathrm{B}a$, or $\mathrm{RB} - \mathrm{B}a = \mathrm{RA} - \frac{1}{2}\lambda$, which is also constant. But this is the property of a hyperbola whose *foci* (not the vertices) are R and a, and whose major axis is $r - \frac{1}{2}\lambda$. This latter result is the strictly correct one. The discrepancy between it and the former is owing to the omission of minute terms in obtaining that result. Put the major axis equal A, and the minor axis equal B. Then, by the law of the hyperbola,

$$\mathrm{B}^2 = r^2 - \mathrm{A}^2 = r^2 - (r - \tfrac{1}{2}\lambda)^2 = r\lambda - \tfrac{1}{4}\lambda^2\,; \quad \text{or } \mathrm{B} = \sqrt{r\lambda},$$

disregarding the minute negative term.

The equation of the curve, if we employ the exact values of the axes, will be—

$$\delta^2 = \frac{r\lambda - \frac{1}{4}\lambda^2}{(r - \frac{1}{2}\lambda)^2}\Big((r - \tfrac{1}{2}\lambda)s + s^2\Big), \qquad [11.]$$

which, when the minute terms are dismissed, simplifies itself to the expression found before.

The semi-axis major of this curve being $\frac{1}{2}r - \frac{1}{4}\lambda$, it appears that the curve itself passes behind the obstructing edge at the distance of $\frac{1}{4}\lambda$, at which distance a wave reflected from the obstacle would meet a wave advancing with a difference of $\frac{1}{2}\lambda$. Whether this theoretic indication is actually realized, it would perhaps be difficult experimentally to determine. Such a reflected wave, considering only the difference of path, would be out of harmony with the advancing wave; but, considering that its molecular movements would be reversed by reflection from the dense ether of the obstacle, the harmony would be restored.

For the distance of the second fringe from the shadow, the expression already found for the first answers perfectly, if we prefix to the quantity under the radical the coefficient 3; since the difference of paths Bc—BA, by which it is produced, is equal to $\frac{3}{2}\lambda$, instead of $\frac{1}{2}\lambda$. For the third we prefix 5. Thus the successive distances are—

$$\delta=\sqrt{\frac{(rs+s^2)\lambda}{r}}\,;\quad \delta'=\sqrt{\frac{3(rs+s^2)\lambda}{r}}\,;\quad \delta''=\sqrt{\frac{5(rs+s^2)\lambda}{r}}. \qquad [12.]\ [13.]$$

The second hyperbola passes at the distance of $\frac{3}{4}\lambda$ behind the obstructing edge, and the third at the distance of $\frac{5}{4}\lambda$. Owing to the great disproportion between the axes, $r-\frac{1}{2}\lambda$, and $\sqrt{r\lambda-\frac{1}{4}\lambda^2}$, which are very nearly in the ratio of $\sqrt{r} : \sqrt{\lambda}$, the curvature is very slight except for a short distance from the vertex; so that the branches in fact sensibly coincide with their asymptotes. But near the vertex the curvature is very decided.

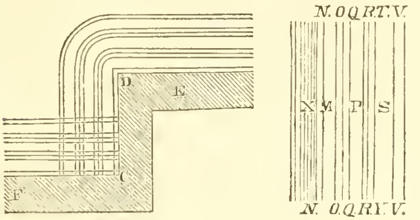

Fig. 40.

From what has been said. it will be evident that when the obstruction presents a salient angle instead of a straight edge, the fringes will pass round the angle in circular arcs, instead of making an angle also. Indeed, the systems of fringes around such an angle are surfaces of hyperboloids of revolution. In the case of re-entering angles of 90° or less, the fringes cross each other without interference, and are continued up to the edge of the shadow on each side of the angle. These are easily seen to be necessary consequences of the theory of their formation. The annexed figure exhibits the phenomena.

When the obstructing body is large, no fringes are seen within the shadow. Some light strays beyond the geometrical boundary of the shadow, but it rapidly fades away, and produces no very sensible effects. If, however, a very narrow object be employed, the waves from opposite sides may mingle and interfere. In this case fringes or stripes will be seen *within the shadow*. The light concerned in producing these fringes comes from the portion of the main wave which is close to the obstruction, as the more distant parts of each half wave will hold each other in check in the manner heretofore explained. Points being taken at a, b, c. a', b', c', &c., such that the successive differences of distance of these points from B may be $\frac{1}{2}\lambda$, we shall have, on each side, a series of resultant actions alternately positive and negative, as illustrated in the foregoing case, such as—

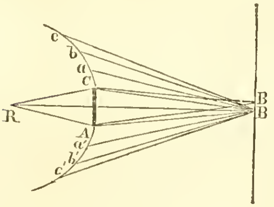

Fig. 41.

$$+m-m'+m''-m'''+m'''', \text{ \&c.}$$

And as the effect of any portion of this series, left by cutting off terms from the beginning, depends on the sign of the first of the remaining terms—that is to say, as the effect due to the first of these terms exceeds the joint effect of all that follow—it is evident that no part of the main wave can have anything to do with producing fringes in the shadow, except Ca and Aa'. And since there can be no fringes produced at all, unless the light from both sides reaches the same point, the centre of the shadow, which is equidistant from the edges of the obstacle—that is, from the front of the main wave—must exhibit a light stripe. On each side of this will be found positions, as for instance B′, where the distances from A and C differ by half an undulation; and here the darkness will be complete. At distances a little greater will be found positions where the differences of distance from A and C amount to an entire undulation; and here will be found once more bright stripes. If the object be very narrow, these interferences may occur not only throughout all the shadow, but to some distance into the light on each side. Should any evidence seem to be needed to confirm the theory on which the formation of these fringes has been explained, it may be found in the fact that if, by an interposed card, the light from one side of the opaque object be arrested, all the fringes will instantly disappear from the shadow.

As for the form of the *loci* of these fringes in space, since each is determined by the intersection of radii from A and C, having a constant difference, they are necessarily hyperbolas, having A and C for their foci. But in this case it is the principal axis which is small, while the conjugate axis is comparatively very great; so that the curves are widely open, having but slight curvature even at their vertices.

Let $B'A - B'C = n \times \frac{1}{2}\lambda$, n being any integral number, even or odd. It is evident, from the law of the hyperbola, that $\frac{1}{2}nl$ is the principal axis of the trajectory of B'. And, putting $CA = c$, the conjugate axis will be $\sqrt{c^2 - \frac{1}{4}n^2\lambda^2}$. Making this the axis of y, and the former the axis of x, the equation of the curve gives us—

$$y^2 = \frac{c^2 - \frac{1}{4}n^2\lambda^2}{\frac{1}{4}n^2\lambda^2} \cdot \left(\frac{n\lambda x}{2} + x^2\right).$$

Suppressing the minute term $\frac{1}{4}n^2\lambda^2$ from the numerator, and reducing the equation with respect to x, we obtain—

$$x + \tfrac{1}{4}n\lambda = \delta = \sqrt{\frac{(4y^2 + c^2)n^2\lambda^2}{16c^2}}. \qquad [14.]$$

According to the notation heretofore used, y, which is the distance of B from the object AC, may be replaced by s. Also c^2, in the numerator under the radical, may be dropped without appreciable error, except when B is quite near to the object. The simplified expression will then be—

$$\delta = \frac{sn\lambda}{2c}, \text{ which is the equation of a straight line.} \qquad [15.]$$

At any considerable distance from AC, therefore, as compared with AC, the hyperbolic trajectory sensibly coincides with the asymptote to the curve. In fact, the equation of the asymptote being—

$$x' = \frac{A}{B}y' = \frac{\frac{1}{2}n\lambda}{\sqrt{c^2 - \frac{1}{4}n^2\lambda^2}} . y', \qquad [16.]$$

by rejecting the minute term under the radical, we obtain—

$$x' = \delta = \frac{n\lambda y'}{2c} = \frac{sn\lambda}{2c}; \text{ which is identical with the former.}$$

By substituting different numerical values for n, this equation serves for all the fringes, light or dark. The even numbers give the loci of the bright stripes, and the odd those of the dark. The distance δ is in all cases measured from the middle of the central bright stripe.

The expression for the value of δ indicates, at sight, that the fringes will increase in breadth, as the opaque intercepting object diminishes in diameter. In fact, δ is inversely as c, and to double the breadth of the fringes, we have only to reduce the diameter of the object one-half. Accordingly, if a tapering object, as a sewing needle, be employed, the fringes will spread out toward the top with a beautiful plumose appearance. This becomes still more striking when the taper is more rapid, as when we use an acute-angled or even a right-angled plate of thin metal. The fringes, which in this case are very remarkable, have been called *Grimaldi's crests.*

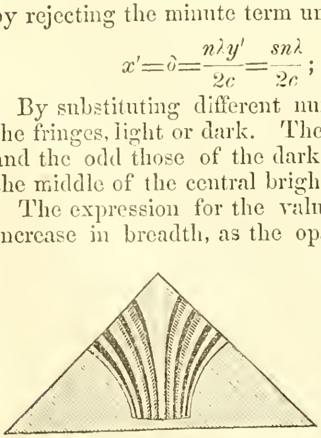

Fig. 42

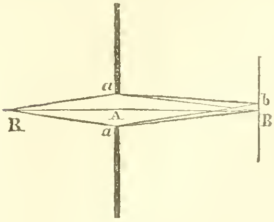

Fig. 43.

The next case which presents itself is that in which a small portion of the wave only is allowed to pass through a narrow opening in the obstructing screen, having straight and parallel sides. In this case a position may be found for the screen B, in which, if RAB (Fig. 43) be drawn from the radiant through the centre of the aperture, B*a* and B*a′*, drawn from B to the edges, may exceed BA by one-half an undulation. All lines drawn from B to points of the wave front nearer to A than *a* or *a′*, will differ from BA less than half an undulation, and the point B will be fully enlightened. If then the screen B be advanced toward A, there will be found somewhere another position in which B*a* and B*a′* will exceed BA by an entire undulation. The spaces A*a*, A*a′*, may then be divided somewhere, so that lines drawn from the points of division to B shall once more differ from BA by half an undulation. All the molecular movements excited at B by the segments next to A will then be in conflict with those which are generated by the segments next to *a* and *a′*; and accordingly in this position of B the middle of the luminous image will be occupied by a dark stripe. By advancing B still nearer, another point may be found, where B*a* and B*a′* will differ from BA by three halves of an undulation; and in this case the arcs A*a* and A*a′* may be divided each into three parts, such that the distances of the points of division from B may successively exceed each other by half an undulation. The pair next to *a* and the pair next to *a′* will then neutralize each other, while the central pair will be efficient, and the point B will be again illuminated. Thus, by varying the distance of B from *a*, the dark stripe in the centre of the luminous image will alternately appear and disappear. It is obvious, however, that when the distance is found at which B*a*—BA is exactly one-half an undulation, the dark stripe will not return at any greater distance. As the screen B approaches A, on the other hand, the entire bright image becomes filled up with fringes, increasing in number, with the central one alternately dark and bright. It is also sufficiently remarkable and striking that if, when B is at the maximum distance producing a dark centre, a very narrow opaque object be placed over the aperture, parallel to its edges, so as to intercept exactly one-half the light, leaving equal portions on each side of it to pass, the brightness of the centre will instantly return. It will disappear again when the opaque object is removed. When B is at other positions nearer to A, producing the dark centre, the restoration of central brightness will not necessarily take place on cutting out the central half of the beam; but it may be effected by cutting out a portion which is somewhat more or less than half.

In order to understand the conditions upon which this difference depends, we must consider that the dark stripe appears in the centre only when $Ba - BA$ is equal to an *even* number of half undulations. But even numbers of two kinds, the *even*-even, and the *odd*-even. The *even*-even are all of them *multiples of 2 by the arithmetical series of even numbers* 2, 4, 6, &c.; the *odd*-even are *multiples of 2 by the odd numbers* 1, 3, 5, &c. If, then, $Ba - BA = n \times \frac{1}{2}\lambda$, the light will be restored to the central dark stripe by stopping out the middle half of the beam, whenever n is an *odd*-even number; and the interposed opaque body must exceed or fall short of half the breadth of the beam by the breadth of two, at least, of the divisions of the wave front, ($2n$ in all,) into which the space aa' is supposed, in the foregoing explanation, to be divided, in order to restore the brightness when n is an *even*-even number. We here assume the several divisions of the wave front to be equal in extent, which is sufficiently exact for the purpose in view. The other dark stripes which form within the bright image of the opening aa' are subject to fluctuations of intensity similar to those of the central one. To understand this, let b be a point so taken, that $ba'-ba=\lambda$. Join bR, and let the line ba revolve round Rb to the position bc. Then $bc=ba$, and $ba'-bc=\lambda$. Divide ca' at d into parts, such that $ba'-bd=\frac{1}{2}\lambda$, and $bd-bc=\frac{1}{2}\lambda$. Then, so far as this portion of the wave is concerned, the point b will be obscure in every position of b which preserves this relation, whatever be the distance from A. Also, at any distance of b for which the divisions of the wave front ac, made as heretofore described, are an even number on each side of Rb, the whole effect of the wave at b will be null, and the point b will be obscure. But if the number of these divisions on each side of Rb be an odd number, there will be a portion of the wave unneutralized, and b will be illuminated.

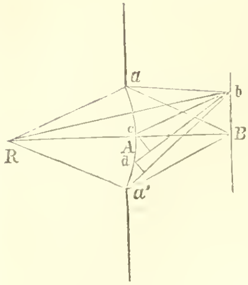

Fig. 44.

The fringes exterior to the bright image of the opening are more beautiful than those interior to it, being, especially when the aperture is very narrow, richly colored. They are not subject to the fluctuations of brightness, as the distance of B from the aperture varies, which attend the interior fringes; since the lines ba and ba', drawn to any point in any of those fringes from a and a', the limits of the aperture, will be both on the same side of Rb.

The distances δ from the central line B are all determined by the same equation which was found for the fringes formed by a narrow opaque object. Indeed, the geometrical conditions in the present case are identically the same as those in that. The optical difference is, that the even values of n give the *loci* of the dark stripes, and the odd those of the bright. The breadths vary, as before, inversely as c, which is the diameter or width of the aperture. With apparatus in which the opposed edges are movable, the expansion of the fringes, as these edges are made gradually to approach each other, is very striking. When the aperture is a very slender isosceles triangle, they spread out widely toward the vertex. The expression,

$$\delta=\frac{sn\lambda}{2c},$$

also shows that the breadth varies directly as the length of the undulation. In homogeneous light, therefore, the broadest fringes are obtained with red, and the narrowest with violet. In such light, a dozen or twenty may easily be counted. When white light is employed, the overlapping of the colors, while it improves the beauty of the display, reduces very much the number that can be distinguished. When monochromatic light cannot conveniently be obtained, the same effects may be substantially produced by viewing the fringes made by white light through colored glasses.

When, instead of a long and narrow aperture, a small circular opening in an opaque plate is used, the fringes are, of course, circular. In this case, the central dark stripe of the preceding experiment becomes a central dark round spot. This spot disappears and reappears as the screen is brought nearer the plate, at the same distances at which this effect was observed in the central stripe in the image of the oblong aperture. Referring to the last figure, and regarding aa' as the diameter of the circular opening, when $Ba-BA=2\times\frac{1}{2}\lambda$, there will be some point between a and A (suppose a'') which, if joined to B, will give $Ba''-BA=\frac{1}{2}\lambda$. Now, it has been shown that $Ba-BA$ varies as y^2; the radius of the aperture (or of the part of it considered) being represented by y. Hence, for the point supposed, a'', we have $Aa^2=A2a''^2$; or the circle of which Aa is the radius, is double in area of the circle of which Aa'' is the radius. But since $Ba-Ba''=\frac{1}{2}\lambda$, and $Ba''-BA=\frac{1}{2}\lambda$, it is obvious that the resultant molecular movement produced at B by the circle of which Aa'' is the radius, will be in total conflict with that produced at the same point by the portion of wave front which forms the ring between this circle and the circumference of the orifice. It is this conflict which produces the dark spot at B. If now a small opaque disk could be introduced into the middle of the orifice, exactly equal to the circle Aa'', stopping out the central pencil of light, B would immediately become bright again.

If $Ba-BA=4\times\frac{1}{2}\lambda$, the circular aperture will be made up of a central circle and *three* concentric rings, of equal areas, producing movements at B alternately equal and opposite. B will accordingly be obscure. If we stop out now one-half the area in the middle—that is to say, the central circle and the first ring—B will still be obscure; but if we stop out the central circle and the *two* interior rings, the light at B will be restored. Or if we stop the central circle only, or, instead of that, the exterior ring, or (which is the same thing) apply over the aperture a smaller one, having only three-fourths the area of the first—in either case the light will be restored. But if we stop the central circle and the outer ring *at the same time*, B will remain obscure.

Generally, if $Ba-BA=n\times\frac{1}{2}\lambda$, n having any integral *even* value, the centre of the bright image of the aperture will be dark. If n be *odd*-even, stopping out one-half the area from the middle of the aperture will restore the light. If n be *even*-even, stopping out one-half the area will produce no change; but the light may be restored by stopping a portion of the area which is by a certain amount greater, or by the same amount less, than one-half. In all these cases the light at the centre, when restored, will be sensibly equal in intensity to that which would reach B through an orifice of the size which would give $Ba''-BA=\frac{1}{2}\lambda$.

This incidentally leads us to the remarkable result that if, in this experiment, instead of a circular aperture in an opaque plate, we employ an *opaque disk* attached to a *transparent plate*, the centre of the shadow will be as highly illuminated as it would be if the wave were not interrupted at all. For an open circle whose centre and circumference give the relation $Ba''-BA=\frac{1}{2}\lambda$, and a ring whose exterior and interior circumferences give $Ba-Ba''=\frac{1}{2}\lambda$, produce sensibly the same illumination at B. In either case all the remaining obstructed portion of the wave exterior to them may be divided into rings, whose relation to the unobstructed part will be alternately negative and positive, and whose total resultant (which takes the sign of the first term) will be opposed to that of the unobstructed portion. If then this exterior portion be allowed to pass, the effect, in either case equally, will be somewhat to diminish the intensity of the brightness at B, which brightness therefore will still remain equal for the circle and for the ring. But in the first instance, this is to allow the *entire wave* to pass; while in the second it leaves the disk. The centre of the shadow of the disk, therefore, which is the point B, is as much illuminated as the same point is when the wave is wholly unobstructed. This curious circumstance, which was first announced by Poisson from theoretic considerations, is easily verified by experiment.

When it is said that an open circle which gives at its centre and its circumference the relation $Ba''-BA=n\times\frac{1}{2}\lambda$, or a ring of which the outer and inner circumferences furnish a similar relation, will exhibit a dark spot at B whenever n is an integral even number, it must be remembered that this proposition is true only of the rays whose undulation length is λ. If λ is the undulation length of the red rays, and λ' that of the blue or violet, then at the distance at which red disappears, the blue or violet will not be entirely suppressed. We have seen that $\mathrm{B}a''-\mathrm{BA}$ has a value expressed by the formula $h=\frac{(r+s)}{2rs}y^2$, y being the radius of the circle. This may be resolved into the parts,

$$\frac{y^2}{2r}+\frac{y^2}{2s},$$

of which the first is constant when y is constant, and the second varies inversely as s, which is the distance BA. The less the value of h $(=n\times\frac{1}{2}\lambda)$, the greater will be the distance at which the color corresponding to λ will be suppressed. And as the color which remains is the difference between the color suppressed and white, it follows that, as the eye approaches A, in the line BA, the ring or the aperture will assume successively all the tints of the spectrum from red upward, and that this series may be several times repeated. Moreover, putting λ for the length of the red, and λ' for the mean length of undulation in the compound color complementary to red, which will correspond nearly to the wave length in the green, when $\mathrm{B}a''-\mathrm{BA}=(2n+1)\times\frac{1}{2}\lambda'<(2n+1)\times\frac{1}{2}\lambda$, a point b may be found on any side of A, but very near it, at which $ba''-b\mathrm{A}=(2n+1)\times\frac{1}{2}\lambda$. A green circle will therefore appear surrounding A, while A itself, whether it be an aperture or a ring, will be red. Also, at other distances, greater or less, circles of other tints will appear; so that the ring or aperture will be encircled by a corona displaying all the prismatic colors, from red to violet, shading outward.

As the eye approaches A, the equation $ba''-b\mathrm{A}=(2n+1)\times\frac{1}{2}\lambda$ will be true of points nearer and nearer to B, until b and B coincide. The rings will therefore appear to be successively absorbed into the aperture. In withdrawing the eye, they will seem to be, in like manner, evolved out of it. In this experiment the aperture should be very small or the ring very narrow, in order that the tints may be vivid.

It will readily be understood that the obscurity and the sharp edges of shadows of bodies of considerable size are owing to the smallness of the values of λ for all the rays of light. On this account, if any point be taken within the line of the geometrical shadow, and if the wave front, beginning at the edge of the opaque body, be divided into portions whose extremes are remote from that point by distances differing $\frac{1}{2}\lambda$, these portions will neutralize each other's effects, except for positions of the assumed point for which the divisions have (as they may near the shadowing body) some slight inequality, and no material obliquity. Such positions can only be found very near the line of the geometrical shadow.

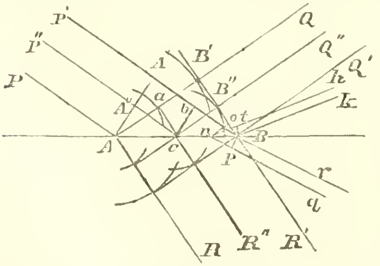

Fig. 33.

To the same cause it is owing that, in refraction and reflection, the beam refracted or reflected is as sharply defined as the incident beam. The demonstration which we have given of these effects, from Huyghens, contains an imperfection on this point, which Fresnel has supplied. Referring to the figure, suppose that an undulation originating at B should take the direction Bk, different from that of the main reflected wave, BQ'. There will always be found, to the left of B, a point, as n, from which another undulation will follow in the parallel and nearly coincident line nh, differing from the first by half an undulation. Draw no perpendicular to BB' and Bt perpendicular to nh; onB is the angle of incidence$=\iota$. Put ρ for nBt. Then $\mathrm{B}o=\mathrm{B}n\sin\iota$, and $nt=\mathrm{B}n.\sin\rho$.

Now, when the wave whose front is Bt starts from n, the movement which is to produce the wave from B is at o. There will accordingly be interference, if $nt-\mathrm{B}o=\frac{1}{2}\lambda$; that is, if $\mathrm{B}n.(\sin\rho-\sin\iota)=\frac{1}{2}\lambda$. But since λ is very small, if $\sin\rho$ sensibly exceed $\sin\iota$, Bn will be very small; showing that interference will take place from a point very near B. As $\sin\rho$ approaches $\sin\iota$, the distance of the origin of the interfering wave will be greater; but there will *be* an interfering wave, (if the surface AB is unlimited,) in every case except that in which $\sin\rho=\sin\iota$; that is, in which Bk coincides in direction with the regularly reflected wave.

In like manner, in the case of refraction, if we suppose a wave to diverge in the direction Br, draw nq parallel to Br and Bp perpendicular to it. Call the angle nBp, ρ, as before. Then $Bo=Bn.\sin\iota$, and $np=Bn.\sin\rho$. But np being the path of a wave in the denser medium, it must be multiplied by the index of refraction, in order to obtain the equivalent distance, or *distance which the wave would have moved in the same time*, in the rarer. Let n be the index of refraction, and we have, for the condition of interference, $Bn.(n\sin\rho-\sin\iota)=\frac{1}{2}\lambda$. If $n\sin\rho$ is sensibly greater than $\sin\iota$, Bn must be very small. And for any value of $n\sin\rho-\sin\iota$, there will be a distance Bn furnishing a wave of interference, if the surface AB is unlimited; except only for the value $n\sin\rho-\sin\iota=0$, when the ray Br ceases to diverge from the direction of the main refracted wave.

These reasonings assume that the forces of the elementary derivative waves are the same in all directions. But it is probable that these forces are less in lines *oblique* to the direction of progress of the primitive wave than *in* that direction. How far this is true could be easily investigated experimentally, by employing apertures less than the length of a half undulation in diameter, were it not that the extreme minuteness of such apertures (the mean length of a half undulation not exceeding one one-hundred-thousandth of an inch) would render the light too feeble for the purpose.

Some material for the formation of an opinion on this subject may, however, be gathered from certain phenomena of diffraction first observed by Fraunhofer, more remarkable and more brilliant than any which have been thus far mentioned. If a single very minute aperture will not furnish light enough for experiment, an assemblage of very many very minute apertures, closely grouped, may do so; and if these be so arranged that, for any determinate point in the shadow, they shall allow only such portions of the wave front to pass as *conspire* in their effects at that point, while the intervals between them obstruct those portions which conflict, we shall possibly find that the tendency of a wave originating in a single molecular impulse to expand equally in all directions, is much more decided than had been supposed. Fraunhofer's original experiments were made with *gratings* formed by stretching an exceedingly fine wire across two parallel screws of a great number of threads to the inch—the threads serving to keep the wires equidistant. He subsequently employed gratings formed by cementing leaves of gold to glass and cutting them through in very fine parallel lines ruled with a sharp instrument. Instead of these, also, he employed similar lines ruled with a diamond on glass itself.

The results of such an arrangement may easily be predicted. The image of an aperture closed by such a grating will appear bright, as though the obstruction were not interposed. But toward either side, in the direction perpendicular to the lines of the grating, will be found several points for which the part of the wave which the grating obstructs would if allowed to pass be more or less in conflict with those which it transmits; and which, therefore, are bright when the grating is present, and dark when it is absent. Suppose, for simplicity, that the open spaces and the opaque bars are equal in breadth. Let a, a, a, represent several of these open spaces, and b, b, b, &c., the intermediate bars. A point, P, may be found from which lines being drawn as in the figure, and perpendiculars let fall upon them from the edges of the apertures, as at c, c, d, d, will give $cd=\frac{1}{2}\lambda$, $db=\frac{1}{2}\lambda$, and therefore $cb=\lambda$. The distances from P to the corresponding parts of the several openings will thus differ by an entire undulation, and hence the waves which reach P through them will be in harmony.

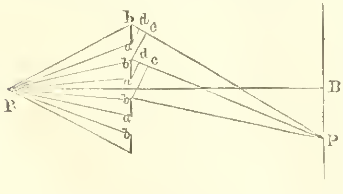

Fig 45.

The distances from P to the corresponding parts of the obstructing bars will differ from the distances to the adjacent openings by half an undulation; and, accordingly, if the bars were removed, the wave which would proceed from those points would neutralize the effects of the former: but being obstructed, P remains illuminated by the resultant effect of all the first set of waves.

Furthermore, since the position of P is determined by the condition that cb shall be the length of an undulation, it will be necessary to take P further from B for the longer undulations and nearer for the shorter. The different colors will thus be separated, and a perfect spectrum will be formed on the screen.

Should the point P be taken so that cb is equal to two undulations, there will be no spectrum: for in this case cd will be equal to one undulation, and as in the cases we have considered of a single aperture, one-half of each opening, a, will hold in check the other half. If we find still another point where bc is equal to three undulations, then cd will equal one undulation and a half; two-thirds of each opening will then be neutralized, but the remaining third will be effective; and there will be another spectrum, but less brilliant than the first. If $bc =$ four undulations, the spectrum will again fail. If $bc = 5\lambda$ it will return, and so on.

If the bars are broader than the open spaces, there will be a spectrum for $bc = n\lambda$, n being any integral number; until the light is too feeble, or until $cd = n'\lambda$, n' being also any integral number. If the spaces are broader than the bars, there will be a spectrum for every integral value of n in $bc = n\lambda$ until $n = \frac{a+b}{b}$, (a and b standing for the breadths of the spaces and bars severally.)

If, however, $\frac{a+b}{b}$ is not integral, take $q =$ the greatest common measure of a and b. Then $n = \frac{a+b}{q}$ will give the number of the first spectrum which will fail. Put this value of n equal to m, and we may say generally that the mth spectrum will fail, and also the nmth, n being, as before, any integral number. If a and b are incommensurable, there could be theoretically no perfect spectra, or spectra of maximum brilliancy; nor would any spectrum absolutely fail: but a near approach to failure would occur for approximate values of q. All these propositions result so obviously from the construction above given, that they require no demonstration.

The same construction indicates a simple expression for the deviation of each spectrum from the direction RB, of the radius of the original wave. For representing this deviation by δ, we have—

$$\sin\delta = \frac{n\lambda}{a+b}. \qquad [17.]$$

Putting $n = 1$, $\lambda =$ one fifty-thousandth of an inch, the length of the mean undulation, and supposing one thousand opaque lines to the inch, the formula gives us, by substitution, $\sin\delta = 0.02 = \sin 1° 9'$. As the sines of small angles are very nearly proportional to the angles themselves, the deviations of the succeeding spectra will be nearly the double, triple, &c., of this. And as the denominator, $a+b$, is the reciprocal of the number of lines to the unit of measurement to which λ has been referred—in this case to the inch—it is evident that the sines of the deviations will increase directly as this number. With five thousand lines to the inch, the fifth spectrum will have a deviation of thirty degrees. The force of the derivative waves from minute apertures thus appears to be great even at large obliquities, when the obstructing effects of interference are removed.

In the above expression for $\sin\delta$, if n be put equal to 1, and $a+b$ equal to λ,

$\sin\delta$ is unity, indicating a deviation of 90°. A grating, therefore, in which the number of lines to the inch is equal to the number of undulations in the same space, will produce no spectra. The same is true, *a fortiori*, of still finer gratings

The spectra formed in this way by diffraction will easily be understood to form the best of all possible measures of the lengths of the undulations corresponding to the different colors. They exhibit very distinctly the principal lines of Fraunhofer; and these lines, as might be inferred as a theoretical necessity, preserve invariably the same relative distances from each other. The spectra formed by refraction afford measures, not of the relative lengths of the undulations in vacuo, but of those lengths as modified by the media of which the refracting bodies are composed. Apparently these modifications are not simply proportional to the lengths of the undulations. Mr. Cauchy's investigations upon dispersion show, as we have seen, that they ought not to be.

Light *reflected* from finely ruled surfaces exhibits colors, as well as that which is transmitted through them. These effects are produced by interference, and are explained upon principles analogous to those we have been considering. Some substances are naturally marked with sinuosities which produce these effects. A familiar example of this kind is seen in mother of pearl. Sir David Brewster found that an impression of the polished surface of this material taken in wax, exhibited the same colors as the substance itself.

The effects produced by diffraction may be endlessly varied, by employing (instead of gratings) reticulations, and groups of apertures, of various figures, symmetrically disposed. Many of the phenomena are exceedingly rich and beautiful. We must content ourselves with the examples which have been given, and which illustrate the general principles on which they all depend.

§ VI. COLORS OF THIN PLATES.

We will now proceed, very briefly, to apply the theory of undulation to the explanation of the colors seen in thin transparent plates; or, as they are commonly called, Newton's rings. These, when seen by reflected light, are caused by the interference of the wave which proceeds from the lower surface of the plate with that which is reflected by the upper. Let us suppose, at first, for simplicity, that the light employed is homogeneous. Where the dark rings occur, there must be a difference of path between the interfering waves, of one-half an undulation. Now the wave which is reflected from the lower surface, passes through the thin plate twice; and that which is reflected from the upper surface does not enter the plate. The difference of path is therefore twice the thickness of the plate; and this ought apparently to be equal to half an undulation, or to some uneven multiple of half an undulation. Let θ represent the thickness, and n any integral number; then—

$$2\theta=(2n+1)\times\tfrac{1}{2}\lambda: \text{ and when } n=0,\ 2\theta=\tfrac{1}{2}\lambda. \text{ or } \theta=\tfrac{1}{4}\lambda.$$

It should seem, accordingly, that the first dark ring should appear, where the thickness is equal to one-quarter of the length of an undulation. As the thickness increases toward $\theta=\frac{1}{2}\lambda$, or diminishes toward $\theta=0$, the light should gradually appear; and when either of these values is reached, we should have the maximum of brightness. The centre of the system should then be bright. It is not so, however, but on the other hand is entirely dark. The reason of this apparent discordance with theory will be understood, when we recall the circumstance, thus far disregarded, that the reflection at the lower surface takes place as the ray is proceeding from a rarer to a denser medium; while that at the first surface occurs as the ray is passing from a denser and to a rarer. It has been already shown that, in the latter of these cases, the molecular movements maintain their original directions; while in the former, these movements are reversed. But to reverse the molecular movements of a wave is to change its phase half an undulation. Accordingly, at the points where $\theta=\frac{1}{4}\lambda$, and where the *difference of path* is $\frac{1}{2}\lambda$, the difference of phase is $\frac{1}{2}\lambda+\frac{1}{2}\lambda=\lambda$. This thickness should accordingly give a bright ring, and not a dark one; and so it is in fact observed to do.

If there could be any hesitation about receiving this explanation of the phenomenon, it may be entirely removed by considering the following two experiments. Mr. Babinet having produced, by means of Fresnel's mirrors, the fringes of interference already described, received the interfering pencils upon a glass mirror, of which one half was transparent and the other half silvered on the back. The reflected pencils, thrown upon a screen, still exhibited the fringes. When both the pencils were reflected from the silvered part of the mirror, or both from the transparent part, the fringe in the middle continued to be bright, as in Fresnel's original experiment. But when one of the pencils was reflected from the transparent glass and the other from the metal, the middle fringe became immediately dark. The other experiment alluded to consists in introducing between the two lenses, in Newton's experiment, a fluid having a refracting power intermediate between that of the upper and that of the lower glass. With a crown glass above, having the index 1.5, and a flint glass beneath, with the index 1.575, the oil of sassafras (index 1.53) or that of cloves (index 1.539) introduced between will convert the dark rings into bright ones, and *vice versa*. In this case the rays, at both surfaces alike, are passing from a rarer to a denser medium.

When the rings are viewed by oblique light the undulatory theory requires that their apparent magnitudes should be governed by the following law. If MM′ be the upper of two glass plates, with parallel surfaces, enclosing a lamina of air between them, and if IPQSTVR be the path of a ray incident obliquely at P, and reflected at the lower surface of the lamina at the point S, this will fall in at T with another, reflected from the upper surface at T, whose path is NOTVR. When the first is at P the other is at N, in PN, drawn from P perpendicularly to NO. These two have the same length of path in the medium MM′. Their difference of path will therefore be QS+ST−NO, or 2ST—NO. As the angle NPO is equal to the angle of incidence (which put $=\iota$) and also KST, we shall have—

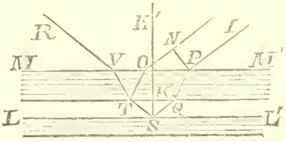

Fig 46.

$$2\mathrm{ST}=\frac{2\theta}{\cos\iota}, \text{ and } \mathrm{NO}=\mathrm{OP}\sin\iota=\mathrm{QT}\sin\iota=2\theta\tan\iota\sin\iota=2\theta\frac{\sin^2\iota}{\cos\iota}.$$

$$\text{Hence } 2\mathrm{ST}-\mathrm{NO}=2\theta\left(\frac{1-\sin^2\iota}{\cos\iota}\right)=2\theta\cos\iota.$$

But in order that there may be interference, this difference of path must be a multiple of half an undulation. Hence—

$$2\theta\cos\iota=n\times\tfrac{1}{2}\lambda, \text{ or } \theta=n\frac{\frac{1}{4}\lambda}{\cos\iota}=n\times\tfrac{1}{4}\lambda.\sec\iota. \qquad [18.]$$

In which n is an odd number for the bright rings and an even number for the dark. At oblique incidences, therefore, the thickness at which a given ring appears is greater than at a perpendicular incidence, in the ratio of the secant of incidence to unity, or in the inverse ratio of the cosine of incidence to unity. But this is the law which observation had established before the theory of undulation had indicated its necessity.

There is still one point to be attended to before the theory of the phenomenon is complete. The dark rings, as seen by reflection in homogeneous light, are *absolutely* dark, showing that the interference is total. But the amount of light in the two conflicting rays ought to be equal in order to produce this effect. Now, if we assume (what will hereafter be proved) that the amount of light reflected at either surface is in a constant ratio to the amount of light incident upon it, when the angle of incidence and the index of refraction are themselves constant, we shall perceive that the ray which emerges after one reflection at the lower surface is feebler than that which is reflected at the upper: for the light incident upon the lower is already enfeebled by the loss at the upper, and the reflected ray is again diminished by the second reflection which occurs at its emergence through the upper. But the light which is thus turned back at the upper surface is again reflected at the lower, and at its return another portion emerges through the upper. A series of reflections thus goes on between the two surfaces, each one contributing to strengthen the emergent ray; and the resultant of all these contributions is to bring the ray from the lower surface, in the end, up to exact equality with that which is originally reflected from the upper without entering the lamina. This will appear to be rigidly true if we consider the following statement. The *intensity* of light is measured by the living force which animates the mass of ether in which the molecular movements are going on. Let the masses in the two adjacent strata of the two media which act upon each other be distinguished by the letters m for the denser and m' for the rarer. Now it is true (as will hereafter be proved) that the velocity of molecular movement in a wave reflected from the separating surface of two given media, at a given incidence, bears a determinate ratio to the molecular velocity in the incident wave. Let this be represented by the ratio $r : 1$, the incident velocity being unity. Then the living force of the reflected wave will be mr^2, and that which passes into the other medium and forms the transmitted wave will be $m(1-r^2)$. Accordingly $\frac{m}{m'}(1-r^2)$ is the square of the molecular velocity in the transmitted wave. Let it be represented by u^2.

By reflection at the second surface, u becomes ru, and this, by another reflection of the returning wave at the first surface, becomes r^2u. From the living force of the wave returning from the second surface subtract the living force which it loses by the second reflection at the first, and the remainder, which is the living force transmitted through the first surface, will be $m'r^2u^2(1-r^2)$. And this, divided by the mass m, gives $\frac{m'}{m}r^2u^2(1-r^2)$ for the square of the first component of the molecular velocity in the wave which reaches the eye from the second surface. In like manner r^2u becomes r^3u by second reflection at the second surface; and r^3u becomes r^4u by the succeeding reflection at the first surface. And the expression for the square of the second component of the molecular velocity we are seeking, will be $\frac{m'}{m}r^6u^2(1-r^2)$. The next term will be $\frac{m'}{m}r^{10}u^2(1-r^2)$; and from a comparison of these three the law is evident. By substituting the value of u^2, taking the square roots of these squares and making their sum, which is the resultant molecular velocity of the wave emergent from the second surface, equal to r', we shall obtain—

$$r'=(1-r^2)(r+r^3+r^5+r^7+r^9 \ldots + r^{\infty}).$$

The sum of the series in parentheses is $\frac{-r}{r^2-1}$. Hence $r'=r$, or the reflected velocities, and consequently the intensities, of the waves reflected from the two surfaces, are equal.

It is assumed in the foregoing that all these components agree in phase. But this is evidently true at the points where the *dark* rings, as seen by reflection, appear. For at these points the first return wave from the lower surface is in conflict with the advancing wave, which it meets at the first surface. This advancing wave does not change its phase by refraction, but the *reflected part* of the return wave does so, and is therefore in harmony with the advancing wave which it joins. The two accordingly conspire from that time forward; their emergent portions at the second surface producing a bright ring by transmitted light, while their reflected portions, returning to the first surface, conflict with the *next* advancing wave which they meet there.

But at the points where the *bright* reflected rings appear, the case is different. The return wave is in harmony with the advancing wave which it meets at the first surface, and its emergent part conspires with the reflected part of the advancing wave. But its *reflected part*, losing half an undulation, *conflicts* with the transmitted part of the advancing wave, and thus produces, by subsequent transmission through the second surface, a ring partially obscure, but not entirely so, from the great inequality of the conflicting molecular velocities. If we disregard the successive advancing waves, and consider the successive values of the terms vu, v^3u, v^5u, &c., at these points, proceeding from a single original wave, we shall find them alternately positive and negative. Their emergent parts must be so likewise. And since they are decreasing, their sum takes the sign of the first term, which is positive; so that their resultant conspires with the wave reflected to the eye from the first surface. The components, simultaneously reflected to the second surface from the first, form a similar series with signs reversed, and therefore have a negative resultant, conflicting with the wave emergent at that surface.*

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* This matter may perhaps be made more clear as follows:

Calling, as above, u the value of the molecular movement in the ray transmitted through the first surface, and v the ratio of reflected to incident light, the advancing and returning waves within the lamina will have the successive molecular velocities—

1. Advancing wave, u, v^2u, v^4u, v^6u, v^8u, &c.
2. Returning wave, vu, v^3u, v^5u, v^7u, &c.

And the squares of the velocities of the emergent components will be—

1st surface, $\frac{m'}{m}(v^2u^2-v^4u^2)$, $\frac{m'}{m}(v^6u^2-v^8u^2)$, $\frac{m'}{m}(v^{10}u^2-v^{12}u^2)$, $\frac{m'}{m}(v^{14}u^2-v^{16}u^2)$, &c.

2d surface, $\frac{m'}{m}(u^2-v^2u^2)$, $\frac{m'}{m}(v^4u^2-v^6u^2)$, $\frac{m'}{m}(v^8u^2-v^{10}u^2)$, $\frac{m'}{m}(v^{12}u^2-v^{14}u^2)$, &c.

Consider the movement in the incident wave to be positive. Then if the lamina were without thickness, the successive reflections still going on, the sign of the movement in the diminished waves successively emergent would be always positive for the second surface, (for which the number of reversals by reflection is always even,) and always negative for the first, (for which the number of similar reversals is always odd.)

By giving greater or less thickness to the lamina, any difference of path may be introduced for either the rays seen by reflection or those seen by transmission. If θ represent the thickness, the differences of path which will exist, after the several successive reflections, will be 2θ, 4θ, 6θ, or generally $2m\theta$, m being any integral number. If $\theta=n\times\frac{1}{2}\lambda$, or $2\theta=2n\times\frac{1}{2}\lambda=n\lambda$, n being also any integral number, it is manifest that $2m\theta=mn\lambda$, being a number of complete undulations, cannot change the sign of the movement, whether m be even or odd.

But if $2\theta=(2n+1)\times\frac{1}{2}\lambda$, then $2m\theta=m(2n+1)\times\frac{1}{2}\lambda$ will be an odd number of half undulations when m is odd, and an even number of half undulations, or an integral number of complete undulations, when m is even. Accordingly, the wave changes its sign for every odd value of m.

Hence, if $2\theta=n\lambda$, or $\theta=n\times\frac{1}{2}\lambda$, the movement will be negative for all component waves emergent from the first surface, and positive for all emergent from the second.

Also, for $2\theta=(2n+1)\times\frac{1}{2}\lambda$, or $\theta=(2n+1)\times\frac{1}{4}\lambda$, the signs will be alternately positive and negative at the first surface, and negative and positive at the second.

For the first case, if we take the square roots of the squares of the emergent components given above, substituting the value of u, we shall have for the resultant v' at the first surface,

$$v'=(v^2-1).(v+v^3+v^5\ldots\text{ad inf.})\quad\text{Whence } v'=-v.$$

The interference is therefore absolute, and the rings formed at these thicknesses will be perfectly dark.

For the rays emergent at the second surface we obtain the expression—

$$u'=(1-v^2).(1+v^2+v^4\ldots\text{ad inf.})=(1-v^2).\frac{-1}{v^2-1}=1.$$

In these explanations we have supposed the incidence perpendicular, and have regarded the faces of the laminæ as parallel. In the case of Newton's rings, neither of these suppositions is usually true; and the second can never be so. The inclination of the faces is not, however, great enough sensibly to affect the conclusions. In the case of oblique incidence, it is obvious that no ray reflected from the second surface can return to the same point of the first surface (supposed parallel) at which it entered. But the loss occasioned by this deviation is made good by the reflected component of some other ray parallel to the first, in the plane of incidence and on the other side of it relatively to the point of emergence.

It will thus be seen that the colors of thin plates, for which, on the theory of emission, it is difficult to assign a cause which does not introduce as many difficulties as it removes, are all necessary consequences, on the undulatory theory, of the simple principle of interference. The hypothesis devised by Newton to account for them has not been presented, since it is now generally abandoned, and the limits of these lectures would not allow its introduction.

The colors of thick plates, of which some examples were noticed in the introductory lecture, depend on causes similar to those above explained. In the case illustrated in Fig. 8, which we here introduce again for the sake of the explanation, if the light entering at *o* be composed of rays perfectly parallel, and be returned from the spherical silvered glass mirror, by a perfect specular reflection, to the perforated screen *c*, placed at the centre of curvature, it will all of it pass through the perforation toward *o*, and no rings will appear; or at least only such as might be due to the diffraction of the aperture, very much enfeebled by the reflection. But if the first surface of the glass be imperfectly polished, the specular reflection will not be perfect, but there will be a reflected cone of scattered light at the first incidence. This has nothing to do with the phenomenon. There will, however, be also a transmitted cone of scattered light, which will become at the second surface a reflected cone, having a virtual apex behind the mirror. Moreover, the light transmitted and subsequently reflected regularly, will, at its emergence after reflection, form a second scattered cone, the rays of which will have a virtual origin behind the mirror, though the apex of this cone is at the first surface. The condition of the light of these two cones is easily seen to be such as to produce interference; hence the formation of the rings observed in the experiment.

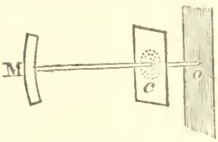

Fig. 8.

That is to say, the brightness of these transmitted rings is equal to that of the incident light.

In the second case, the equation for the first surface is,

$$v'=(1-v^2).(v-v^3+v^5-v^7\ldots\ldots ad\ inf.)$$

This may be separated into two equations, thus:

$$\text{Put } v'=w+w'.$$

$$\text{Also } w=(1-v^2).(v+v^5+v^9+v^{13}\ldots\ldots ad\ inf.)$$

$$\text{And } w'=(v^2-1).(v^3+v^7+v^{11}+v^{15}\ldots\ldots ad\ inf.)$$

$$\text{Then } w=\frac{v}{v^2+1};\quad w'=\frac{-v^3}{v^2+1};\quad \text{and } w+w'=v'=v.\frac{(1-v^2)}{(1+v^2)}.$$

This value is positive, and shows that the rings by reflection at these thicknesses will be bright.

At the second surface, for the same thicknesses, we shall have,

$$u'=(1-v^2).(1-v^2+v^4-v^6\ldots\ldots ad\ inf.).$$

And, by proceeding as before,

$$w+u'=u'=\frac{(1-v^2)}{(1+v^2)}.$$

Showing that the rings seen at these thicknesses by transmitted light are obscure, but not dark, because u', which embraces all the light transmitted, has still a value.

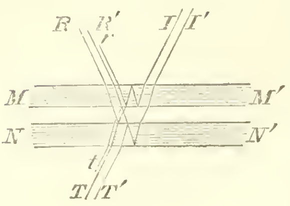

Fig. 9.

In the other case, it will be observed that the rays I and I′ undergo repeated reflections between the surfaces of the plates—a portion of the light escaping and being transmitted at each reflection. If the plates are of perfectly equal thickness and all the surfaces perfectly parallel, there will be no interference. Suppose, however, that one of them is slightly thicker than the other. Then, if we attend first to the transmitted rays, T, T′, we shall see that the path of T, after incidence and up to final emergence, is made up of three times the thickness of the first plate, once the thickness of the second, and once the distance between the plates. Put θ for the first thickness, θ' for the second, δ for the distance between, and L for the length of path. Then—

$$L=3\theta+\theta'+\delta.$$

Tracing back T′ in the same way, and denoting its length of path by L′, we have—

$$L'=\theta+3\theta'+\delta.$$

$$\text{Hence } L'-L=2(\theta'-\theta).$$

And when this value is so small as to be comparable to the absolute thicknesses which produce the colors of Newton's rings, similar colors may be seen here.

If we attend to the reflected rays R and R′, we shall see (employing the same notation as before) that—

$$L=4\theta+2\delta.$$

$$L'=2\theta+2\theta'+2\delta.$$

$$\text{Hence } L'-L=2\,(\theta'-\theta), \text{ as before.}$$

It will be noticed that there are other rays, as *r* and *t*, which do not form tints, their differences of path, as compared with R, R′, or with T, T′, being too great.

§. VII.—POLARIZATION BY REFLECTION AND BY REFRACTION.

We will now proceed to give a physical theory of the phenomenon called polarization of light, and of its production by reflection and refraction. It has already been hinted that the phenomenon itself consists in the determination of the molecular movements in the succession of undulations which constitutes a ray or beam of light, to one constant azimuth, or definite direction in space; those which exist in common light being distributed impartially through all azimuths. In order to simplify the problem of the influence of reflection upon molecular movement Mr. Fresnel commenced this investigation by considering first the case of a wave polarized already in the plane of incidence. In such a wave the molecular movements are (for reasons which will appear hereafter) presumed to take place in a plane which is *at right angles to the plane of polarization*. At the reflecting surface, they are therefore coincident with the surface itself. If the ray is passing from a medium of less refracting power into one of greater, we must suppose that the ether possesses either a different elasticity, or a different density, or both, in the two media. Mr. Fresnel assumed a difference of density without a difference of elasticity. He assumed, secondly, that in the common surface or stratum bounding the media, the movements parallel to the surface are common to both media, so that the components of velocity in the incident and reflected wave, parallel to the surface of reflection, are together equal to the component of velocity in the transmitted wave parallel to the same surface. Or, if unity represent the incident molecular velocity, *v* that of the reflected wave, and u that of the transmitted wave, we shall have the equation, $1+v=u$.

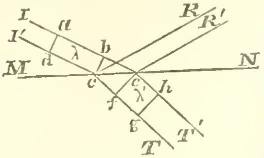

Fig. 47.

With these suppositions let I*e*, I*c* be the bounding limits of a mass of the ether, along which an undulation moves, meeting the reflecting surface MN in *ce*. Let *c*R, *e*R′ be the limits of the reflected undulation, and *c*T, *e*T′ those of the transmitted undulation. Draw *cb*, *cf* perpendicular to I*e*, *c*T. Let *cd* be the length of the incident, and *eh* that of the transmitted undulation. Draw *ad*, *gh* parallel to *cb*, *cf*, respectively. We may regard the incident undulation as a mass whose bulk is the prism *abcd*, and density δ, impinging upon a mass whose bulk is *cfgh*, and density δ'. Since the molecular movements in this case are *in* the the common surface of the media, the *breadths* of the prisms, according to the second assumption foregoing, are equal to each other. Their *lengths* will be λ and λ', and their depths *bc* and *cf*. Or, putting m and m' for their masses, $m : m' :: bc \times \lambda \times \delta : cf \times \lambda' \times \delta'$ Now, the wave lengths are proportional to the velocities of progress of the wave in the two media, (which we may denote by V and V′.) Hence, if we put ι for the angle of incidence, and ρ for the angle of refraction, we shall have, $\lambda : \lambda' :: V : V' :: \sin\iota : \sin\rho$. Also, $ecb=\iota$, and $ecf=\rho$; from which we derive $bc=ce.\cos\iota$, and $cf=ce.\cos\rho$. And substituting—

$$m : m' :: \sin\iota\cos\iota.\delta : \sin\rho\cos\rho.\delta'.$$

But, in the propagation of tremors through elastic media, the velocity of progress is directly as the square root of the elasticity, and inversely as the square root of the density. Or, putting ε for the elasticity, $V^2=\frac{\varepsilon}{\delta}$, and $V'^2=\frac{\varepsilon}{\delta'}$, ε being constant. Accordingly—

$$\delta : \delta' :: \frac{1}{V^2} : \frac{1}{V'^2}; \text{ and, by substitution,}$$

$$m : m' :: \frac{\sin\iota\cos\iota}{V^2} : \frac{\sin\rho\cos\rho}{V'^2} :: \frac{\sin\iota\cos\iota}{\sin^2\iota} : \frac{\sin\rho\cos\rho}{\sin^2\rho} :: \frac{\cos\iota}{\sin\iota} : \frac{\cos\rho}{\sin\rho}.$$

Now the living force in the reflected and transmitted waves must be equal to the living force in the incident; or—

$$m \times 1^2 = m \times v^2 + m' \times u^2; \text{ that is,}$$

$$\frac{\cos\iota}{\sin\iota} = \frac{\cos\iota}{\sin\iota}v^2 + \frac{\cos\rho}{\sin\rho}u^2; \text{ or, } (1-v^2).\frac{\cos\iota}{\sin\iota} = \frac{\cos\rho}{\sin\rho}u^2.$$

From this, with the equation previously given, $u=1+v$, we obtain, by elimination,

$$v^2 = \left(\frac{\sin(\iota-\rho)}{\sin(\iota+\rho)}\right)^2, \text{ and } u^2 = \left(\frac{2\cos\iota\sin\rho}{\sin(\iota+\rho)}\right)^2. \qquad [19.]\,[20.]$$

If, in order to embrace in the formula only the angle of incidence, we eliminate ρ from the foregoing by means of the equation $\sin\iota = n\sin\rho$, we shall obtain—

$$v^2 = \left(\frac{\sqrt{n^2-\sin^2\iota}-\cos\iota}{\sqrt{n^2-\sin^2\iota}+\cos\iota}\right)^2, \text{ and } u^2 = \left(\frac{2\cos\iota}{\sqrt{n^2-\sin\iota}+\cos\iota}\right)^2. \quad [21.]\,[22.]$$

When the incidence is perpendicular, $\iota = 0^\circ$, $\sin\iota = 0$, and $\cos\iota = 1$. In this case, $v^2=\left(\frac{n-1}{n+1}\right)^2$; and $u^2=\left(\frac{2}{n+1}\right)^2$.

The intensity of light is measured by the living force of the molecular movements, or by the mass multiplied by the square of the velocity. As the mass is the same for the incident and reflected waves, their intensities are as 1^2 and v^2. When we compare the transmitted with the incident or reflected waves, we must consider the masses. By multiplying v^2 by $\frac{\cos\iota}{\sin\iota}$, and u^2 by $\frac{\cos\rho}{\sin\rho}$, their sum will be found equal to $\frac{\cos\iota}{\sin\iota}$, which is $m\times 1^2$, or the living force of the incident wave.

For perpendicular incidence $m=\frac{\cos\iota}{\sin\iota}$ and $m'=\frac{\cos\rho}{\sin\rho}$ become infinite; but their ratio remains finite; and as they are not expressions for the *absolute* values of the masses, but only of their relative values, their ratio only is needed. By replacing $\sin\iota$ by its equivalent $n\sin\rho$, we have—

$$m : m' :: \frac{\cos\iota}{n\sin\rho} : \frac{\cos\rho}{\sin\rho} :: \frac{\cos\iota}{n} : \cos\rho :: \cos\iota : n\cos\rho\,;$$

which, when $\cos\iota=\cos 0°=\cos\rho=1$, gives $m : m' :: 1 : n$. And the sum of the intensities at perpendicular incidence is—

$$1\times v^2+n\times u^2=\frac{(n-1)^2}{(n+1)^2}+\frac{4n}{(n+1)^2}=\frac{(n+1)^2}{(n+1)^2}=1=1\times 1^2,$$

which is the intensity of the incident wave.

If now we consider the general value of v^2, given above, we shall see that v increases with the increase of the angle of incidence, and becomes equal to the total molecular velocity of the incident wave, when $\iota=90°$. For, at this incidence, $\sin^2\iota=1$, and $\cos\iota=0$. Hence—

$$v^2=\left(\frac{\sqrt{n^2-1}}{\sqrt{n^2-1}}\right)^2=1\,;\ \text{and}\ u^2=0.$$

For intermediate incidence, we may transform the expression thus:

$$v^2=\left(\frac{\sqrt{(n^2-1)+\cos^2\iota}-\cos\iota}{\sqrt{(n^2-1)+\cos^2\iota}+\cos\iota}\right)^2$$

The value of the radical diminishes with the increase of the incidence; but it diminishes less rapidly than $\cos\iota$. For, the form of a binomial square being—

$$x^2+2xy+y^2,$$

if we put $2xy+y^2=$ *constant*, it is evident that as x diminishes, y must increase. Putting therefore, $\cos\iota$ in place of x, we shall have—

$$\cos^2\iota+2y\cos\iota+y^2=\cos^2\iota+(n^2-1).$$

$$\text{or,}\quad 2y\cos\iota+y^2=n^2-1=\text{constant}\,;$$

and as $\cos\iota$ diminishes, the other part of the root of the radical increases, so that the value of the entire radical diminishes less rapidly than $\cos\iota$. The numerator of the expression accordingly increases with increase of incidence, and the denominator diminishes: and both these changes increase the value of v. Hence, the amount of light reflected increases from incidence $=0°$ to incidence $=90°$. It is worth observing that the expressions we have obtained above for the molecular velocities of the reflected and refracted wave, are also deducible directly from the ordinary formulæ for the impact of elastic bodies. These formulæ, (employing m and m' for the masses, as above,) are—

$$v=\frac{m-m'}{m+m'}\,;\ \text{and}\ u=\frac{2m}{m+m'}.$$

If, in place of m and m', we substitute the values found for the masses, viz: $m=\frac{\cos\iota}{\sin\iota}$, and $m'=\frac{\cos\rho}{\sin\rho}$, the foregoing formulæ will be reproduced.

Let us next examine the case of a wave polarized in a plane at right angles to the plane of incidence. In this case, the molecular movements are *in* the plane of incidence. The expressions for the masses will be the same as before; but the *components* of the molecular velocities *parallel to the reflecting surface* are to be taken, instead of the velocities themselves. These components are, for the incident wave, $1\times\cos\iota$; for the reflected wave, $v'\times\cos\iota$; and for the transmitted wave, $u'\times\cos\rho$: and the assumption of Fresnel is—

$$(1+v')\cos\iota=u'\cos\rho.\text{*}$$

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* In the attempt to apply the theory of undulation to the case of reflection and refraction at the surfaces of crystalline media, it has been found more satisfactory by Profs. McCullagh and Neuman to employ the following assumptions, viz:

I. The vibrations of polarized light are parallel to the plane of polarization.

II. The density of the ether in both media is the same as in vacuo.

III. The vis viva is preserved.

IV. The resultant of the vibrations is the same in both media; and therefore, in singly refracting media, the vibration of the refracted ray is the resultant of the vibrations of the incident and reflected rays.

On these principles, the case of reflection in the plane of polarization is simple. Let the refracted ray be extended backward, and it will divide the angle between the incident and reflected rays (which equals 2ι) into two parts, which are, respectively, $\iota+\rho$ and $\iota-\rho$. Upon this retroduced line as a diagonal, if a parallelogram be constructed with the incident and reflected rays as sides, this diagonal and these sides will be proportional to the amplitudes, and therefore to the velocities, of the molecular movements which are perpendicular to them severally. Employing then, as before, unity and the symbols v and u to designate these velocities, we shall have directly—

$$v=\frac{\sin(\iota-\rho)}{\sin(\iota+\rho)}.\qquad u=\frac{\sin2\iota}{\sin(\iota+\rho)}.$$

In the case of reflection in a plane at right angles to the plane of polarization the vibrations are all parallel, and the fourth principle above gives—

$$1+v'=u'.$$

Also the third principle gives (m and m' representing the masses of the ether put into motion in the contiguous strata of the two media)—

$$m(1-v'^2)=m'u'^2.$$

These equations lead to the values—

$$v'=\frac{m-m'}{m+m'}.\qquad u'=\frac{2m}{m+m'}.$$

The same values may also be deduced from the laws of impact of elastic bodies

According to the second principle, the masses m and m' are proportional to their volumes; and these volumes have been found in the text to be proportional to $\sin\iota\cos\iota$ and $\sin\rho\cos\rho$. Substituting these expressions in place of m and m' in the foregoing fractions, we shall obtain—

$$v'=\frac{\tan(\iota-\rho)}{\tan(\iota+\rho)}.\qquad u'=\frac{\sin2\iota}{\sin(\iota+\rho)\cos(\iota-\rho)}=\frac{2\sin2\iota}{\tan(\iota+\rho)(\cos2\iota+\cos2\rho)}.$$

Comparing these values with those of the text, we find those for v alike, but with reversed signs and interchanged—that which represented the velocity of molecular movement *normal to* the plane of polarization before, now denoting the velocity of movement *in* the plane, and *v.v.* In the expressions for the transmitted rays there is a difference which results from the adoption of the second of the principles foregoing, making the density of the ether in the two media the same, which is not the supposition of the text.

However strongly on some accounts this view of the subject may seem to recommend itself to our acceptance, it introduces a difficulty (elsewhere noticed) into the theory of double refraction, which has never yet been met, and which seems to have been singularly ignored by many who have engaged in this discussion.

In order to facilitate the comparison of the values of the several expressions foregoing, they may be reduced to a simple form of common denominator, when they become—

1. For the case of vibration in the plane of reflection—

$$v=\frac{\sin2(\iota-\rho)}{\sin2\iota+\sin2\rho}.\qquad u=\frac{2\sin2\iota\cos(\iota-\rho)}{\sin2\iota+\sin2\rho}.$$

2. For vibration normal to the plane of reflection—

$$v'=\frac{\sin2\iota-\sin2\rho}{\sin2\iota+\sin2\rho}.\qquad u'=\frac{2\sin2\iota}{\sin2\iota+\sin2\rho}$$

Combining this with the equation of living forces given above, and reducing, we obtain these results:

$$v'^2=\left(\frac{\tan(\iota-\rho)}{\tan(\iota+\rho)}\right)^2, \text{ and } u'^2=\left(\frac{2\cos\iota\sin\rho}{\sin(\iota+\rho)\cos(\iota-\rho)}\right)^2. \qquad [23.]\ [24.]$$

Replacing, as before, the value of $\sin\rho$ by its equivalent derived from the equation $\sin\iota=n\sin\rho$, we arrive at the following values which embrace only one variable:

$$v'^2=\left(\frac{\sqrt{n^2-\sin^2\iota}-n^2\cos\iota}{\sqrt{n^2-\sin^2\iota}+n^2\cos\iota}\right)^2; \text{ and } u'^2=\left(\frac{2\cos\iota}{\sqrt{n^2-\sin^2\iota}+n^2\cos\iota}\right)^2. \qquad [25.]\ [26.]$$

The following forms are convenient for discussion:

1. For vibration in the plane of reflection—

$$v=\frac{n\cos\rho-\cos\iota}{n\cos\rho+\cos\iota}. \qquad u=\frac{2n\cos\iota}{n\cos\rho+\cos\iota}.$$

2. For vibration normal to the plane of reflection—

$$v'=\frac{n\cos\iota-\cos\rho}{n\cos\iota+\cos\rho}. \qquad u'=\frac{2n\cos\iota}{n\cos\iota+\cos\rho}.$$

At a perpendicular incidence $\cos\iota=1$ and $\cos\rho=1$. Hence, in both cases,

$$v=\frac{n-1}{n+1}, \text{ and } u'=\frac{2n}{n+1}.$$

Thus u and u' will always be positive, and v and v' will be positive when n is greater than 1 and negative when n is less than 1. This ought to be so, according to the laws of impact of elastic bodies, because, the density of the ether being by hypothesis the same in both media, the masses reacting on each other will be as $\sin\iota$ to $\sin\rho$, or as n to 1.

As the incidence increases, the variations of the value of v and v' will be dissimilar. When vibration is in the plane of reflection and n exceeds unity, the positive term $n\cos\rho$ is necessarily always greater than the negative term $\cos\iota$. Both these terms diminish as ι increases. If they diminished at the same rate, the value of v would be constant. But as ι is always greater than ρ and neither exceeds 90°, the rate of diminution of $\cos\iota$ is more rapid than that of $\cos\rho$, and the value of v increases with the incidence. The same is true when n is less than unity; only in that case the increasing value of v is negative. When the incidence is maximum, or $\iota=90°$, n being greater than 1, $\cos\iota=0$ and $v=1$; that is to say, the reflected is equal to the incident light.

For the amount transmitted in the same case, we have, at a perpendicular incidence—

$$u=\frac{2n}{n+1}.$$

And for $\iota=90°$, or the maximum incidence, n being greater than 1,

$$u=0.$$

It is also apparent that the amount transmitted constantly diminishes as ι increases.

When vibration is normal to the plane of reflection, the positive term in the value of v', which is $n\cos\iota$, is at first greater (n being greater than unity) or less (n being less than unity) than the negative term $\cos\rho$. But since at the maximum incidence $\cos\iota=0$, there must be some value of ι which will give $n\cos\iota=\cos\rho$, or $n\cos\iota-\cos\rho=0$. Accordingly, at this incidence no light will be reflected.

The two conditions—

$$n\cos\iota=\cos\rho, \text{ and } n\sin\rho=\sin\iota,$$

give immediately—

$$\sin\rho=\cos\iota, \text{ or } \iota+\rho=90°.$$

The incidence ι is the polarizing incidence; and we here see that it fulfils the law of Brewster.

At the maximum incidence, n being greater than 1, $v'=-1$. The sign of the molecular movement, therefore, changes at the polarizing angle.

The transmitted light is in this case, at the perpendicular incidence,

$$u'=\frac{2n}{n+1}.$$

And at the maximum incidence,

$$u'=0.$$

At the polarizing incidence, where, as we have just seen, $\cos\rho=n\cos\iota$,

$$u'=\frac{2n\cos\iota}{n\cos\iota+n\cos\iota}=1.$$

If we suppose the incidence perpendicular, we shall have, as before, $v'^2=\left(\frac{n-1}{n+1}\right)^2$, and $u'^2=\left(\frac{2}{n+1}\right)^2$. At incidence 90°, we have again, $v'^2=\left(\frac{\sqrt{n^2-1}}{\sqrt{n^2-1}}\right)^2=1$; and $u'^2=0$.

The agreement of these formulas with those obtained in the other case, is what ought to be expected, since, at a perpendicular incidence, the direction of molecular movement can have no influence on reflection; and at 90° impact ceases. But if we examine the first value of v' given above, we shall perceive that it does not constantly increase with the increase of the incidence; for the denominator, $\tan(\iota+\rho)$, becomes infinite when $\iota+\rho=90°$; and at this incidence $v'=0$, or there is no reflection. If then, in the originally incident beam, there had been a succession of waves, some of them polarized in the plane of incidence, and the rest polarized at right angles to that plane, all this latter class of waves would, at this particular incidence, be transmitted, while a portion of the others would be reflected. The incident light, from the mixture of the two classes of waves, would be imperfectly polarized, or not polarized at all: but the reflected light would be wholly polarized in the plane of reflection.

In the result just reached, we see a reproduction of the law experimentally established by Brewster, viz., that, at the polarizing angle, the transmitted ray is at right angles to the incident ray, or $\iota+\rho=90°$.

If we now take the case of a wave whose plane of polarization is in any azimuth to the plane of reflection between 0° and 90°, we may apply the principles already illustrated, by decomposing its molecular movements into components, one of which shall coincide with the plane of reflection, and the other with the reflecting surface. If the given azimuth be α, the azimuth of the molecular movements will be $90°-\alpha$. The molecular movement in the *plane of reflection* will therefore be $\cos(90°-\alpha)=\sin\alpha$; and that in the *reflecting surface* will be

---

That is to say, at this incidence the entire molecular movement normal to the plane of reflection is transmitted.

In this case the condition $1+v'=u'$ is always fulfilled. When vibration is in the plane of reflection it is fulfilled only for the perpendicular incidence. At other incidences the fourth principle of McCullagh and Neuman, quoted above, necessarily involves the truth of Fresnel's assumption for this case, viz:

$$(1+v')\cos\iota=u'\cos\rho.$$

When n is less than 1, these formulæ fail for incidences beyond the limiting angle of total reflection.

The formulæ in the text admit of reductions similar to the foregoing. They thus become—

1. For vibrations in the plane of reflection—

$$v=-\frac{\sin2\iota-\sin2\rho}{\sin2\iota+\sin2\rho}. \qquad u=\frac{4\sin\rho\cos\iota}{\sin2\iota+\sin2\rho}.$$

2. For vibrations normal to the plane of reflection—

$$v'=-\frac{\sin2(\iota-\rho)}{\sin2\iota+\sin2\rho}. \qquad u'=\frac{4\sin\rho\cos\iota\cos(\iota-\rho)}{\sin2\iota+\sin2\rho}.$$

And for convenience of discussion:

1. For vibrations in the plane of reflection—

$$v=-\frac{n\cos\iota-\cos\rho}{n\cos\iota+\cos\rho}. \qquad u=\frac{2\cos\iota}{n\cos\iota+\cos\rho}.$$

2. For vibrations normal to the plane of reflection—

$$v'=-\frac{n\cos\rho-\cos\iota}{n\cos\rho+\cos\iota}. \qquad u'=\frac{2\cos\iota}{n\cos\rho+\cos\iota}$$

The first of these values of v becomes zero at the polarizing angle, and is positive for all higher incidences.

$\cos\alpha$. The living force in the reflected beam (which we may represent by R) will consequently be—the mass being assumed for convenience=unity—

$$R=\left(\frac{\sin(\iota-\rho)}{\sin(\iota+\rho)}\right)^2.\cos^2\alpha+\left(\frac{\tan(\iota-\rho)}{\tan(\iota+\rho)}\right)^2.\sin^2\alpha. \qquad [27.]$$

The agreement of this result with our previous conclusions may be verified by making α successively $=0^\circ$ and $=90^\circ$. In the first case—

$$R=\left(\frac{\sin(\iota-\rho)}{\sin(\iota+\rho)}\right)^2;\ \text{and in the second, } R=\left(\frac{\tan(\iota-\rho)}{\tan(\iota+\rho)}\right)^2$$

If $\alpha=45^\circ$, $\sin^2\alpha=\frac{1}{2}$, and $\cos^2\alpha=\frac{1}{2}$. Consequently,

$$R=\tfrac{1}{2}\left(\left(\frac{\sin(\iota-\rho)}{\sin(\iota+\rho)}\right)^2+\left(\frac{\tan(\iota-\rho)}{\tan(\iota+\rho)}\right)^2\right). \qquad [28.]$$

This might easily be anticipated by considering that, in the supposed case, the incident beam is equivalent to two beams, each having an intensity of $\frac{1}{2}$, and polarized—one in the plane of reflection, and the other in the plane perpendicular to it. The reflected beam should contain one-half the force in each plane which it would have done had each intensity been $=1$.

Let there now be two beams each $=\frac{1}{2}$, incident together, and polarized in the azimuths α and α'. From what has just been said, it is evident that the value of R will be—

$$R=\tfrac{1}{2}\left(\frac{\sin(\iota-\rho)}{\sin(\iota+\rho)}\right)^2.(\cos^2\alpha+\cos^2\alpha')+\tfrac{1}{2}\left(\frac{\tan(\iota-\rho)}{\tan(\iota+\rho)}\right)^2.(\sin^2\alpha+\sin^2\alpha'). \qquad [29.]$$

In this expression, if $\alpha'=90^\circ-\alpha$, $\cos^2\alpha+\cos^2\alpha'=1$. Also $\sin^2\alpha+\sin^2\alpha'=1$. R becomes, therefore, equal to the sum of the intensities of two rays each $=\frac{1}{2}$, polarized, one in the plane of incidence, and the other at right angles to it, no matter what may be the value of α. If, then, any number of waves, in different azimuths, follow each other in so close succession as to blend their impressions upon the eye, and if their azimuths are so impartially distributed that for every value of α there is another $=90^\circ-\alpha$, the forces in all these azimuths being equal, then the resultant effect of the whole must necessarily be—

$$R=\tfrac{1}{2}\left(\frac{\sin(\iota-\rho)}{\sin(\iota+\rho)}\right)^2+\tfrac{1}{2}\left(\frac{\tan(\iota-\rho)}{\tan(\iota+\rho)}\right)^2. \qquad [30.]$$

But this is the condition of common light. The formula just stated, therefore, represents the living forces in the two principal planes, in a beam of common light after reflection, the original force being taken $=1$. When $\iota+\rho=90^\circ$, the second term disappears. The reflected beam is then entirely polarized.

If we decompose the second term in the value of R, above, into its factors, we shall have (disregarding the numerical coefficient, and omitting the exponent)—

$$\frac{\tan(\iota-\rho)}{\tan(\iota+\rho)}=\frac{\sin(\iota-\rho)}{\sin(\iota+\rho)}.\frac{\cos(\iota+\rho)}{\cos(\iota-\rho)}. \qquad [31.]$$

The molecular velocity of the wave polarized at right angles to the plane of reflection appears thus to be equal to that of the wave polarized *in* the plane of reflection, multiplied by the factor $\frac{\cos(\iota+\rho)}{\cos(\iota-\rho)}$. When $\iota=90^\circ$ (which is the greatest value it can have) the numerator and denominator of this fraction are equal, with opposite signs. The sign does not concern us at present, as it has no effect upon the value of the living force in the wave. For all values of ι less than 90° (ρ being necessarily less than ι when n exceeds 1) the denominator is greater than the numerator. It follows that in the reflection of common light, a larger amount of living force will, in the reflected beam, be preserved in the movements perpendicular to the plane of reflection than in those coincident with that plane; or, in other words, that the reflected beam will be more or less polarized in the plane of reflection.

In order to estimate the *amount* of this polarization, we must take the difference between the two terms in the value of R. And if we desire to find the *proportion* of light polarized, we must divide this difference by the sum. Or putting P to represent this proposition:

$$P=\frac{\frac{\sin^2(\iota-\rho)}{\sin^2(\iota+\rho)}\cdot\left(1-\frac{\cos^2(\iota+\rho)}{\cos^2(\iota-\rho)}\right)}{\frac{\sin^2(\iota-\rho)}{\sin^2(\iota+\rho)}\cdot\left(1+\frac{\cos^2(\iota+\rho)}{\cos^2(\iota-\rho)}\right)}=\frac{\cos^2(\iota-\rho)-\cos^2(\iota+\rho)}{\cos^2(\iota-\rho)+\cos^2(\iota+\rho)}. \qquad [32.]$$

Reverting once more to the case of a wave polarized in azimuth α to the plane of reflection, we shall perceive, by the formulæ, that after reflection it is still polarized, though not in the same plane as before; for the rectangular components of the molecular movement being unequally altered, their resultant must have a new direction. In the expressions,

$$v=\pm\frac{\sin(\iota-\rho)}{\sin(\iota+\rho)}.\cos\alpha,\ v'=\pm\frac{\tan(\iota-\rho)}{\tan(\iota+\rho)}.\sin\alpha=\pm\frac{\sin(\iota-\rho)}{\sin(\iota+\rho)}.\frac{\cos(\iota+\rho)}{\cos(\iota-\rho)}.\sin\alpha.$$

the first is the molecular velocity normal to the plane of reflection, and the second is the same velocity *in* the plane of reflection. The second divided by the first is therefore the tangent of the inclination of the molecular movement to the normal; or of the resultant plane of polarization to the plane of reflection. Putting this inclination $=\alpha'$, we have

$$\tan\alpha'=\frac{\sin\alpha}{\cos\alpha}.\frac{\cos(\iota+\rho)}{\cos(\iota-\rho)}=\tan\alpha.\frac{\cos(\iota+\rho)}{\cos(\iota-\rho)}. \qquad [33.]$$

If the reflected ray undergo reflection from a second mirror parallel to the first, its incident azimuth will be α'; and, after reflection, it will have another azimuth α'', of which the tangent will be

$$\tan\alpha''=\tan\alpha'.\frac{\cos(\iota+\rho)}{\cos(\iota-\rho)}=\tan\alpha.\frac{\cos^2(\iota+\rho)}{\cos^2(\iota-\rho)}. \qquad [34.]$$

And, as the law is manifest, we may say that after any number, n, of reflections, the tangent of the azimuth will be

$$\tan\alpha^{(n)}=\tan\alpha\left(\frac{\cos(\iota+\rho)}{\cos(\iota-\rho)}\right)^n. \qquad [35.]$$

While $\cos(\iota+\rho)$ has a value—that is, while $\iota+\rho$ is more or less than 90°—$\tan^{(n)}\alpha$ will also have a value; or the plane of polarization of the wave will not be brought, by any number of reflections, into absolute coincidence with the plane of reflection; but when $\iota+\rho=90°$, it will be so by the first reflection. When $\frac{\cos(\iota+\rho)}{\cos(\iota-\rho)}$ is a small fraction, or at least not a large one, the plane of polarization will, after a few reflections, be brought *sensibly* into the plane of reflection. For instance, let ι be 45°, and also $\alpha=45°$. Then, for glass (index 1.50) ρ will be 28° nearly; and $\frac{\cos(\iota+\rho)}{\cos(\iota-\rho)}$ will be about $\frac{3}{10}$. In this case one reflection will reduce the azimuth to 16° 42′; two to 5° 9′; three to 1° 32′; four to 0° 28′; and five to 0° 8½′.

§ VIII.—CIRCULAR AND ELLIPTICAL POLARIZATION BY REFLECTION.

In all that precedes it has been tacitly assumed that the initial phase of the reflected undulation is a continuation of the final phase of the incident, or of the same reversed; and also that the virtual origins of the elementary waves of which we suppose the resultant reflected wave to be composed, are in *one invariable surface*, whatever be the azimuth of the incident molecular movements. If these assumptions are entirely true, the expressions for the molecular velocity of the reflected wave ought to correspond with observation in all cases. These expressions, nevertheless, fail for the case of total reflection at the second surfaces of denser media, as will be apparent if we substitute the value of n, which, in the case supposed, is less than unity in the formulæ.

$$r^2=\left(\frac{\sqrt{n^2-\sin^2\iota}-\cos\iota}{\sqrt{n^2-\sin^2\iota}+\cos\iota}\right)^2;\quad r'^2=\left(\frac{\sqrt{n^2-\sin^2\iota}-n^2\cos\iota}{\sqrt{n^2-\sin^2\iota}+n^2\cos\iota}\right)^2.$$

When $\sin^2\iota=n^2$, r^2 and r'^2 each $=1$; or the reflection is total. We know, experimentally, that it continues to be total for all higher values of ι; but the radicals in the foregoing become imaginary. Mr. Fresnel, therefore, concluded that reflection in some manner modifies the *phase* of the undulation. Experiment proves that it does so, and also that the degree of the modification depends upon the azimuth of the molecular movements, and upon the incidence.

The conversion of plane into circular polarization by reflection in "Fresnel's rhombs" has been described. The manner in which this change takes place may now be understood. If the plane ray is incident in either of the principal azimuths 0° or 90°, its plane of polarization is not affected by reflection. But if its azimuth be 45°, it emerges from the rhomb after having undergone two total reflections circularly polarized. Now the plane polarized ray in azimuth 45°, is equivalent to two plane polarized rays of half the intensity in azimuths 0° and 90.° And as these components would singly undergo no sensible change of plane by reflection, while jointly they produce a circularly polarized ray, we infer that one of them has been advanced or retarded upon the other by a quarter of an undulation. If the ray had undergone only one reflection in the rhomb, or if it had undergone three, it would have emerged neither plane polarized nor circularly polarized. If it had undergone four, it would have emerged plane polarized again, with a change of 90° from its original azimuth. Now, all these phenomena are represented by the equation [1] for the resultant of vibrations at right angles to each other, which is as follows:

$$a'^2y^2+a^2x^2-2aa'xy\cos\theta=a'^2a^2\sin^2\theta.$$

If we substitute for θ, which is the interval between the two compounded undulations, the more convenient symbol $2\pi\frac{h}{\lambda}$, in which $\frac{h}{\lambda}$ expresses the difference of phase as a fraction of an undulation, the equation takes the form,

$$a'^2y^2+a^2x^2-2aa'xy\cos2\pi\frac{h}{\lambda}=a'^2a^2\sin^2 2\pi\frac{h}{\lambda}.$$

This equation becomes the equation of a circle if we make $a=a'$, and $\frac{h}{\lambda}=\frac{1}{4}$, or an odd multiple of $\frac{1}{4}$. It is then $x^2+y^2=a^2$. It is the equation of an ellipse for any values of $\frac{h}{\lambda}$ if a is not $=a'$. Hence the necessity of the condition that the original plane of polarization should be in azimuth 45°, that the components into which the velocity is decomposed in the principal azimuths may be equal. It is an equation of an ellipse when $a=a'$, if $\frac{h}{\lambda}$ is not $=\frac{1}{4}$, or an odd multiple of $\frac{1}{4}$. The ellipse becomes a straight line if $\frac{h}{\lambda}=\frac{1}{2}$, or an odd multiple of $\frac{1}{2}$. It is therefore evident that, at each reflection in the rhomb, one of the component waves is accelerated or retarded one-eighth of an undulation upon the other.

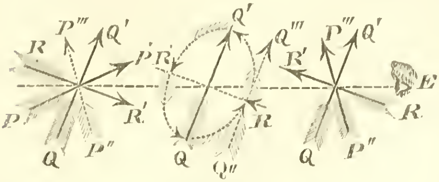

Fig. 48.

The restoration of the plane polarization, after four reflections, in an azimuth 90° removed from the azimuth of incidence, may be understood by considering the following illustration. Let the arrow PP' represent the amplitude of movement in a polarized wave at a given moment. Let this be resolved into two rectangular components 45° inclined to it on each side, represented by the arrows QQ' and RR'. Suppose the molecule, at a given instant, to be situated at the point C* of its path. CP' is the direction in which it is moving, and the length of the line is the extent of its range. By two reflections in Fresnel's rhomb, or by any other cause, let the component RR' be so advanced upon QQ' that, at some future instant, the molecule shall have reached the limit of its range in the horizontal direction, and shall be about to return (as at R in the second system of arrows) when, in the vertical direction it is in the middle point, and going toward Q' as before. This would be in C', but for the horizontal displacement. In point of fact, it is at R. The vertical velocity, (represented by the dotted arrow Q''Q''',) is at its maximum, and the horizontal velocity is zero. The conditions are such as, in the section on vibration, are shown to produce a revolution of the molecule in a circle. The path of the molecule will accordingly be RQ'R'Q. After two additional reflections in the rhomb, the horizontal movement will be advanced over the vertical by another quarter of its double vibration, and will bring the molecule, in its progress from right to left, to the middle point, C'', in the third system of arrows, at the same instant at which the vertical movement, in the direction Q', brings it to the same point. The velocities are now both at their maximum, and are equal. The molecule takes the mean direction, P''P''', between them, and the ray is plane polarized in an azimuth 90° from the original plane.

Suppose the eye of the observer to be at E, the revolution of the molecule is dextrogyre, or, as it is also called, *dextrorsum.* Conceived as viewed in the opposite direction, it would be lævogyre, or *sinistrorsum.* In the earlier history of this subject, some confusion arose from the fact that different observers applied these terms in different ways. Since observation, however, is only made upon rays approaching the observer, this is the point of view now universally adopted in naming the direction of gyration. It appears, then, that the advance of the left-hand component, by a quarter undulation, produces a dextrogyration, and *vice versa.* If the plane of original polarization were P''P''', then, in the resolution, RR' would be reversed, and the advance of RR' would be the advance of the right-hand component, producing lævogyration. In this case, after four reflections in Fresnel's rhomb, the resultant plane of polarization would be PP'.

If we distinguish, as *positive*, the azimuths to the right of the plane of reflection, and, as *negative*, those to the left, we may say that a plane polarized ray in original azimuth —45°, is circularly polarized dextrorsum by passing through one of Fresnel's rhombs; and becomes plane polarized again in azimuth +45°, after passing through two. If the original azimuth be +45, the circular polarization is sinistrorsum, and the final azimuth of plane polarization, negative. One of these rhombs may, therefore, be used as a polariscope, to detect the direction of rotation of a circularly polarized ray.

If two rays, one in azimuth +45°, and the other in azimuth —45°, were to be reflected simultaneously in one of Fresnel's rhombs, the two consequent

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* The letters C, C', C'', are accidentally omitted from the diagram. They should be placed on the dotted line at the intersections of the successive systems of arrows.

gyrations, being in opposite directions, would produce a rectilinear resultant. In this case, suppose the molecule, M, to be in any part of the circumference in which either of the gyrations would cause it to revolve; it will be subject to the action of three forces: one, MC, directed toward the centre of its orbit, and the other two, represented by P and Q, equal and opposite. The two latter neutralize each other, and the molecule pursues the path MC. When the molecule is at M′, the tangential forces P and Q, which will then have the directions P′ and Q′, will not directly balance each other, but will have a resultant in the direction RC. And for all other points in the path of the molecule, as M″, M‴, &c., the resultant of the tangential forces will always be in the diameter, MN, of the orbit.

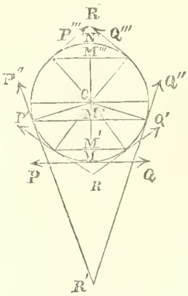

Fig. 49

In Fig. 48 we have supposed the arrows PP′ and P″P‴ to represent not only the *positions of the planes* of molecular vibration, but the *direction* of the movements. Their resultant plane is accordingly QQ′. If the direction of one of them, as of P″P‴, had been opposite, the resultant would be RR′. If the two were in any equal positive and negative azimuths, greater or less than 45°, their resultant gyrations would be elliptic; but the ellipses, being equal and similar, and similarly situated to the plane of reflection, while they are opposite in movement, would still produce the vibration QQ′. And two movements in azimuths equally above and below 90°, either positive or negative, would in like manner produce the plane vibration RR′. Now the condition of natural light is such that, for every azimuth of its successive plane vibrations, as PP′, producing, by total reflection, a gyratory molecular movement, whether circular or elliptic, there will always be found another which will produce an equal and opposite gyration. And, although these gyrations are successive and not simultaneous, though, therefore, there is never, in this case, any *real* composition, like that illustrated in Fig. 49, neutralizing, *in fact*, the gyratory movements, yet the compensatory effects follow each other with such rapidity that, to our instruments and our powers of vision, they are as if they did not exist. Common light cannot, therefore, be polarized by total reflection. Moreover, common light need not, in any case, be supposed to be made up strictly of plane vibrations. It is only necessary to suppose its gyratory movements to be as impartially distributed as we have heretofore presumed its plane vibrations to be.

If, however, we suppose a surface which is not a surface of total reflection to possess the power of accelerating or retarding one of the rectangular components of the incident molecular velocity over the other, then the reflected light will, in general, be elliptically polarized. For the two components are never equally reflected except in total reflection. Now there are very few substances capable of reflecting light which do not possess this power, and, accordingly, elliptical polarization is the effect most usually attending reflection. As has been elsewhere stated, it is only those substances whose indexes of refraction are very near to 1.414 that produce a kind of polarization that is sensibly plane.

This subject has been very thoroughly investigated, theoretically, by Mr. Cauchy, and experimentally by Mr. Jamin, with results mutually corroborative of each other. In order to clearly understand the experimental methods employed, let us observe that, if a plane polarized wave be supposed to be decomposed into two rectangular component undulations, the curves representing these components will cross the common intersection of their planes in the same points. These crossing points may be called *nodes*. In the case supposed, the nodes of the two components are coincident. The effect of reflection is, in general, (*i. e.*, in all cases except those which have the index of refraction just now mentioned,) to throw the nodes of the components out of coincidence. And the original plane polarization will be restored by *bringing back* the nodes to their original coincidence, or by *increasing* the discrepancy between them by repeated reflections until it amounts to half an undulation. In the first case, if the reflection were total, as at the second surface of glass, the plane of polarization, after restoration, would be unaltered. In the second case, on similar suppositions, it would be changed 90°. When the reflection is not total, the resultant plane, after the reunion of the displaced nodes, will differ from the original plane in consequence of the unequal losses experienced by the two components in reflection.

Both those methods have been employed in experimentally determining the dislocating effects of different media, at different incidences, upon the rectangular components of plane polarized light. Many of Mr. Jamin's more recent and elaborate researches have been made by the method first mentioned. The essential part of his apparatus consisted in a double prism, formed of two equal acute wedges of rock crystal, cut parallel to the axis, and combined as shown in the figure annexed. The wedge ABC has the edge AB *parallel* to the axis, and the wedge ADC has the edge DC *perpendicular* to the axis. The *surfaces* AB and DC are both *parallel* to the axis. If a plane polarized ray, PQ, pass at right angles to AB through the middle of this system, where the wedges are equally thick, it will remain plane polarized, and the position of its plane will remain unaltered whatever be the azimuth of incidence; the dislocating effect of the double refraction of one of the wedges being compensated by an equal and opposite effect of the other. But if the system be moved to the right or to the left, the two opposite effects will no longer be equal. This being a positive crystal, the component of molecular movement parallel to AB will be in retardation of that perpendicular to AB, for the position of the ray, $P'Q'$, and will be in advance for the position $P''Q''$. But if the ray has been already dislocated by reflection, some part of the prism may be found which will produce an equal and contrary effect, so as to restore the plane polarization All that is necessary, then, to make this instrument a *measure* of the dislocation is to connect it with a scale and a screw movement, and to determine the value of the scale divisions. This last determination is easy, since a run which converts a plane polarized ray into a ray plane polarized with a difference of azimuth of 90°, is equivalent to half an undulation. Mr. Jamin's apparatus accomplished this change in twelve complete turns of the screw. Smaller divisions were measured by the graduation of the screw-head, of which there were two hundred divisions to the revolution. The delicacy of the contrivance may be appreciated from this statement. It is known as Mr. Jamin's "compensator."

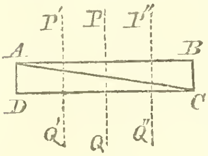

Fig. 50

It would occupy too much space to attempt here to give a full account of Mr. Jamin's interesting researches, or of the methods employed by him auxiliary to that just described. The most important results of his investigations are the following:

Nearly all transparent bodies produce, by reflection from their surfaces, a difference of phase between the component waves polarized in the two principal planes. All whose indexes of refraction exceed 1.414, advance the phase of the component polarized in the plane of incidence. All those whose indexes are below that value retard the phase of the same component.

The difference of phase augments with the incidence from $\frac{1}{2}\lambda$ at 0° to λ at 90°, and becomes $\frac{3}{4}\lambda$ at the polarizing angle. The variations are slow and almost insensible for some distance from either 0° or 90°. They usually become sensible near the polarizing angle. The limits are nearer together as the polarization under that angle is greater.

Beyond these limits the polarization is plane, but imperfect. Within them it is elliptic.

The two limits are nearer to each other as the index of refraction approaches

1.414. They *unite*, for substances whose index has that exact value. Mr. Jamin found but two substances in which this condition is fulfilled. They were a specimen of menilite, and a crystal of alum cut perpendicularly to the axis of the octahedron. Water and glass, which under ordinary light appear to polarize perfectly, are easily seen not to do so under the strong light of the sun.

In the cases in which the index has the particular value just mentioned, the advance of phase at the polarizing angle is "brusque" from $\frac{1}{2}\lambda$ to λ. This is very nearly the case with water and glass.

Reflection from the surface of metals always produces elliptical polarization. The advance of phase is progressive from incidence 0° to incidence 90°. There are, however, very large differences between metals in this respect.

§ IX. ROTATORY POLARIZATION.

We are now perhaps prepared to understand the reason of the rotation of the plane of polarization of a ray transmitted along the axis of a crystal of quartz. We have seen that Fresnel, by an ingenious combination of prisms, succeeded in demonstrating the existence within the crystal of two circularly polarized rays, gyrating in opposite directions. And we have seen that the resultant effect of two opposite gyrations is to produce a movement in a plane. The gyratory movements within the crystal are then not *actual* but *virtual*—in other words there are forces constantly tending to produce these gyrations, which hold each other in equilibrio or at least nearly so. We must consider these forces as successively traversing all azimuths within the length of each undulation. If the wave were of the same length for both gyrations, the forces being presumed equal, the molecular movement would be constantly rectilinear, and the plane of polarization would not change. But, as the plane does in fact change, we are led to infer that the undulation lengths for the two rays are *not* equal. The annexed figure may serve to illustrate the mutual action of these rays. Suppose MADB to be the orbit in which a force P tends to urge a molecule, M, to revolve around the centre, C, to which it is drawn by the force MC. Suppose the equal force Q to urge the same molecule to describe the same orbit in the opposite direction. These forces holding each other in equilibrio, the molecule will follow the direction of the third force, MC.

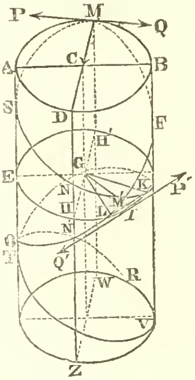

Fig. 51.

Now suppose the force Q suspended, the molecule will take the direction of the circle ADB, and will continue to revolve in it so long as the force P (supposed always tangential) continues to act. But its movement will impart to the molecule next below it a similar motion, and that to the next, and so on; so that, as these successive molecules take up their movement later and later, there will be a series in different degrees of advancement in their several circles, forming a spiral; and when the molecule M shall have returned to its original position, the series will occupy a position like the curve MFLN'OR. If now P be supposed to be in turn suspended, while the force Q continues to act, the effect of Q will be to produce a contrary spiral, which may be represented by MSKTV. If MD be a diameter of the circle MADB, drawn from M, and DNHN' be a line parallel to the axis CG of the cylindrical surface which is the locus of the spirals, then, if the undulation lengths are the same for both movements, the two spirals will intersect DH in the same point, the intersections marking the completion of a half undulation for each. But if these lengths be unequal, the intersections with DH will take place at different points, as N and N'.

Let now a plane intersect the cylinder at any distance below MADB, as at

E, parallel to MADB. It is conceivable that this plane may be made to pass through the point where the spirals intersect each other. If I mark the point of intersection, and we draw the tangents IP′ and IQ′ in the plane of the circle EHI, then there will be a molecule at the point I which will be in the circumstances of the molecule in Fig. 49 at the point M—that is to say, solicited by three forces, of which two, IP′ and IQ′, are equal and opposite, and the third is directed in the line IG toward the centre. The molecule will, therefore, move in this line, and not in a circle; and if the plane of the circle EHIH′ be the bounding surface of the crystal, or the surface of emergence of the light, IG will mark the azimuth of the molecular movement of the emergent ray.

But if the plane of EHIH′ do not pass through the point of intersection of the spirals, it must cut each spiral in a different point. The figure is drawn to represent this more general case, the points of intersection with the spirals being severally L and K. By joining LK, and drawing the radius GI perpendicular to it, GI will bisect the angle LGK, and M′, at the intersection of GI and LK, will be the position of the molecule in the plane EHLIK, which, if the tangential force P only were acting, would be at L, and if the tangential force Q only were acting, would be at K. The tangential forces acting at the moment on this molecule will not be represented by IP′ and IQ′, but by tangents at K and L, like RP′ and RQ′ in Fig. 49, in which figure the position of the molecule M′ corresponds to that marked by the same letter in the present figure; but in that figure the resultant of the tangential forces is RC, directed to the centre, and in this it will, in like manner, be IG.

Now, as DH, the distance between the planes ADB and EHI, is a larger part of the length of an entire turn of the spiral MSNK than of the spiral MFLN′, the line GI will fall on the right of GH, the position it would occupy if the two undulations were equal in length. We may therefore say, as before, that if the plane EHI were the surface of emergence of a ray from a crystal, in which it had been subject to the action of the forces supposed, its plane of polarization, GI, would be turned toward the right from its original azimuth. The plane of polarization turns, therefore, in the direction of the *winding* of the closest spiral, or of the ray of *shortest undulation;* but it turns in the direction of the *gyration* of the ray of *longest undulation.*

This rotation of the plane thus demonstrates that the two rays advance with unequal velocities in the axis of quartz—a remarkable fact which is not true of any crystal which produces plane polarization only.

It also enables us to determine the relative velocities, or to ascertain the index of rotatory polarization. For since GI bisects the angle between the points K and L, which mark the relative degrees of advancement of the two rays in their respective rotations, if we take a thickness, θ, which produces a rotation of 90°, we know that the difference of phase is then one half an undulation. If λ denote the length of the longer undulation, and λ' that of the shorter, then—

$$\theta = m\lambda = (m+\tfrac{1}{2})\lambda'; \quad \text{or } \frac{\lambda}{\lambda'} = \frac{m+\frac{1}{2}}{m} = \frac{2m+1}{2m}.$$

As $\frac{\theta}{\lambda} = m$, and λ may be determined by experiments on refraction, the value of m is known when θ is measured. By pursuing this method, Mr. Babinet found the value of $\frac{\lambda}{\lambda'} = 1.00003$; a value which, small as it is, is the largest known for rotatory polarization.

When light is transmitted through quartz at right angles to the axis, the emergent rays are plane polarized. Mr. Airy has proved that, for directions oblique to the axis, the polarization is elliptic, the ellipticity increasing from the direction perpendicular up to the direction parallel to the axis, where it becomes circular.

It is difficult to conceive exactly the physical action by which rotatory polarization is produced. But there is no difficulty in imagining such a decomposition of the molecular movements in a plane polarized ray, as shall represent the relations which exist *after the rotatory polarization has been established.* We have seen that when a plane undulation has been resolved into two equal rectangular components, if the *nodal* joints of these components become dislocated by a quarter of an undulation, the resultant will be a movement in a circular orbit. We have also seen that when the left-hand component is advanced by this amount, the motion becomes dextrogyre; and when the right hand component is advanced, it becomes lævogyre. In order, then, to explain the co-existence of two opposite circular polarizations, we must suppose two sets of equal rectangular components dislocated in these two opposite ways. This was the hypothesis of Fresnel.

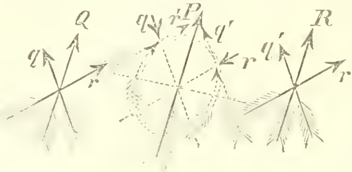

Fig. 52.

In order to facilitate the conception, suppose the arrow P to represent, in quantity and direction, the molecular movement, at a given instant, in the original plane polarized wave. Imagine it to be a resultant of two other waves, Q and R, one in front of it, and the other behind it, each at the distance of one-eighth of an undulation. These will then be a quarter of an undulation distant from each other. Let Q and R be again resolved, each into two equal rectangular components, in azimuths $+45°$ and $-45°$; Q into q and r; and R into q' and r'. Consider all these four component movements, at the instant supposed, and in the positions represented, to be at their maximum of velocity, in the direction of the several arrows denoting them. Then, if we consider the relative stages of advancement, or phases of movement, of the pair q and r, in respect to q' and r', when both are referred to a common plane, it will be seen that the latter, though most advanced in position, are least advanced in phase For, if we conceive the curves of these waves to be drawn, the *ascending node* of q' will be found in the plane of qr, and the *descending node* of q in the plane of $q'r'$. Hence, at the point where the wave q' *begins*, the wave q is *one-quarter advanced.*

We have, then, two pairs of plane undulations, q and r' and r and q', severally normal to each other, and with nodes dislocated to the extent of one-quarter of an undulation; q and r being the members of the pairs which are most advanced in phase. In the case of the pair q and r', the right-hand component being that which is most advanced, the resultant movement is a revolution sinistrorsum In the case of r and q' the resultant will be a revolution dextrorsum.

The values of these several components are determined from the general equation following, which is simply equation [3,] with the symbols changed:

$$P^2 = Q^2 + R^2 + 2QR\cos\theta.$$

By hypothesis $R = Q$, and $\theta = 90°$. Hence $P^2 = 2Q^2$, and $Q = \frac{P}{\sqrt{2}}$.

$$\text{Again, } Q^2 = q^2 + r^2 = 2q^2. \quad \text{And } q = \frac{Q}{\sqrt{2}} = \frac{P}{\sqrt{2}\sqrt{2}} = \tfrac{1}{2}P.$$

$$\text{Also, } R^2 = q'^2 + r'^2 = 2q'^2. \quad \text{And } q' = \frac{R}{\sqrt{2}} = \frac{P}{\sqrt{2}\sqrt{2}} = \tfrac{1}{2}P.$$

It appears, therefore, that the molecular velocity in each of the component waves q, r, q', r', is equal to one-half that of P, as it should be, in order that the sum of their living forces may be equal to the living force of the primitive wave.

Since the two circularly polarized rays in the axis of quartz have unequal velocities, there must be certain thicknesses of the crystal, which will make the difference of their paths equal to half an undulation, or to an odd multiple of half an undulation. It might be supposed, therefore, that in such cases interference would occur, so that the crystal should naturally exhibit colors. The fact is not so; and if we consider the conditions we shall discover without difficulty the reasons why it is not. If, in Fig. 52, the two components q and r' of one of the circularly polarized rays be supposed to advance or gain upon q' and r, the distance between q and q' will diminish until q passes q', and the distance between r and r' will constantly increase. If c represent the amount of advance, the distance of r' from the plane of qr in the figure will be $c+\frac{1}{4}\lambda$, and the distance of q from the same plane will be c only. Now, since q and q' are equal, the resultant to which they are equivalent will fall half way between them, (page 167.) The same is true in regard to the resultant of r and r'. But the point half way between q and q' will be situated at a distance from the plane qr which is the mean of the distances of q and q'; thus—

$$\left.\begin{array}{l}\text{Distance of } q=c\\ \text{Distance of } q'=\frac{1}{4}\lambda\end{array}\right\}\text{Mean}=\tfrac{1}{2}(c+\tfrac{1}{4}\lambda)=\tfrac{1}{2}c+\tfrac{1}{8}\lambda.$$

In like manner—

$$\left.\begin{array}{l}\text{Distance of } r=0\\ \text{Distance of } r'=c+\frac{1}{4}\lambda\end{array}\right\}\text{Mean}=\tfrac{1}{2}(c+\tfrac{1}{4}\lambda)=\tfrac{1}{2}c+\tfrac{1}{8}\lambda.$$

That is to say, the resultants of the components in each plane always coincide in *position*. We have next to consider their *values*. From the statements above it appears that the *distance between* r and r' is the entire distance of r' from the plane qr—that is, $=c+\frac{1}{4}\lambda$. And the distance between q and q' is the *difference* of the distances of q and q' severally from the same plane qr—that is, $c-\frac{1}{4}\lambda$.

In the general equation for the resultant of two waves whose molecular movements are in the same plane, (equation [3.]) we must accordingly introduce the following values of θ:

$$\text{For } q \text{ and } q',\ \theta=2\pi\left(\frac{c}{\lambda}-\frac{1}{4}\right).$$

$$\text{For } r \text{ and } r',\ \theta=2\pi\left(\frac{c}{\lambda}+\frac{1}{4}\right).$$

Then putting p for the first resultant, and p' for the second, and remembering that $q=q'=r=r'$, the equations become,

$$p^2=q^2+q'^2+2qq'\cos 2\pi\left(\frac{c}{\lambda}-\frac{1}{4}\right)=q^2+q'^2+2qq'\sin 2\pi\frac{c}{\lambda}=2q^2\left(1+\sin 2\pi\frac{c}{\lambda}\right).$$

$$p'^2=r^2+r'^2+2rr'\cos 2\pi\left(\frac{c}{\lambda}+\frac{1}{4}\right)=r^2+r'^2-2rr'\sin 2\pi\frac{c}{\lambda}=2r^2\left(1-\sin 2\pi\frac{c}{\lambda}\right).$$

Whence $p^2+p'^2=2q^2+2r^2=constant$. And the intensity of the light is invariable.

By considering the foregoing values of p^2 and p'^2, however, it will be seen that they are severally variable, though their variations are always compensatory. If c be any number of half undulations—

$$\text{Sin}2\pi\frac{c}{\lambda}=0;\ \text{and } p^2=p'^2.$$

But if c be an odd number of quarter undulations,

$$\text{Sin}2\pi\frac{c}{\lambda}=1 \text{ or} -1;\ \text{and either } p^2=0, \text{ or } p'^2=0.$$

Both p^2 and p'^2, therefore, pass through a succession of maxima and minima, the increments of the one corresponding always in value to the simultaneous decrements of the other, and each becoming periodically zero. It accordingly follows that if two circularly polarized rays, whose molecular gyrations are performed in opposite directions, be thrown together in a nearly common direction, and observed by means of an analyzer, fringes of interference may be detected in the two principal azimuths, those in one of these azimuths being complementary to those in the other.

Mr. Babinet made this observation, employing Arago's prism to produce interfering pencils of plane polarized light; which pencils he polarized circularly by means of "quarter-wave laminæ" of mica placed in their paths. The gyrations were made of opposite kinds in the two pencils, by placing the two laminæ so that their principal planes should be at right angles to each other. As an analyzer, he employed a doubly refracting prism. The fringes immediately appeared; thus furnishing a very interesting experimental corroboration of a theoretic anticipation.

In the year 1845 Mr. Faraday communicated to the Royal Society of London a very remarkable discovery which he had made, of the apparent influence of *magnetism* upon light. If any homogeneous transparent body be placed under the influence of a powerful electro-magnet, it will possess the property, while the magnetism is maintained, of turning the plane of a ray of polarized light traversing it in the direction of a line joining the magnetic poles, in the same manner as such a ray is turned by quartz, or by liquids possessing the property of rotatory polarization. Mr. Faraday was at first disposed to attribute this effect to a direct action of magnetism on light, but that idea is now abandoned; and the received opinion on the subject supposes that the molecules of the medium undergo some modification during the continuance of the magnetic influence, which assimilates their action upon the ether to that of substances which possess permanently the power of rotatory polarization. The direction in which the plane of polarization was turned in these experiments depended on the direction of the electric currents. When the currents were reversed, the rotation was reversed also. It is impossible in this place to do more than to allude to this interesting discovery.

§ X. CHROMATICS OF POLARIZED LIGHT.

We will now proceed to apply the principles we have been considering to the explanation of the colors produced in doubly refracting substances by polarized light. We have seen that double refraction consists in the generation of two waves of unequal velocity and of dissimilar form in the doubly refracting body. We have also seen that the molecular movements in the two waves are at right angles to each other. In consequence of the inequality of velocity the two rays into which a doubly refracting body divides a single incident ray may emerge from a surface opposite and parallel to the surface of incidence in different phases. If not entirely separated by the deviation of their paths, they may thus, *so far as phase is concerned*, be in condition to interfere. But we have seen that interference is impossible between waves whose molecular movements are perpendicular to each other. If, then, by any contrivance, we can turn the planes of polarization of two rays which, by double refraction, have been made to differ in their length of path by half an undulation, or by any odd number of half undulations, so that these planes shall coincide, interference will be produced. It is this which is done in the arrangements which have been described, by which the gorgeous colors first observed by Arago in plates of doubly refracting crystals, are made to appear. In the first place, the lamina must be doubly refracting, in order that there may be two rays. In the second place, it must be thin, that the difference of length of path may be small. In the third place, the original light must be polarized, otherwise there will be two systems of interferences compensating each other, and obliterating each other's effects.

In the fourth place, the principal plane of the lamina should, in order to produce the most complete interference, be at an azimuth of 45° to the plane of original polarization—the two rays being, in this position of the crystal, exactly equal to each other. In the fifth place, we must observe the phenomena by means of an analyzer, which allows only the light polarized in a single plane to come to the eye, or which, like a doubly refracting prism, separates the emergent light which is polarized in one plane from that which is polarized in the transverse plane; otherwise in this case again we shall have the blended effects of two compensatory interferences. Finally, the principal plane of the analyzer should, in order to produce the best effect, be at an azimuth of 45° from the principal plane of the lamina. The necessity of this condition may be readily deduced from the law of Malus. The annexed figure may illustrate the changes which take place in the passage of the ray through the system. If the arrow, P, represent the direction of an elementary molecular movement of the original polarized ray, this movement will be resolved in the lamina into two movements at right angles to each other, and each inclined 45° to P, as shown by the arrows R_o (the ordinary ray) and R_e (the extraordinary.) Suppose these rays to emerge, without difference of path, from the lamina, and to be received upon a crystal of Iceland spar whose principal plane coincides with the direction of P. Then R_o will be resolved into R_{oo} and R_{oe}; and R_e will be resolved into R_{eo} and R_{ee}. R_{oo} and R_{eo} will conspire, and R_{oe} and R_{ee} will conflict. The first pair, on the supposition we have made, will only be effective. The second will destroy each other.

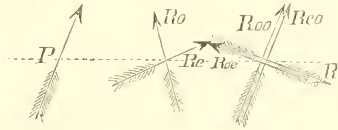

Fig 53.

But R_o is retarded (in the case of a negative crystal) behind R_e. Let the retardation amount to an odd number of half undulations, and the arrangement of the illustrative arrows will be what is seen here. In this case the pair in the principal plane of the analyzer conflict and are destroyed; while the pair in the transverse plane conspire. And this represents what actually occurs, when the thickness of the lamina is such as to produce exactly the amount of retardation here supposed.

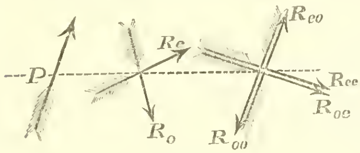

Fig. 54.

But since the undulations of the component rays of white light are unequal in length, the retardation which will be sufficient to suppress one color, will not entirely suppress the others. The ray $R_{oo}+R_{eo}$ will not therefore be wholly extinguished, but will exhibit a color which will be the resultant of the unsuppressed tints. Moreover, the retardation which produces perfect coincidence in the ray $R_{oe}+R_{ee}$ for one color, will not do the same for the rest. There will therefore be a color in this ray also, in which the tint suppressed in the other plane will be predominant. It is to be observed, however, that when the plate is so extremely thin that the retardation suffices only to produce a difference of path equal to a single half undulation of the mean ray of the spectrum, or less than this, no color will appear. And the reason will be obvious, if we consider that, though the undulations of the different colors are unequal, their inequalities as compared with the total length of the mean undulation are not great. The undulations of the middle violet, middle green, and middle red—the extreme and mean colors of the spectrum—are approximately in the ratio of the numbers 17, 21, and 26. A retardation of half an undulation of the green would therefore be about the fourth part more than a half undulation of the violet; and a fifth part less than a half undulation of the red. But a retardation of *five* half undulations of the green would be not far from *six* half undulations of the violet or *four* of the red. The violet and the red, therefore, having in this case lost (approximately) an *even* number of half undulations, will comport themselves, on being restored to the original plane of polarization, as if they had lost nothing at all; while the green, which has lost an *odd* number, will interfere and be extinguished. The tint observed in this plane will accordingly be the resultant of red and violet; which, on account of the comparative feebleness of the violet, will be but a slightly modified red. In the transverse plane, however, the red and violet will interfere and be extinguished, while the green components will reinforce each other, and produce their characteristic tint.

It will be seen that the planes of polarization of the pairs of rays which produce the complementary effects we have been speaking of, undergo two successive movements. The first movement is *from* the original plane of polarization. The second movement is, for one pair of rays, similar to this, and for the other, opposite. The opposite movements restore the pair of rays which they affect, back to the original plane of polarization: the similar movements carry the other pair of rays into the transverse plane.

If there were no difference of path introduced in the passage of the lamina, or in the case that the difference of path produced were always an *even* number of half undulations, two movements in contrary directions would simply restore the ray to its original condition, and produce no interference; while two movements in the same direction would extinguish it entirely. But if an *odd* number of half undulations has in any case been lost, two successive *contrary* movements will extinguish the ray, and two *similar* ones will restore its original condition. A single half undulation of the mean ray of the spectrum lost, will produce a total, or almost total, extinction of the light, after two contrary movements; and will produce sensibly white light after two similar movements. Plates so thin as to produce a difference of path less than this, will fail to extinguish the light in either plane; but, as the thickness goes on diminishing, the original plane will gain and the transverse plane will lose; until, when the thickness is zero, the light will be entirely restored in the first, and entirely lost in the second.

It is common to speak of polarized light which has passed in this manner through a thin crystalline lamina, as having undergone *depolarization* in the lamina: an expression which seems to imply that it is restored to the condition of common light. This, however, is not true. There is one analogy between the cases, which consists in the fact that the vibrations of common light, when resolved into components parallel to two planes passing through the direction of the ray and normal to each other, are equivalent to those of the two rays into which the one original polarized ray is divided by the lamina. But the great dissimilarity of physical condition between the two is evidenced by the fact that in the one case the analyzer produces colors, while in the other it does not. There is a particular thickness of the lamina which produces something more resembling depolarization. It is that at which one ray is retarded behind the other a single quarter of an undulation. In this case the analyzer finds an equal amount of light in both planes, and in fact in all planes; so that, so far as this test is concerned, the light is truly depolarized. But we have already learned that this amount of dislocation of the rectangular components of molecular movement in a plane polarized ray—the components being equal—produces circular polarization. And in fact, the most convenient mode of producing circularly polarized light is to employ for the purpose what is called a "quarter-wave lamina." Such a lamina will convert a plane polarized ray, incident upon it in azimuth 45° to its principal section, into a circularly polarized ray.

When the lamina which is the subject of experiment is so thick that the difference of path between the two rays amounts to many half undulations, then no color can be totally extinguished in either plane. For it must be remembered that each color occupies a considerable space in the spectrum, and therefore has undulations belonging to it of many different lengths. The differences may be slight, but slight values many times repeated become large values at last; so that two red rays whose phases are for several undulations sufficiently unlike to conflict, may, after a larger number, be nearly enough alike to conspire. If the numbers 21 and 22 represent the lengths of two undulations of green, after a retardation of eleven times the length of the former, the latter will have fallen half an undulation behind it. Thus, after a certain amount of retardation is reached, there will be found undulations of all colors impartially distributed through all varieties of phase, and the chromatic phenomena above described will cease.

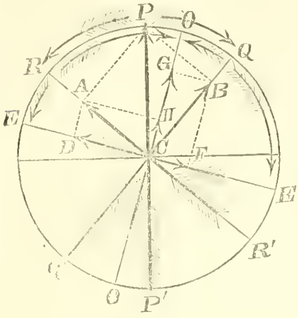

Fig. 55.

A general expression for all these phenomena may be found as follows: Let PP′ be the plane of polarization of the original ray; QQ′ the principal plane of the lamina; RR′ the conjugate plane; OO′ the principal plane of the analyzer, which we will suppose to be a doubly refracting prism or rhomb of Iceland spar; and EE′ its conjugate plane. Draw PA, PB, perpendicular to QQ′ and RR′; BF, BG perpendicular to EE′, OO′; and AD, AH perpendicular to EE′, OO′. Then if CP represent the velocity of molecular movement in the original ray, CA and CB will represent its equivalent components in the directions R R and Q′Q. If these components be farther decomposed in the directions OO′ and EE′, we shall have the original velocity CP represented by the four elements CG, CH in the principal plane, and CD, CF in the conjugate plane of the analyzer.

Represent the original velocity CP by V. Put the angle PCQ$=\alpha$, and the angle PCO$=\beta$. Then the angle OCQ will be $\alpha-\beta$. The triangles PCA, PCB give $CA=V\sin\alpha$; $CB=V\cos\alpha$. And the triangles ACD=ACH, and BCF=BCG, give $CD=V\sin\alpha\cos(\alpha-\beta)$; $CF=V\cos\alpha\sin(\alpha-\beta)$; $CG=V\cos\alpha\cos(\alpha-\beta)$; $CH=V\sin\alpha\sin(\alpha-\beta)$. Then, to find the resultant of CG, CH, the molecular velocities of the two rays emergent in the plane OO′—that is, of the emergent ordinary ray—we recur to the general equation—

$$A^2=a^2+a'^2+2aa'\cos\theta.$$

in which we must substitute for a and a' the values of CG and CH given above; and for θ the amount of retardation in phase of one of the rays behind the other in passing the lamina, which, if h represent the actual difference in length of path, may be represented by $2\pi\frac{h}{\lambda}$. The equation just stated may be conveniently transformed by adding $2aa'-2aa'$ to the second member, when it will become—

$$A^2=a^2+a'^2+2aa'-2aa'+2aa'\cos\theta.$$

$$\text{Or }A^2=(a+a')^2-2aa'(1-\cos\theta)=(a+a')^2-4aa'\sin^2\tfrac{1}{2}\theta.$$

Substituting now the values of a, a', and θ, we obtain—

$$A^2=V^2\left[[\cos\alpha\cos(\alpha-\beta)+\sin\alpha\sin(\alpha-\beta)]^2-4\sin\alpha\cos\alpha\sin(\alpha-\beta)\cos(\alpha-\beta)\sin^2\pi\frac{h}{\lambda}\right].$$

Which may be reduced to the following entirely equivalent forms:

$$A^2=V^2\left[\cos^2\beta+[\cos^2(2\alpha-\beta)-\cos^2\beta]\sin^2\pi\frac{h}{\lambda}\right]. \qquad [37.]$$

$$A^2=V^2\left[\cos^2\beta-[\sin^2(2\alpha-\beta)-\sin^2\beta]\sin^2\pi\frac{h}{\lambda}\right]. \qquad [38.]$$

And by pursuing a similar course with the values of the components CD, CF, of the extraordinary ray, we shall obtain for its resultant intensity the two values, also equivalent to each other—

$$A'^2 = V^2 \left[\sin^2\beta + [\sin^2(2\alpha-\beta) - \sin^2\beta]\sin^2\pi\frac{h}{\lambda} \right] \qquad [39.]$$

$$A'^2 = V^2 \left[\sin^2\beta - [\cos^2(2\alpha-\beta) - \cos^2\beta]\sin^2\pi\frac{h}{\lambda} \right] \qquad [40.]$$

The intensity of the light in either plane is thus expressed in a formula of two terms, one of which is affected by interference, while the other is not. It is from the second that the colorific effects proceed—directly, when this term is positive relatively to the first, and indirectly when it is relatively negative. This term may therefore be called the chromatic term, and the other the achromatic.

In considering these equations we observe, first, that if we add either value of A^2 to either value of A'^2, the chromatic term disappears. The colors are therefore complementary; and if blended, the resultant is white.

Secondly, since $A^2+A'^2=V^2(\cos^2\beta+\sin^2\beta)=V^2$, the sum of the two intensities is equal to the intensity of the original ray; as it ought to be on the principle of the preservation of living forces.

Thirdly, the chromatic effects being dependent on the factor $\sin^2\pi\frac{h}{\lambda}$ for their *character*, will be dependent not only upon this factor, but also on the coefficient, $\sin^2(2\alpha-\beta)-\sin^2\beta$, or $\cos^2(2\alpha-\beta)-\cos^2\beta$, for their *quantity*. Their greatest values will hence occur when this coefficient is maximum. There being two variables, α and β, if we make the first constant, we shall find maxima when $\cos 2(\alpha-\beta)=0$, or $\cos 2(\beta-\alpha)=0$. This gives a series of values for $2(\alpha-\beta)$ or $2(\beta-\alpha)$, which are 90° and its odd multiples. It is sufficient to consider the first, which gives $\alpha-\beta=45°$, or $\beta-\alpha=45°$; from which $\alpha=\beta+45°$, or $\alpha=\beta-45°$. For the higher values we need only replace 45° by the numbers 135°, 225°, 315°, &c., successively. These values substituted in the coefficient all give the same result; hence all the maxima dependent on β alone are equal; and it is obvious that they are independent of α, since α is not a function of β. If we find, then, the maximum with reference to α, and substitute in the resulting expression, instead of β, its value $=\alpha\pm45°$, as obtained above, we shall have the maximum of the maxima, or the azimuths of QQ′ and OO′, in which the chromatic effects are the most brilliant possible. The solution gives $\sin(4\alpha-2\beta)=0$. Hence $4\alpha-2\beta=0$, or 180°, or 360°, &c. Contenting ourselves with the first value, and substituting for β, we have $4\alpha-2\alpha\pm90°=0$, or $\alpha=\mp45°$. And as $\beta=\alpha\pm45°$, we conclude that the arrangement in which the colors will be most brilliant is that in which the principal plane of the lamina is inclined 45° to the plane of polarization of the incident light, and in which the principal plane of the analyzer is in azimuth 0° or 90°—theoretic conclusions already anticipated by experiment.

Fourth, attending to the first of the formulæ for A^2 and A'^2 we see that the chromatic term in each is symbolically positive. If the term, therefore, is essentially positive in itself, the color of the ray is the color which the interference expressed by that term would produce, diluted with such an amount of white light as is expressed by the achromatic term. When the chromatic term in the same formulæ becomes essentially negative, the color will be that which is left by *subtracting* its own color from the amount of white in the achromatic term. That the subtraction will be possible—that is to say, that, when the chromatic term is negative, the achromatic term will always be the greater—will be evident on inspection. For examining the coefficient of the chromatic term within the bracket, it will be seen to consist of a positive and negative element, which elements, being squares, have their essential the same as their written signs; but the negative element of this coefficient is the same as the positive achromatic term. Hence the entire coefficient can never be greater than the achromatic term; and can only be equal to it in the single case when cosine or sine $(2\alpha-\beta)=0$. But the chromatic term has another factor, $\sin^2\pi\frac{h}{\lambda}$, which is always less than unity, except when h is an integral odd number of half undulations. This, therefore, usually still further reduces the value; so that neither of the expressions for intensity can ever become negative.

Fifth, if β remain constant, the value of the chromatic term will vary with α, and may even become zero when $\alpha=\beta$. The force of the color will therefore undergo corresponding variations; and all color will disappear in the case just mentioned. The same also will be true when $\alpha=\beta+90°$, $\beta+180°$, &c., &c. But though, in these positions of the lamina white light only is seen, the color reappears for values of α intermediate between β and $\beta+90°$, $\beta+90°$ and $\beta+180°$, &c.; and this color is the same as before, since the sign of the chromatic term does not change. When α is constant and β varies, the color in like manner rises and descends in brilliancy, having a minimum $=0$, at the values $\beta=\alpha$, $\beta=\alpha+90°$ and $\beta=\alpha+180°$, &c. But as, in passing each of these successive values, the coefficient of the chromatic term, as is evident on inspection, changes its essential sign from positive to negative, or the contrary, the tints observed in the successive quadrants will be complementary to each other.

Sixth, when $\alpha=0°$, $90°$, $180°$, &c., the chromatic term disappears for every value of β. In this case the light remains white throughout the entire revolution of the analyzer, and one or the other of the achromatic terms disappears, for the azimuths $\beta=0°$, $\beta=90°$, &c.

Seventh, if we suppose the lamina and the analyzer both to remain stationary while the polarizer revolves, we shall see that the chromatic term changes its sign in the course of every quarter revolution. For example, since the change of plane of original polarization affects the azimuths of the lamina and of the analyzer equally, if we suppose a revolution of 90° in the negative direction, the coefficient $\cos^2(2\alpha-\beta)-\cos^2\beta$ becomes $\cos^2(2\alpha+180°-90°-\beta)-\cos^2(90°+\beta)=\cos^2(90°+2\alpha-\beta)-\cos^2(90°+\beta)=\sin^2(2\alpha-\beta)-\sin^2\beta$. But, by reference to the two equivalent values of A^2, [37.][38.] we see that $\cos^2(2\alpha-\beta)-\cos^2\beta=-(\sin^2(2\alpha-\beta)-\sin^2\beta.)$ Hence, in the rotation of the plane of polarization through an arc of 90°, the coefficient of the chromatic term passes from positive to negative or the contrary. If we suppose a revolution of 180° still in the negative direction, we shall find the sign once more the same as in the original expression. Thus, $\cos^2(2\alpha-\beta)-\cos^2\beta$ becomes $\cos^2(2\alpha+360°-180°-\beta)-\cos^2(180°+\beta)=\cos^2(180°+2\alpha-\beta)-\cos^2(180°+\beta)=\cos^2(2\alpha-\beta)-\cos^2\beta$. If we suppose the rotation in the opposite direction, the alternations are similar.

It is, hence, manifest that unless the light employed in these experiments be originally polarized, no chromatic phenomena will make their appearance. For unpolarized light being made up of successive undulations impartially distributed through all azimuths, those which are embraced within any one quadrant will neutralize the effects of those within the adjacent one, the complementary colors produced by each similarly situated pair succeeding each other with such rapidity as to blend their effects upon the retina.

Eighth, if we consider the factor $\sin^2\pi\frac{h}{\lambda}$, we shall see that, when $h=\frac{1}{2}\lambda$, $\frac{3}{2}\lambda$, $\frac{5}{2}\lambda$, &c., the value of this factor is unity, which is its greatest possible value. The chromatic effect is, therefore, greatest when the retardation of one ray upon the other is an odd number of half undulations. If, in this case, $\beta=2\alpha$, then $A^2=V^2$, and $A'^2=0$. If $\beta=2\alpha+90°$, $A^2=0$, and $A'^2=V^2$. Supposing, therefore, the light homogeneous, the apparent planes of polarization of the emergent rays will be in azimuths $2a$ and $2a+90°$.

Ninth, if $h=\lambda, 2\lambda, 3\lambda$, &c., $\sin^2\pi\frac{h}{\lambda}=0$; and the equations become simplified to the forms

$$A^2=V^2\cos\beta\text{; and }A'^2=V^2\sin^2\beta,$$

which are a reproduction of the law of Malus. The interposition of the lamina produces, therefore, no apparent change in the plane of original polarization.

Tenth, if $h=\frac{1}{4}\lambda, \frac{3}{4}\lambda, \frac{5}{4}\lambda$, &c.—that is to say, an odd number of quarter undulations—$\sin^2\pi\frac{h}{\lambda}$ becomes $\sin^2 45°=\frac{1}{2}$. If, then, $a=45°$, the equations become—

$$A^2=V^2[\cos^2\beta+\tfrac{1}{2}(\sin^2\beta-\cos^2\beta)]=\tfrac{1}{2}V^2(\cos^2\beta+\sin^2\beta)=\tfrac{1}{2}V^2.$$
$$A'^2=V^2[\sin^2\beta+\tfrac{1}{2}(\cos^2\beta-\sin^2\beta)]=\tfrac{1}{2}V^2(\sin^2\beta+\cos^2\beta=\tfrac{1}{2}V^2.$$

This result, being independent of the value of β, indicates an apparent depolarization of the light. But in fact, it is the case of circular polarization, which we have already considered. It will be seen that it is necessary to the production of the effect that a should be 45°, in order that the two normally polarized rays may be equal to each other. In any other azimuth of the lamina, the polarization will be elliptical.

If a lamina of crystal cut *at right angles to the axis* be employed, then in the direction of the central incident light the two rays are of equal velocity, and the factor $\frac{h}{\lambda}=0$. No chromatic effects will therefore be perceived in the centre. But the rays which come to the eye converging from points not central, will differ in velocity, the difference increasing with the obliquity. As every plane which passes through the axis is a principal plane, there will be an infinite number of principal planes intersecting each other in the line which forms the path of the central ray, the projections of which upon the surface of the lamina will form so many radii diverging from a centre. And as all planes which are parallel to the axis, however placed, are principal planes also, it is obvious that the planes normal to these radiating principal planes will form cylindrical principal sections having a common axis. The plane of polarization of the incident light can only coincide with one of the radiating principal planes. For that plane, the value of a in our formulæ will be 0°. For the principal plane at right angles to that, the value of a will be 90°. But we have seen that when $a=0°$ or 90°, the value of the chromatic term is 0. Hence there will be two planes in which no color will appear for any position of the analyzer—that is, for any value of β. But the brightness of the light seen in those planes will undergo variations of intensity, as β varies, according to the law of Malus.

For every plane except the two which have just been mentioned, the chromatic term will have a value—very slight in the neighborhood of those planes, and maximum at 45°. Very near to the centre, converging rays will have but a slight obliquity to the axis; and as a difference in length of path of one-quarter of an undulation or less fails to produce color in white light, there will be a central area which will be alternately white and black as the analyzer turns. From this area will proceed at right angles the arms of a cross, alternately dark and bright, which, from the faintness of the color in the neighborhood of azimuths 0° and 90°, will have a very sensible breadth.

At that degree of convergency which makes the amount of retardation for the most refrangible rays $=\frac{1}{2}\lambda$, will appear the first decided chromatic effect. And as, in a plate of uniform thickness, this convergency must be the same on every side, the color will take the form of a ring. This ring will be bright if the analyzer is *crossed* upon the polarizer; in the opposite position, dark. In order to observe the phenomena to the greatest advantage, it is best to employ homogeneous light. Then at greater convergencies, corresponding to retardations, or values of h, equal to $\frac{3}{2}\lambda$, $\frac{5}{2}\lambda$, &c., will be seen, with the crossed analyzer, other bright rings; while at the intermediate convergencies, corresponding to values of $h=\lambda$, 2λ, 3λ, &c., will be seen dividing rings, intensely dark. When white light is used, the dark rings will be mainly occupied by the smaller rings of the colors whose undulations are shorter than the mean, or the larger rings of those which are longer. Since the retardation depends directly upon the convergency, and the place of a ring of any color depends on the equality of the retardation with the length of a half undulation of that color, it will be evident without further demonstration, that the longer the undulation the larger the ring, and *vice versa.*

Rotating the crystalline plate in azimuth will produce no change in the phenomena. For in all positions of the plate there will always be one principal plane in azimuth 0°, and another in azimuth 90°.

Rotating the analyzer will cause the rings to pass by progressive changes into the complementary tints. In this rotation β becomes $=\alpha$ and $>\alpha$ successively for every one of the radiating principal planes which it passes, up to $\beta=90°$. The sign of the chromatic term changes, therefore, in every such case. And, as the sign changes also for $\beta=\alpha\pm90°$, $\beta=\alpha\pm180°$, or $\beta=\alpha\pm270°$, the color in all the quadrants will undergo similar changes simultaneously. Thus, in an entire revolution of the analyzer, the colors will be four times successively reversed; and for every position in which $\beta=45°$ or any of its odd multiples they will disappear.

The remarkable dislocation of the rings seen in crystals cut across the axis, when examined in circularly polarized light, has been mentioned. By applying the principles we have been considering we shall be able now to account for this singular effect. Suppose the crystal under examination to be a positive one, in which the ordinary ray has a higher velocity than the extraordinary. When a circularly polarized ray falls upon such a crystal, its component undulations, which, as we have seen, are at right angles to each other, with nodes dislocated by a quarter of an undulation, will advance with unequal velocities, and the amount of their nodal discrepancy will be changed. This would not disturb the symmetry of the rings, if the change were similar in all the quadrants. By attending to the following analysis, we shall see that such is not the case.

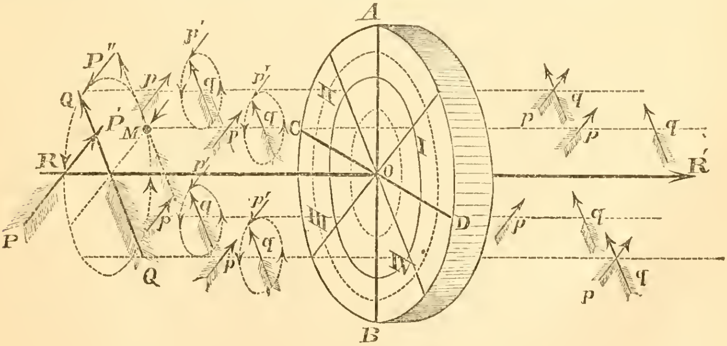

Fig. 56.

Let RR′ represent the direction of progress of a circularly polarized wave, of which the component molecular movements are represented by PP′ and QQ′. The positions of these arrows are those in which the molecular movements of their respective undulations are assumed to be at their maximum of velocity, and the distance between them, upon the line RR′, is to be taken as representing a quarter of an undulation. If we consider the effect of this composition of forces upon a molecule in the plane of movement of QQ′, we shall perceive that

PP′ will there be about commencing the return movement, in the direction denoted by P″, while QQ′ will be in the height of its activity.

The actual position of the molecule in the orbit described on QQ′ as a diameter will in fact be at M; but QQ′ and P″ may be taken as representing the *directions* of motion at the instant supposed. Let ACBD be a plate of the crystal cut across the axis, and let the analyzer (not represented) have the plane of its free molecular movement parallel to AB. Draw CD at right angles to AB, dividing the crystal into four quadrants.

As all the molecules in the wave front are actuated by similar movements, it will be sufficient to consider one of them in each quadrant. Let their several component forces be represented by the arrows marked *p q*, each pair having the same relations to each other as PP′ QQ′. It will be possible, in every quadrant, to draw a radius parallel to *p* or *q*. Let these radii be drawn. Now, the radii being principal sections of the crystal, that component in each case whose direction of movement coincides with the radius, will (if at all inclined to the axis) be an extraordinary ray, and will be retarded behind the other component. An inspection of the figure will show that this will happen for *p* in the first and third quadrants, and for *q* in the second and fourth. Let the inclination of the rays to the axis be such as to cause a retardation of the extraordinary ray of one quarter of an undulation behind the ordinary. Then, by comparing the positions of the arrows which represent the relations of the components after emergence, it will be seen that the effect has been to bring the planes of maximum molecular movement into coincidence in the second and fourth quadrant, and to increase the distance between them to half an undulation in the first and third.

But these changes are just what is required to obliterate the nodal dislocations in both cases, so that the waves will emerge plane. The resultant molecular movements in the second and fourth quadrants will be obviously vertical and parallel to AB. At the inclination or distance from the centre, therefore, which produces this amount of retardation, will be seen in these quadrants the first bright ring. Had the incident light been plane-polarized, however, the first ring would not have appeared until after a retardation of a half instead of a quarter undulation had taken place; it would have consequently required a greater inclination of the incident ray, and its apparent distance from the centre would have been increased. In the first and third quadrants, the resultant molecular movement may be inferred by referring the two components *p* and *q* to a common plane. If *q* be referred to the plane of *p*, for example, then, as the distance between them in the figure is half an undulation, the arrow *q* must be reversed, and the resultant movement will be horizontal. The analyzer will suppress this movement; or, in other words, at this distance in the first and third quadrants will appear a dark ring. In these quadrants there will not appear a bright ring until the retardation is increased half an undulation more; that is to say, to the total amount of three quarters of an undulation. Plane polarized light would have required a total retardation of only a half undulation to exhibit this ring. In the first and third quadrants, therefore, the bright rings are removed outward, and in the second and fourth they are removed inward, from the places they are seen to occupy in plane-polarized light, for a distance corresponding to a difference of a quarter of an undulation.

From what has been said, it will be easy to understand why two crystalline laminæ, of equal thickness, and cut from a crystal parallel to its axis, or equally inclined to the axis, when *crossed* upon each other, neutralize each other's effects. For the original polarized ray is, by the first lamina, divided into two which we have represented by R_o, R_e. In the supposed relative position of the two laminæ the ray R_e passes without double refraction in the principal plane of the second crystal, and the ray R_o in the conjugate plane. After emergence, and before analysis, therefore, the two rays may be represented by R_{oe} and R_{eo}, which symbols show that each has been equally modified in its passage through the system, and hence, that they reach the analyzer without any difference of path. In the foregoing formulæ, accordingly, $h=0$ for this case, and the chromatic term disappears.

We also obtain an explanation of the effects produced by *inclining* the lamina to the incident light. In general, the increased thickness of the crystal which the rays will have to traverse at an oblique incidence, will have the effect to increase the value of h, and the colors will *descend*, or take the tints belonging to thicker plates. But the new direction of the rays within the crystal may be one in which their difference of velocity is greater or less than that which belongs to the direction of perpendicular incidence. In this case the tints will descend more rapidly by inclining in one direction than they do when the inclination is opposite; or they may possibly remain stationary, or rise on one side and descend on the other. We suppose here as before that the analyzer is crossed upon the polarizer

Crystals of two axes, cut at right angles to either axis, will exhibit elliptical rings, the variations of velocity of the two rays being subject to different laws in the principal plane which contains the two axes, and in two other principal planes co-ordinate to that. When the axes are not largely inclined to each other, a lamina of the crystal taken perpendicularly to the line bisecting the angle between them will exhibit both systems at once. In these crystals neither ray obeys, in general, the law of Snellius. But there are three planes—those just mentioned—in which one of the rays obeys this law. These three planes are, in the first place, that which passes through both axes; and, secondly, both the planes normal to the first, which bisect the angles between the axes. The terms "ordinary ray" and "extraordinary ray," in the sense in which those words have been used, in speaking of crystals of one axis, are inapplicable in the present case.

The following equation, deduced by Fresnel from the general theory of double refraction, expresses the relation between the velocities of two rays traversing the crystal in the same direction, but possessing the differing polarizations produced by its double refraction :

$$\frac{1}{v'^2}-\frac{1}{v^2}=\left(\frac{1}{c^2}-\frac{1}{a^2}\right)\sin\varphi\sin\varphi'. \qquad [41.]$$

In this formula, v and v' are the two variable velocities; a and c are the *Snellian* velocities (constant) in the two principal planes co-ordinate to that which contains the axes; and φ and φ' are the angles made by the common direction of the two rays with the axes themselves.

It may be remarked that the rays whose velocities are here denoted by v and v', cannot be the two rays which proceed from one incident ray, since these two rays do *not* pursue the same course within the crystal. This consideration is not important, when the divergence of the rays produced by double refraction is small, (which is the case with most crystals of two axes, and with *all* for rays in the vicinity of the axes themselves;) and therefore we may employ this law for the purpose of determining the forms of the colored rings, in plates cut so as to make it possible to observe both axes at once. Putting $vv'=ac$ and $v+v'=2\sqrt{ac}$, suppositions which are sensibly true near the axes, the formula gives—

$$v-v'=\frac{v^2v'^2}{v+v'}.\left(\frac{a^2-c^2}{a^2c^2}\right)\sin\varphi\sin\varphi'=\frac{a^2-c^2}{2\sqrt{ac}}\sin\varphi\sin\varphi', \text{ (very nearly.)}$$

As we propose to confine the inquiry to the immediate vicinity of the axes, where φ and φ' are small, we may take the angles themselves, or their chords, instead of their sines. Let these chords be r and r'. Then the equation becomes—

$$rr' = \frac{2\sqrt{ac}}{a^2 - c^2}(v - v'), \qquad [42.]$$

which, if $v - v'$ be made constant, is the equation of a *lemniscate*. The annexed figure represents a *lemniscate*, or curve whose distinguishing property is that the products of every pair of *radii vectores*, drawn from two polar points, and intersecting in the curve, are equal to each other, and to a constant quantity. If PQ, the distance between the poles, be bisected at A. and PA made $= q$, then the constant value of PR × QR, divided by q, is called the *parameter*, and may be represented by p. Put $\text{PR} = r$, $\text{QR} = r'$, $\text{AN} = x$, and $\text{RN} = y$. The construction gives, immediately—

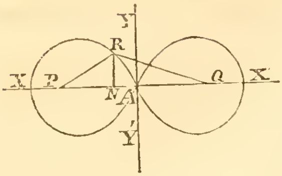

Fig 57

$$r'^2 = (q+x)^2 + y^2;\ r^2 = (q-x)^2 + y^2: \text{ whence}$$
$$r^2 r'^2 = \left((q+x)^2 + y^2\right)\left((q-x)^2 + y^2\right) = p^2 q^2.$$
$$\text{Or } r^2 r'^2 = (q^2 + x^2 + y^2)^2 - 4q^2 x^2 = p^2 q^2. \qquad [43.]$$

In the case in hand, the parameter is the quotient found by dividing the second member of the equation for the velocities, in its last form, by q. The value of q itself may be directly measured, if the chromatic image be thrown upon a screen, as was done by Sir John Herschel in his study of the forms of these curves; or it may be assumed at pleasure, from a knowledge of the angle between the axes. Thus, if ABCD be the lamina, and aa', bb' the axes, then, to the eye at E the poles are a and b in directions parallel to aa' and bb'; and half their distance is the value of q. The rings, however, may be referred to any distance, as EP; and the poles will then be at Q and R The distance EP and the angle REQ are all that is necessary to determine q, which is now PQ. It must be observed, however, that for a projection on this scale, the value of the constant, or second member of the equation above, must be increased in the ratio of the square of the distance of QR to that of AB from the eye.

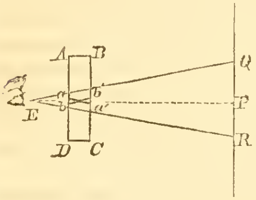

Fig. 58.

When the direction in which the rays reach the eye is such that the difference of path of the two rays is half an undulation, there will be seen, in homogeneous light—the analyzer being crossed upon the polarizer—the first bright ring. When the difference becomes an entire undulation, the first dark ring will appear. The parameter of the lemniscate changes with every new ring. For the bright rings, the parameters will evidently form an arithmetical series, corresponding to the odd numbers 1, 3, 5, 7, &c. For the dark rings there will be a similar series of values, proportional to the even numbers.

The lemniscates are not perfect, (though some of them are nearly so,) because we have admitted some small errors into our assumption. The inner curves also will, in many cases, form ellipses around a single pole. It is obvious that this must be the case when the constant is less than q^2. For q^2 is the smallest value that the product of the radii vectores can have; and when the parameter is not equal to q, there can be no lemniscate.

When the analyzer is crossed upon the polarizer, in observing these curves, if the plane of the axes is in the plane of polarization of the incident light, there will be seen a black cross intersecting the system symmetrically; the principal bar of which will coincide with the plane of the axes. The transverse bar will pass at right angles to this, half way between the poles. In these two planes there is (in the position of the crystal supposed) no double refraction of the incident polarized ray. The light is therefore transmitted without interruption, and being cut off by the analyzer, shows the dark cross. By rotating the analyzer 90°, the cross becomes bright, as with crystals of one axis. But when the crystal itself is turned in azimuth, while the analyzer remains in one of the principal azimuths, the arms of the cross break at the centre, two of them on each side forming together a curve. At 45°, the two curves present the appearance of opposite hyperbolas.

To follow these changes analytically would require a larger acquaintance with the physical theory of double refraction than is furnished in what precedes. We will therefore, next in order, turn our attention to that subject.

§ XI. DOUBLE REFRACTION.

We have seen that the double refraction of light is always attended with polarization. It is proposed now to attempt a physical explanation of this phenomenon.

Refraction, in general, considered as a bending of the ray, is owing to a change in the velocity of the wave as it enters the refracting medium. When the refraction is double—in other words, when a single wave is divided by refraction into two waves—the velocities of the two waves must be unequal. It is presumed that this difference of velocities is owing to a difference of elasticity of the ether within the medium. But, inasmuch as the two rays often follow the same track, each with its own determinate velocity, while they remain quite distinct from each other, it is evident that their velocities cannot be determined by the elasticity of the ether *in the direction of their progress.* It becomes therefore a necessity to assume that their molecular movements are transverse to the ray, and in the surface of the wave itself. The *fact* of double refraction is thus an incontrovertible proof of the truth of the doctrine of transverse vibrations, independently of the many evidences of the same truth derived from polarization and the phenomena of interference.

But inasmuch as, in a medium in which the elasticity of the ether varies according to a certain law, the elasticity will usually be different in each of the indefinite number of planes which may pass through a given ray, it follows that if the ray pursue a determinate course with a constant velocity, its transverse vibrations must be confined to some determinate plane. Double refraction involves, therefore, as an indispensable condition, polarization; and, as a general rule, plane polarization would seem to be the necessity.

Experiment proves that these theoretic inferences are correct; and also that the planes of polarization of the two rays which originate from a single incident ray, in a doubly refracting body, are at right angles to each other. Two questions present themselves, therefore, for solution: First, how is the direction of molecular movement in a polarized ray related to the plane of polarization? and secondly, what cause determines this movement in the doubly refracting body, to these particular directions?

In regard to the first question, we may arrive at a conclusion, by considering the case of a crystal of one optic axis, like Iceland spar. If we suppose such a crystal to be ground to a perfect sphere and polished, a ray incident perpendicularly upon any part of its surface will coincide in direction with the radius of the sphere. Such a ray falling upon a sphere of homogeneous glass would pass undivided through the centre. But with the sphere of crystal which we have supposed, there is but one diameter in which this will happen. This is the diameter coincident with the optic axis; and in this direction there is no double refraction. If the incident ray is common or unpolarized light, (a supposition which is to be understood in all that follows,) the emergent ray will be unpolarized also. And, as the molecular movements of common light are in all azimuths around the ray, it is evident that the elasticity of the ether in the crystal is the same in all directions at right angles to the optic axis. The moment, however, that we depart from the pole of the sphere—maintaining still a perpendicular incidence upon its surface—a second ray makes its appearance. The light is now equally divided. A part, which we call the ordinary ray, still follows the radius and passes through the centre of the sphere. The other portion is bent at the surface, and crosses the diameter in which we found no double refraction, above the centre or between it and our first supposed point of incidence; that is, the point which we have called the pole. The deviation will be slight at first, and will go on for a time increasing, as we descend in *latitude;* but will afterwards diminish till we reach the *equator*, when it will become nothing. But though the deviation diminishes, the double refraction increases; that is to say, the difference of velocity between the two rays becomes greater and greater as we approach the equator, and in that plane attains its maximum. Both rays now pass through the centre; but one is so far behind the other that two images may be seen of any object beyond, at different apparent distances from the eye. If the incidence be *not* perpendicular, the ray which has always passed through the centre undergoes refraction according to the simple law of Snellius, in all planes and in all azimuths; but this is not at all true of the other. The inference is that the velocity of the first of these rays is always determined by the same elastic force; which must be *that* force which we have seen to be at right angles to the axis, or parallel to the equator of our supposed sphere.

And here, in order to avoid error or confusion, let it be observed that the line which we have called the axis of this sphere is not *the* optic axis of the crystal, but only *one of the* optic axes. All lines parallel to this are equally optic axes. In other words, the name optic axis is the name, not of a *line*, but of a *direction*.

Now if we once more follow, in mind, our ray at perpendicular incidence, from the pole of the sphere to the equator, we shall see that there is no difficulty in imagining its molecular movements to be constantly parallel to the equator, provided we suppose them perpendicular to that meridian plane (principal section) which passes through the ray and the axis of the sphere. The constant velocity of the ordinary ray is thus accounted for without difficulty.

The velocity of the extraordinary ray being variable, its molecular movements must encounter a different elasticity in different directions of its progress. Moreover, as its plane of polarization is at right angles to that of the ordinary ray, its molecular movements should be so likewise. We have only to suppose these movements to take place *in* the meridian, or principal section, plane, and we shall see that they will turn with the ray itself, as we pass from the pole to the equator: so that, while, in the first position, they are parallel to the equator like those of the ordinary ray, they are inclined to it at increasing angles as we descend in latitude, and become perpendicular to it in latitude zero; that is, when the ray is in the plane of the equator itself. Now this would make no difference in the velocity, provided the ether were equally elastic in all directions. As the velocity *is* variable, in point of fact, the conclusion must be that the elasticity is variable also. In the direction of the axis we must assume it to be greatest, and in intermediate directions to possess an intermediate force.

Now the plane of polarization of the ordinary ray (experimentally ascertained) is the principal section of the crystal. And as we have been compelled to conclude that the molecular movements of this ray take place at right angles to the principal section, it follows that, in plane polarized light, the vibrations are at right angles to the plane of polarization. This settles the first of the questions proposed above. The second is less simple.

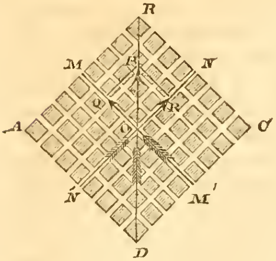

Fig. 59.

If a polarized ray, whose molecular movements are in the direction OP, in the annexed figure, fall upon a lamina of doubly refracting crystal, whose principal section is MM′, it undergoes double refraction in every case except that in which OP coincides with MM′ or NN′, the conjugate plane. The effect is the same, in seeming, as if the lamina allowed no free passage for movement, except in these directions. The imaginary structure presented in the figure is in accordance with this idea. The undulations of which OP is an element, encountering such a structure, would be necessarily resolved into two movements, taking the directions of the open passages; and according to the laws of the resolution of forces, we should have $\overline{OP}^2=\overline{OQ}^2+\overline{OR}^2$. Or putting I for the total intensity of the light, and a for the angle between OP and MM′,—

$$I=I\cos^2 a+I\sin^2 a,$$

which is the law of Malus.

This illustration is given merely to facilitate the conception of the constant determination of the ethereal vibrations in crystalline bodies to fixed directions. The cause must be one more general than such a mechanical structure could possibly be. The theory of Fresnel, embracing all the cases of double refraction, is founded on the assumption that the elasticity of the ether may be different in the directions of *three rectangular axes;* and among the conclusions mathematically deducible from this assumption, is the proposition that, in a medium so constituted, the molecular movements of an incident ray will be *unstable* except in two determinate azimuths at right angles to each other. If they are not in those azimuths, or one of them, on entering the medium, they will be instantly *turned* into them; and thus the ray will be polarized in planes having different directions in the crystal.

If we take a crystal of two axes, and form from it prisms, of one of which the edges shall be perpendicular to the plane containing the axes, while the others have their edges respectively parallel to the lines which bisect the angles between the axes, we shall find that, in the planes of refraction of these prisms, one of the rays follows the law of Snellius; but that the indexes of refraction for these are different. These rays thus obeying the ordinary law are moreover polarized *in* their several planes of refraction. Their molecular movements are therefore perpendicular to those planes, or parallel to the edges of the prisms, that is to say, parallel (by construction) to three determinate fixed lines in the crystal, each at right angles to the other two. These velocities, then, *determine the elasticities* in the direction of three rectangular axes. From these as constants, Fresnel derived an equation expressing the elastic force in all intermediate directions. The three velocities are distinguished by the letters a, b and c, in the order of their magnitude—that denoted by a being greatest. And elasticities being as the *squares* of the velocities which they generate, the three elasticities are a^2, b^2, and c^2. Now if any line be taken which makes with the directions of the elasticities a^2, b^2, c^2, angles represented by A, B and C, and if R denote the velocity which the elasticity in the direction of that line is capable of generating, then we shall have the equation—

$$R^2=a^2\cos^2 A+b^2\cos^2 B+c^2\cos^2 C. \qquad [44.]$$

Giving A, B, and C all possible values, R will have all possible directions; and, considered as a radius vector, its extremity will describe a surface the squares of whose radii will be equal to the elasticities in their directions. This surface, therefore, Fresnel denominated the *surface of elasticity*.*

The surface, as might be inferred from the principle of its construction, is an ellipsoid, of three unequal axes.

Now, it is a point important to be clearly conceived, that when, in a medium of variable elasticity, the equilibrium of forces is disturbed by a displacement of its molecules in a given direction, the resultant of elastic resistances excited is not generally in the line of the displacement. Were the displacement to take the direction of one of the axes of the surface of elasticity, the resistance would be directly opposed to the disturbance. But suppose it to be in the direction of some oblique radius; and, to simplify the matter, suppose this radius to be in a plane passing through two of the axes. Let then, in Fig. 60, ADBE be the section, passing through AB and DE, the axes of greatest and least elasticity. Let, a molecular disturbance, which we will call r, take place in the direction CF; and, for facility of conception, let us take the line FC itself to represent the resistance it encounters *in this direction*. AC is a, and DC is c. Now, the displacement r, if it took place wholly in the direction of a, would develop a resistance proportional to ra^2, or equal to fra^2, f being a constant. And if it took place wholly in the direction of c, it would develop a different resistance$=frc^2$. But the amount of displacement in the direction of a is only $r\cos A$. And that in the direction of c is only $r\cos C$. Also, as A=ACF in this case, and C=90°−ACF, we have $r\cos C=r\sin A$. Hence the resistances developed are $fra^2\cos A$, and $frc^2\sin A$. Now, the first of these expressions being the horizontal component of the resistance (as the figure is drawn) and the second, the vertical, the second divided by the first will give the tangent of the inclination to a, which inclination we will call A′.

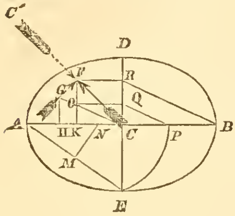

Fig. 60.

$$\text{Then } \frac{frc^2\sin A}{fra^2\cos A}=\frac{c^2}{a^2}\tan A=\tan A', \qquad [45.]$$

which is less than tanA—or the resultant is less inclined to a than FC.

A graphic method of determining the resultant, both in magnitude and direction, is suggested by this formula. TanA′ is a fourth proportional to a^2, c^2, and tanA. Calling KC radius (for present purposes) FK=tanA. Draw FR perpendicular to DE, and join RB. Join AE, bisect it in M, and draw MN perpendicular to AE. With N as a centre, describe the arc EP. Then $CE^2=c^2=AC.CP$. And $CB^2=a^2=AC.CB$. Or, $a^2 : c^2 :: CB : CP$. Draw, therefore, PQ parallel to BR, and QO parallel to AB. Draw FG perpendicular to FC, and the radius CO, through O, to meet it in G. CG is the resultant, and GCA=A′.

The resultant force consists, then, of two components—one, equal and opposite to CF, and the other FG at right angles to it. This latter force deflects the motion of the molecule in FC, and turns it toward the shorter or longer axis, according as the movement is one of condensation or of rarefaction. And there can be no stability of oscillation in this place, in any line which is not parallel

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* This polar equation may be referred to rectangular co-ordinates, by putting x, y, and z for the co-ordinates parallel to a, b, and c, respectively, and substituting the following values:

$$R^2=x^2+y^2+z^2. \quad \text{Cos}A=\frac{x}{R}; \ \cos B=\frac{y}{R}; \ \cos C=\frac{z}{R}.$$

$$\text{Whence } R^4=a^2x^2+b^2y^2+c^2z^2.$$

$$\text{Or, } (x^2+y^2+z^2)^2=a^2x^2+b^2y^2+c^2z^2,$$

which is an equation of the fourth degree.

to a or c. In either of those directions the displacement develops no deflecting force; since, in the former, cosA=1 and sinA disappears; and in the latter sinA=1 and cosA disappears.

The arrows illustrate the relations and mutual action of these forces, and the corresponding movements of the molecules. During compression the disturbing force is CF, and the movement from C toward F. The opposing component of the resistance is C′F, and the deflecting component GF. While CF predominates over C′F, the point of the arrow CF—that is, the direction of the molecular movement—will be turned nearer the direction CD. But when C′F predominates over CF, as in the return vibration, C′F represents the movement, and the deflecting force turns the point of the arrow C′F nearer the direction AC.

The value of the resultant may be determined by means of those just given for its components, from the right angled triangle CGH. For this gives us, (putting ρ for the resultant,)

$$\rho^2=f^2r^2a^4\cos^2A+f^2r^2c^4\sin^2A\,;$$

$$\text{or }\rho=\pm fr\sqrt{a^4\cos^2A+c^4\sin^2A}.$$

The equation of the surface of elasticity also gives us, for the value of the radial resistance (denoted by ρ')

$$\rho'=R^2fr=fra^2\cos^2A+frb^2\cos^2B+frc^2\cos^2C.$$

Or, as cosC=sinA, and cosB=cos90°=0,

$$\rho'=fr(a^2\cos^2A+c^2\sin^2A).$$

Hence, if ω represent the angle GCF, we shall have

$$\text{Cos}\,\omega=\frac{\rho'}{\rho}=\pm\frac{a^2\cos^2A+c^2\sin^2A}{\sqrt{a^4\cos^2A+c^4\sin^2A}}\pm\frac{R^2}{\sqrt{a^4\cos^2A+c^4\sin^2A}}\qquad[46.]$$

This simple case has been examined in detail, in order to facilitate the conception of the more general one, which will now be attended to. Let a molecular displacement, r, occur in any direction whatever. Let CF, Fig. 61, be the direction of displacement, and let it be assumed as the representative of the force developed in that direction. As in the former case, if this force be resolved into three component forces in the directions of the several axes, the resistances developed will be fra^2cosA, frb^2cosB, frc^2cosC.

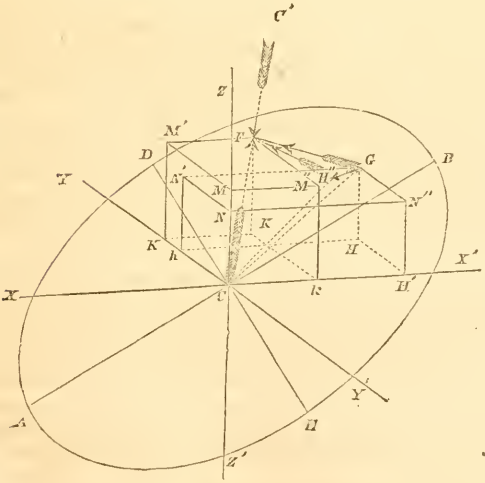

Fig. 61.

According to the laws of the composition of forces, these three components are in the relation of the three dimensions of a parallelopipedon, of which the resultant is the diagonal. Let CNN′GH, &c., be this parallelopipedon. CG is the resultant expressing the total resistance in both quantity and direction. But R²fr, by the equation of the surface, expresses the total resistance in the direction of the radius; and if, to facilitate conception and comparison, we conceive it to be the diagonal of another parallelopipedon, CMM′FK, &c., the three dimensions of this solid will be $R^2 fr\cos A$, $R^2 fr\cos B$, and $R^2 fr\cos C$. For the sake of symmetry, we will employ for a moment these components, instead of $R^2 fr$ itself. We will denote, also, as before, the two resultants by ρ and ρ' and the angle, GCF, between them, by ω. Then,

$$\operatorname{Cos}\omega=\frac{\rho'}{\rho}=\pm\frac{fr\sqrt{R^4\cos^2A+R^4\cos^2B+R^4\cos^2C}}{fr\sqrt{a^4\cos^2A+b^4\cos^2B+c^4\cos^2C}}=\pm\frac{R^2}{\sqrt{a^4\cos^2A+b^4\cos^2B+c^4\cos^2C}}.$$

$$\text{Or, }\cos\omega=\pm\frac{a^2\cos^2A+b^2\cos^2B+c^2\cos^2C}{\sqrt{a^4\cos^2A+b^4\cos^2B+c^4\cos^2C}}. \qquad [47.]$$

Now the wave front in which r, having the direction CF, is one of the movements, cuts the surface of elasticity in an ellipse, which may be represented by ADBE. The line CG will not usually lie in the plane of this ellipse. If the resultant, ρ, be decomposed into two forces, one of them equal and opposite to ρ', or CF, and the other GF, perpendicular to CF, this last tends to turn the movement in CF out of that line, as before. But, as it is not in the plane of the ellipse, ADBE, which is the wave front, in order to understand more clearly its effect upon the direction of movement in the plane of the wave (which is all that concerns the question of polarization) decompose this force again, by dropping, from G, the perpendicular GH″ upon the wave front, and joining H″F. The component, GH″, being normal to to the wave, can produce no effect in the way of polarization. The other component, H″F, tends to turn the movement, as in the former case, alternately, in the direction of the shorter axis, DE, of the elliptic section of the surface of elasticity, and of the longer axis, AB.

Observe that if the displacement had been originally in the direction of one of these axes, there would have been no deflecting force, H″F. For this lateral force owes its existence to the inequality of elasticity, or resisting force, on the two sides of the movement of displacement. But, by the law of construction of the surface of elasticity, the squares of its radii are the measures of the elastic forces in their directions; and at the extremities of the major and minor axis of an ellipse the radii on either side of the axis are equal and symmetrically disposed.

It follows, that whenever a ray of light falls upon a medium of such a nature as we have been considering, all its movements will be thrown into parallelism with the two axes of the elliptic section made by its front with the surface of elastictity. And thus we have a physical account of the polarization of light by double refraction.

We have, at the same time, the cause of the unequal velocity of the two waves. For, by the construction of the surface of elasticity, all its radii are measures of the velocities of undulations whose molecular movements coincide with them in direction. The two velocities will, accordingly, be to each other as the major and minor axes of the elliptic section of the surface of elasticity made by the wave front.

We have also the cause of the polarization of the two rays in planes at right angles to each other. This is so, because the two axes of the ellipse are in that relation.

Since the two velocities are both uniform, though unequal, a plane wave is transformed into two plane waves, by double refraction. Supposing the refracting surface to be also plane and of indefinite extent, and that a plane wave enters it obliquely, the intersection of the wave front with the surface will be a straight line, and will advance along the surface parallel with itself, as the wave advances. The refracted waves necessarily both intersect the refracting surface in the same straight line. And if we suppose these refracted waves to be compounded of the infinitely numerous elementary waves which may be imagined to originate in the line of intersection, each resultant refracted wave front will be a common tangent plane to all the elementary waves of its own kind thus generated.

Though the planes of vibration of the two refracted rays are originally perpendicular to each other, yet the taking of different velocities slightly modifies this relation. The change is hardly sufficient to be sensible.

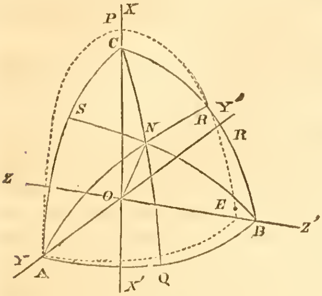

Fig. 62.

There are two sections of the surface of elasticity which are circles. Let, for example, in the figure annexed, the axes of elasticity be OX, OY, OZ, and let the dotted lines represent the contour of one-eighth of the surface of elasticity; OP being $=a$, OA$=b$, and OE$=c$. Upon the same axes, with OA$=b$, as radius, let there be constructed a corresponding portion of the surface of a sphere, ACB; in which AC, AB, BC are quadrants. Since a is the largest, and c the least axis, the ellipsoidal and spherical surfaces must cut each other somewhere between B and C. They will also touch at A. Let one point of the intersection be at R, and through R pass a plane, OAR, intersecting the spherical surface in AR. In this, take any point, as N, and draw through it the quadrants BNS, CNQ. Considering N as a point of the surface of the sphere, the radius ON$=b$, and we have

$$b^2 = b^2\cos^2 A + b^2\cos^2 B + b^2\cos^2 C.$$

Considering it as in the surface of the spheroid, ON$=$R, and

$$R^2 = a^2\cos^2 A + b^2\cos^2 B + c^2\cos^2 C.$$

If both these suppositions are true $R^2 = b^2$; whence we deduce

$$(a^2 - b^2)\cos^2 A = (b^2 - c^2)\cos^2 C.$$

If α be put for the arc CR, the inclination of the plane, ANR, to the axis a, then, in the triangle CNR, we have

$$\text{Cos}^2\text{CN} = \cos^2 A = \cos^2\text{NR}\,\cos^2\text{CR} = \sin^2 B\,\cos^2\alpha.$$

And, in the triangle BNR,

$$\text{Cos}^2\text{BN} = \cos^2 C = \cos^2\text{NR}\,\cos^2\text{BR} = \sin^2 B\,\sin^2\alpha.$$

Substituting these values for $\cos^2 A$ and $\cos^2 C$ in the foregoing, and dropping the common factor, $\sin^2 B$,

$$(a^2 - b^2)\cos^2\alpha = (b^2 - c^2)\sin^2\alpha$$

$$\text{And } \frac{a^2 - b^2}{b^2 - c^2} = \frac{\sin^2\alpha}{\cos^2\alpha} = \tan^2\alpha. \quad \text{Or, } \tan\alpha = \pm\sqrt{\frac{a^2 - b^2}{b^2 - c^2}}, \qquad [48.]$$

the double sign indicating two positions for the section, one in the first, and the other in the second quadrant—that is to say, indicating that there are two such circular sections.

The inclination to a of the *normals* to these circular sections—that is, of the *directions of progress of the waves of which they are the planes*—will, of course, have for tangent the reciprocal of the expression just given; or, if a' represent this inclination,

$$\tan a' = \pm\sqrt{\frac{b^2-c^2}{a^2-b^2}}. \qquad [49.]$$

If the wave front of the incident light coincide with one of these circular sections of the surface of elasticity, it appears, from the principles already laid down, that the wave can have no determinate plane of polarization. For all the radii of the section being equal, the elastic forces are in equilibrio in every azimuth; and there will be no lateral force to deflect the molecular movements. If, in the first expression foregoing, we make $b=c$, the denominator becomes zero, and the tangent is infinite, or $\tan 90°$. The two circular sections then coincide in the plane of bc at right angles to a, and the crystal is a negative crystal of one axis. If $a=b$, $\tan a=0$, and the two circular sections meet in the plane of ab at right angles to c, and the crystal is positive. If $a=c$, then, since b is the mean axis, all the axes are equal and $\tan a=\frac{0}{0}$; an indefinite value, signifying that the circular sections have no fixed positions; or that all the sections are circular.

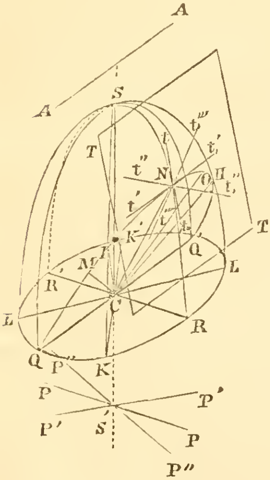

Fig. 63

Let us now apply the principles we have been considering to the phenomena presented by crystals cut across the axis of greatest elasticity, or the line intermediate between the optic axes. In the accompanying figure, let QRQ'S represent one-half the surface of elasticity, in which SC$=a$, QC$=b$, RC$=c$. Let PP parallel to RR' represent the direction of molecular movement in an incident wave, whose direction of progress is S'S. Let AA represent the direction of free molecular movement in an analyzer with which the crystal is observed. Also let QNOQ' represent one of the circular sections of this surface. The ellipse QRQ'R' is the section of the wave with the surface of elasticity; and the axes QQ' and RR' are the directions into which it turns all molecular movements in its plane. But PP being parallel to RR', is already in one of these directions, and hence this wave passes through without modification; but encountering the analyzer crossed upon it, is suppressed.

If, instead of a single plane wave, we suppose many waves more or less inclined to each other, convergent toward S, and all having the general direction of molecular movement PP, their intersections with the surface of elasticity will be ellipses whose axes are variously directed. There are two planes, however, SQQ' and SRR', which will contain the axes of all sections made by waves normal to them. For it is easily seen that, if the plane QRQ'R' turn about RR', this line RR' will always be the minor axis of the section. If the same plane turn about QQ', this latter line will be the major axis of the section until the turning plane reaches the position QNQ', when the section will be circular. Afterwards it will be again elliptical with QQ' for its minor axis. It follows that all the convergent waves which are normal to SQQ′ and SRR′ will suffer no modification of their molecular movements; and as the analyzer AA is crossed upon them all, there will be seen in the field of view two dark lines, or bars, intersecting each other at right angles in the point C.

Every other converging wave will, however, make a section of which the axes are not in the planes SQQ′ or SRR′. Let, for example, the plane QRQ′R′ turn around the line LL′, and let KK′ be at right angles to LL′ in the turning plane. Then CK gradually increases in length, while LL′ remains constant. When CK reaches the position SC, it becomes the major axis of the section. The original position of the major axis being QC, it appears that, during the turning, it changes its azimuth by the total amount QCK, while the minor axis changes from CR to CL. There is therefore no position in which either of these axes can be parallel to PP or RR′. It follows that every convergent wave not normal to either of the two principal planes SQQ′ or SRR′ must undergo double refraction; and, therefore, in passing the analyzer AA, will exhibit chromatic effects.

If now the direction of molecular movement in the incident wave be changed to P′P′ or P″P″, there will immediately be double refraction in both the principal planes, or the dark bars will disappear from them. But as, in the turning of the plane QRQ′R′ round the various diameters LL′, the axes of the section made by the plane turn in azimuth, it is evident that some section can always be found which at some inclination will have one axis parallel to P′P′.

To take an extreme case, let P′P′ be 45° distant from RC, when it will be equidistant between the axes RC and Q′C. The optic axes of the crystal, which are in the plane SRR′, will then be in azimuth 45° from the plane of polarization. Now, since the axes of the section formed by the plane QRQ′R′, as it turns round LL′ or KK′, do not reach LL′ or KK′ until K or L reaches S, if LL′ be parallel to P′P′, no light will come to the analyzer in the planes SLL′ SKK′, without being or doubly refracted. The dark brushes will not therefore appear in the central plane coinciding with, or normal to, the direction of incident molecular movement.

There will be other sections, however, which will have an axis parallel to LL′ or P′P′, or to KK′ normal to LL′. To discover their positions, let us consider for a moment the circular section QNQ′. If at N, in the plane SRR′, there be a plane TT′, tangent to the surface of elasticity, and if, in this plane, the tangent lines tt_1 and $t't'_1$ be drawn—the first tangent to the elliptic section SNR, and the second tangent to circular section QNQ′—then the angles made by the radius CN, of the surface of elasticity, with the latter, will be right angles; but the angle CNt will be greater than a right angle, and the angle CNt_1 will be less than a right angle. If the plane QNQ′ turn about CN—say to the position $t''t''_1$—the angle CNt'' will be greater than a right angle, and the angle CNt''_1 will be less than a right angle. The minor axis of the elliptic section made by the plane in this position will therefore fall toward t''_1, from N. So, if the plane turn toward the position $t'''t'''_1$, the minor axis of the section it makes will fall toward t''', from N; that is, below the circular section in each case.

Now, LL′ being supposed parallel to P′P′, and KK′ normal to it, let CH CI be the intersections of the planes SLL′ and SKK′ with the circular section. If the plane QNQ′ turn about CH, so that Q′ approaches L, the minor axis of the elliptic section it makes will fall to the right of H. But if another line, to the left of CH, as CO, be made the turning line, a position may be found for it in which, for a given amount of turning, the minor axis of the section, which will be to the right of CO, may fall in the plane SHL. The nearer O is to H, the less the plane will be required to turn to produce this effect. Accordingly there will be a series of sections, more and more inclined to QNQ′, and also to QRQ′, and whose intersections with QNQ′ will differ in azimuth along the arc HON, which will have their axes parallel to LL′ or P′P′.

By considering the effect of turning the plane about CI, we should arrive at a similar conclusion in regard to a series of sections cutting the circular section near the point I, one axis of each of which would be parallel to CK, or normal to P′P′. The *normals to the planes* of all these sections are the directions of wave progress, or nearly the directions of ray progress; and if, from a point above AA, the analyzer, lines should be drawn parallel to all those normals, they would indicate the directions in which (no double refraction of the incident polarized ray occurring in them) the several points of the axis of the dark bands or brushes ought to appear. These directions being all more inclined to SC than is the normal to the circular section, it is evident that the pole in this case will be the point of nearest approach of the dark band to the centre of the field of view. It is furthermore evident that the bands are curved. For if they are not so, the normals must all lie in one plane. But they cannot lie in one plane unless the sections to which they are normals have a common intersection—a condition which, from the law of their construction, cannot exist. The plane QNQ′ turns about an axis movable in azimuth, and the surface which is the locus of all the normals is necessarily curved.

The foregoing illustration accounts for only one of the dark bands. The other is produced in the same way, and depends on the other circular section which is not drawn. The analytic investigation of these changes would be extremely complicated.

It will be seen that this mode of explanation applies itself to the case of one-axed crystals with great facility. The surface of elasticity for such crystals being an ellipsoid of revolution, every section has one of its axes in the plane which contains its normal, and also the axis of the ellipsoid. The loci of the dark bands will, therefore, always necessarily be planes normal to each other, intersecting in the optic axis of the crystal.

The direction of ray-propagation is that of the radius of the wave. When the theoretic wave is spherical, the ray is normal to the surface, but not otherwise. The velocity of *wave progress* is measured by the normal let fall from the centre of the wave upon the wave front; and this in spherical waves is the same as the velocity of *ray progress;* but in waves not spherical, ray progress may exceed wave progress.

§ XII. WAVE SURFACE.

In order to determine, *a priori*, the direction which a ray will take on entering a doubly refracting medium it is necessary to know what is the figure of the wave surface. For crystals of one axis we have seen that this problem was solved by Huyghens; but the complete generalization of the theory was reserved for Fresnel.

Could a molecular movement be produced, starting from a single point and propagated in all directions in a medium of variable elasticity of three axes, the surface defining the limits of the tremor at any moment would be the wave surface. The same form of surface (sensibly) would be defined by an infinite number of planes tangent to a luminous sphere like the sun, moving outwardly from the body in all directions with velocities such as the law of variable elasticity requires, when their distance from the body becomes very great compared with the diameter of the sphere itself. Proceeding upon this supposition, Mr. Fresnel obtained an equation for the wave surface, which is the following:

$$a^2x^2[x^2+y^2+z^2-(b^2+c^2)]+b^2y^2[x^2+y^2+z^2-(a^2+c^2)]+c^2z^2[x^2+y^2+z^2-(a^2+b^2)]+a^2b^2c^2=0. \qquad [50.]$$

$$\text{Or, } (a^2x^2+b^2y^2+c^2z^2)[x^2+y^2+z^2-(a^2+b^2+c^2)]+a^4x^2+b^4y^2+c^4z^2+a^2b^2c^2=0.$$

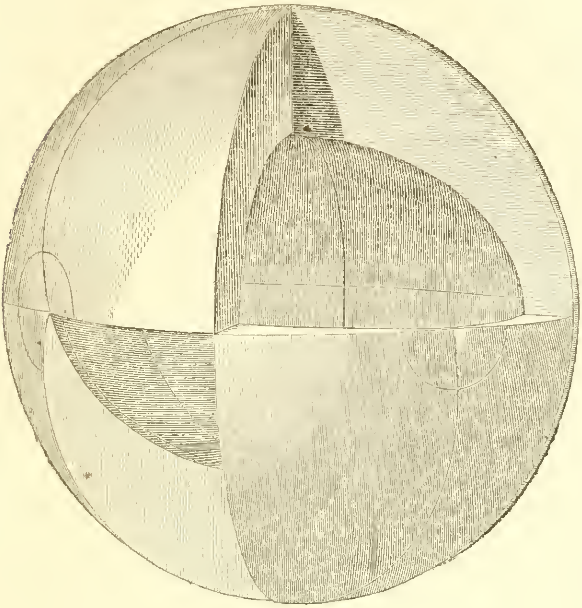

Fig. 64.

This is an equation of the fourth degree, and represents a surface of two nappes, or sheets, inosculating at four points. Figure 64 is a representation of this surface copied from a drawing made by Mr. Ferdinand Engel, of Washington city. In order to exhibit the interior nappe, two ungulae are represented as cut away; one of the section planes passing through the two points of inosculation in the visible surface, and another through one of them.

The form of the wave being known, we may apply, for the determination of the direction of a ray, the principle on which Huyghens founded his construction for spherical and spheroidal waves. Resuming once more the figure employed in illustrating that construction, we may say let CD be the direction of the semi-axis of elasticity a—the semi-axes b and c being at right angles to this, and to each other. Upon these axes let the wave surface be constructed in space, with C, the point of incidence, as the centre; the values of a, b, and c being the velocities of rays moving at right angles to them (and whose molecular movements are therefore parallel to them) when the velocity in vacuo is made unity. If MN be the surface of refraction, RC incident ray, and CP the normal to the surface, then RCP is the plane of incidence. In this plane draw CG perpendicular to RC. CG will be in the incident wave front. Make RC=1, draw RG parallel to the refracting surface, and cutting CG in G. Draw also GQ parallel to RC. Then when the wave front has advanced to Q it will intersect MN in a line drawn through Q perpendicular to the plane of incidence. If the plane of the diagram be supposed to be the plane of incidence, this perpendicular will be projected into the point Q. Both the refracted waves will intersect MN in the same line; and their planes will be also tangent to the two sheets of the surface. If ADB represent one of these sheets, and HFK the other, then tangent planes passing through the perpendicular projected in Q and meeting these sheets, as at E and F, will determine the directions, CE, CF, of the refracted rays. It is to be observed, however, that the points E and F, and therefore the refracted rays CE, CF,

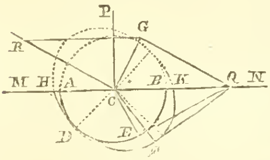

Fig. 34.

will generally not be in the plane of incidence; nor will the ray or radius of the wave surface be normal to the tangent plane.

The three principal sections of the wave surface present each two curves returning into themselves, as shown in these figures:—

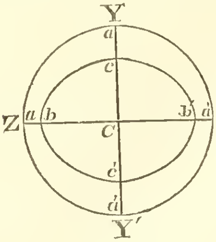

Fig. 65.

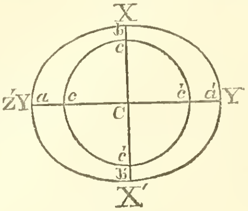

Fig. 66.

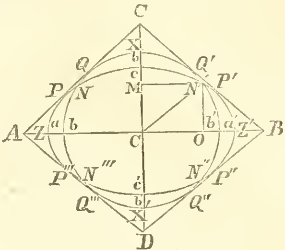

Fig. 67.

The equation of the section through $a\ b$, Fig. 66, is deduced from the general equation of the wave surface, by putting $z=0$, when it becomes—

$$(a^2x^2+b^2y^2)(x^2+y^2-(a^2+b^2)+a^4x^2+b^4y^2+a^2b^2c^2=0,$$

which may be resolved into the two factors—

$$(a^2x^2+b^2y^2-a^2b^2)(x^2+y^2-c^2)=0, \qquad [51.]$$

being the equation of an ellipse and a circle combined. In like manner, making x nothing, we obtain the equation of the section through $b\ c$, Fig. 65—

$$(b^2y^2+c^2z^2-b^2c^2)(y^2+z^2-a^2)=0\,; \qquad [52.]$$

and making y nothing, that of the section through $a\ c$, Fig. 67—

$$(a^2x^2+c^2z^2-a^2c^2)(x^2+z^2-b^2)=0. \qquad [53.]$$

This last section is remarkable, as showing an intersection of the circle and ellipse. The intersection is necessary, because the diameter of the circle is the mean axis of elasticity$=b$, while the major and minor axes of the eclipse are the extreme axes of elasticity, a and c. The points of intersection, shown at N, N′, &c., are the inosculating points of the two nappes of the wave surface.

Since the velocity of ray propagation is measured by the radius of the wave surface, it is evident that, along the radii drawn to N, N′, &c., there may be two refracted rays having the same velocity. These lines have a peculiar optical interest. Their inclination to x, or a, the axis of greatest elasticity, (or the angle MCN) may be found from the equation (putting $\beta=$MCN,)

$$\tan\beta=\frac{\mathrm{MN}}{\mathrm{CM}}=\frac{\mathrm{MN}}{\mathrm{NO}}.$$

MN and NO are obtained by making both factors of the equation of the section, just given, simultaneously$=0$. The values of x and z which render this possible are the values of NO and NM. We have then,

$$x^2+z^2-b^2=0, \text{ and } a^2x^2+c^2z^2-a^2c^2=0.$$

from which we obtain, by elimination,

$$x^2=\frac{c^2(a^2-b^2)}{a^2-c^2}, \text{ and } z^2=\frac{a^2(b^2-c^2)}{a^2-c^2}$$

$$\text{Whence, } \frac{\mathrm{MN}}{\mathrm{NO}}=\frac{z}{x}=\pm\frac{a\sqrt{b^2-c^2}}{c\sqrt{a^2-b^2}}=\tan\beta, \qquad [54.]$$

which differs a little from the value found for the tangent of inclination of the normals to the circular sections. But these normals are the directions of *equal wave velocity;* and CN is the direction of *equal ray velocity.* These two directions are therefore not coincident, though nearly so.

The lines drawn through the centre and the points N and N′ are however the optic axes; for it is equality of ray velocity which makes an optic axis. But it is not true that the two rays whose velocities in CN are equal, can spring from the same incident ray. Herein there is an important difference between crystals of one axis, and those of two. In crystals of one axis, when it is possible for two rays whose planes of polarization are transverse to each other, to have a common path and common velocity, they both proceed, or may proceed, from the same original ray.

This is not so in crystals of two axes; and what is more, no single incident ray of common light, in this class of crystals, can give a single refracted one; for there are no common points of tangency, in which both nappes may be met by the same plane.

If a tangent plane be drawn to the wave surface parallel to one of the circular sections of the surface of elasticity, it will take the position of AD, DB, &c., in the figure; and will be tangent at once to the ellipse and the circle in the principal section through the axes.* If, then, (in the same figure,) AB represent a refracting surface, and N′C a ray of common light incident at C, in such a manner as to take the direction CQ‴ within the crystal, for the nappe whose section is circular, it will yield another ray, CP‴ for the nappe whose section is elliptical. These two rays will be polarized in planes transverse to each other. The directions of their respective molecular movements, and therefore the positions of their planes of polarization, may be inferred from the following considerations.

The circular form of the section QQ′Q″Q‴, shows that the velocity of the rays belonging to that section is equal in all directions. The molecular movements must therefore be affected by a constant elasticity. Their directions must accordingly be invariable. In order that these directions may remain invariable, while a ray moves as a radius vector in the plane QQ′, &c., they must be perpendicular to this plane, or parallel to *b*. Accordingly the ray CQ‴ is polarized in the plane of the section. The other ray, CP‴, is polarized at right angles to the plane of the section.

The radius, CQ‴, of the circular section is normal to the tangent plane AD. For the angle CQ‴A is a right angle, by the property of the circle. And the wave surface on opposite sides of the plane of the section is symmetrical. The molecular movements of the ray CP‴ are, therefore, *in* the plane, which, passing through the ray, is normal to the tangent plane. Or, if we draw a line joining the point of contact with the foot of the normal from the centre, this line will be the direction of molecular movement in the ray.

The proposition just stated may be generalized, and extended to all rays. In the case of CQ‴, the point of contact and the foot of the normal coincide; and any line drawn through Q‴ fulfils the required condition, leaving the direction

---

* The truth of this statement may easily be shown thus: Suppose ordinates to XX′, ZZ′, to be drawn from P′ and Q′. Let x and z represent the ordinates from P′, and x' and z' those from Q′. It is evident that the angle at C, where the tangent BC intersects the axis of x, which we will put $=a$, will have for tangent $\frac{\text{BC}}{\text{CC}}$. Also, that the same tangent $=\frac{z-z'}{x'-x}$. Put CC$=k$, BC$=k'$. Then, by the property of the ellipse, we have—

$$kx=c^2,\ kx'=b^2,\ k'z'=b^2,\ k'z=a^2.$$

$$\text{Hence, } k(x'-x)=b^2-c^2;\ \text{and } k'(z-z')=a^2-b^2.$$

Dividing the second of these equations by the first, member for member, we obtain—

$$\frac{k'(z-z')}{k(x'-x)}=\frac{a^2-b^2}{b^2-c^2};\ \text{or } \tan^2 a=\frac{a^2-b^2}{b^2-c^2};\ \text{and } \tan a=\pm\sqrt{\frac{a^2-b^2}{b^2-c^2}}.$$

But this (equation [48]) is the tangent of the inclination of the circular section of the surface of elasticity to a, the axis of greatest elasticity, which is the axis of x. It follows that a plane which, being normal to the section through the inosculating points of the wave surface, is tangent at once to the ellipse and the circle in that section, is parallel to one of the circular sections of the surface of elasticity.

indeterminate. We have seen, however, that the direction is, in this case, fixed by other considerations; and it is furthermore demonstrable, that, as the point of tangency approaches Q‴, the line joining it with the foot of its corresponding normal approaches perpendicularity to the principal section; and that, in the limit, when the two points unite, the perpendicularity becomes absolute.

In the discussion of the tangent plane AD, or DB, drawn parallel to one of the circular sections of the surface of elasticity, Sir William Hamilton made the remarkable discovery that the tangency is not confined to the points P and Q in the principal section; but that it extends throughout the circumference of a minute closed curve, sensibly circular, of which P and Q are only two points of the circumference. The point N′ is, therefore, the vertex of a conoidal or umbilical depression; and all the points of the circumference of the circle of contact are equally points in the wave front to which CQ‴ is normal, and which is parallel to the same circular section of the surface of elasticity to which the tangent plane is parallel. The annexed figure represents this little circle. As, in this, CQ is the normal to the circular section of the surface of elasticity, and CN* is the optic axis, we have—

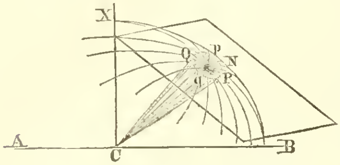

Fig. 68.

$$\tan QCX = \tan\alpha' = \pm\sqrt{\frac{b^2-c^2}{a^2-b^2}}, \text{ and } \tan NCX = \tan\beta = \pm\frac{a}{c}\sqrt{\frac{b^2-c^2}{a^2-b^2}}.$$

$$\text{Whence } \tan\alpha' = \pm\frac{c}{a}\tan\beta. \qquad [55.]$$

In anhydrous sulphate of lime (anhydrite) in which the doubly refracting power is uncommonly great, the ratio of c to a is .9725 to 1. The value of β is 14° $3\frac{1}{2}'$, from which we deduce $\alpha' = 13° \; 41' \; 11''$. And $\beta - \alpha' = 0° \; 22' \; 19''$.

A general expression for the value of $\beta - \alpha'$ may be found thus:

$$\tan\beta - \tan\alpha' = \left(\frac{a}{c} - 1\right)\tan\alpha' = \frac{a-c}{c}\tan\alpha'$$

$$\text{Or } \frac{\sin\beta}{\cos\beta} - \frac{\sin\alpha'}{\cos\alpha'} = \frac{a-c}{c}\,\frac{\sin\alpha'}{\cos\alpha'}$$

$$\sin\beta\cos\alpha' - \cos\beta\sin\alpha' = \frac{a-c}{c}\,\frac{\sin\alpha'}{\cos\alpha'}\cos\alpha'\cos\beta = \frac{a-c}{c}\sin\alpha'\cos\beta$$

$$\text{And } \sin(\beta - \alpha') = \frac{a-c}{c}\sin\alpha'\cos\beta. \qquad [56.]$$

In so far as the variation dependent on the trigonometrical function $\sin\alpha'\cos\beta$ is concerned, we may easily determine the outside limit. For, since α' is less than β, $\sin\alpha' < \sin\beta$; and $\sin\alpha'\cos\beta < \sin\beta\cos\beta$. But when $\sin\beta\cos\beta$ is at its maximum, $\cos^2\beta = \sin^2\beta = \sin^2 45° = \frac{1}{2}$. Therefore, $\sin\beta\cos\beta = \frac{1}{2}$ also. And $\sin\alpha'\cos\beta$ is always less than $\frac{1}{2}$. Hence, the sine of the angle between the optic axes and the normals to the corresponding circular sections is always less than half the difference between the greatest and least axes of elasticity, divided by the least axis.

Inasmuch as all the points of the little circle Q*q*P*p* are in the tangent plane, it follows that, if a ray should be incident upon a crystal in such a manner as that CQ should be its direction for one nappe and CP for the other, neither the ray CQ nor the ray CP would be confined to the point Q or P, but both would spread themselves along the circumference Q*q*P*p*, until, by blending together, they should form a hollow cone. And as, at the umbilical point, the

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* The letter N should stand at the centre of the conoidal depression in the figure.

tangents to the two nappes intersect each other, the direction CN will be the direction of a refracted ray, which will correspond to *different incident rays for the two nappes.* The annexed figure illustrates these propositions. Let ZZ′XX′ be the section of the wave surface through x, z, or a, c. Let APQ be the linear projection of the common tangent plane of both nappes, and N the umbilical point. Draw NA′, tangent at N, to the circular section, and NA″, tangent also at N, to the elliptical section. The radii of the wave surface being the measures of ray velocity in their several directions, as related to an assumed unit, which is the uniform velocity of light in vacuo, take AD, equal to that assumed unit, and from A, A′, and A″, where the several tangents cut the axis ZZ′ produced, describe the arcs aa, $a'a'$, $a''a''$. From C draw tangents to these arcs, CD, CD′, CD″. And from C again draw the perpendiculars, CR, CR′, CR″, to these tangents.

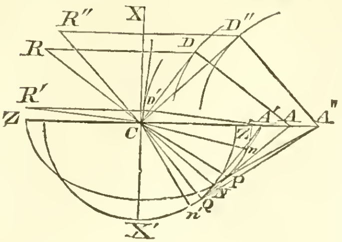

Fig. 69.

RC is the direction which an incident ray must have upon the surface ZZ′ of a crystal cut perpendicularly across the line intermediate between its optic axes, (which is the axis of its greatest elasticity,) in order that it may be refracted to P and Q;—a case in which, as we have seen, it will be refracted within the crystal in a hollow cone. At emergence (if the second surface is parallel to ZZ′) the emergent light will resume its original direction; and, as this will happen for every point of the circular base of the cone, the emergent beam will be a hollow cylinder.

The lines R′C, R″C are the directions of incidence of two rays, of which the first will send a refracted ray to N, belonging to the nappe whose section is circular; and the second will send another refracted ray to the same point, belonging to the nappe whose section is elliptical. Each of these will have a companion refracted ray which will not go to N. The companion of the first will take the direction Cn, found by drawing the tangent A′n to the ellipse; and that of the second will take the direction Cn', found by drawing the tangent A″n' to the circle.

The rays refracted to N will, on emergence, resume their parallelism to the incident rays R′C, R″C, and will therefore be divergent. Now, if it be considered that the umbilical points, N, are conoidal, it will be perceived that any plane passing through CN, will furnish two tangents like A′N, A″N, and therefore two incident rays, which will send corresponding refracted rays to N. It will accordingly be understood that a conical pencil of convergent rays, incident at C, will produce a conical pencil of divergent rays at its emergence from the opposite and parallel face of the crystalline plate. Also, though the incident cone of light be a solid cone, the emergent cone will be hollow. For, from the graphic construction by which the direction of refracted rays is determined, it is evident that none of the rays of the solid incident cone are refracted to N, except only those whose incident direction is R′C, R″C, &c., in the several azimuths around CN.

These propositions were deduced by Sir William Hamilton from the equation of the wave surface, before any phenomena of the kind had been observed or even suspected. At his request Dr. Lloyd made a careful study of a crystal of arragonite cut in the manner just supposed; and the result of his examination confirmed the theory in every particular. The success of the observation requires very delicate adjustments. Mr. Soleil, of Paris, has since constructed a small apparatus to facilitate the observation.

When the emergent cylinder or cone of rays is observed with an analyzer like Nicol's prism, one radius of the circle disappears. As the analyzer is turned in azimuth, this dark radius changes position, advancing in azimuth twice as fast as the analyzer. When the analyzer is in azimuth 90° from its original position, the dark radius is in azimuth 180° from *its* original position; when the analyzer has completed half a revolution, the dark radius has made a whole one. This singular fact is, however, easily explained. The analyzer suppresses that ray on whose plane of polarization it is *crossed;* and the planes of polarization of the rays in this small circle are, at opposite ends of every diameter, at right angles to each other. If QAPA′ be the small circle of tangency, Q being the point of contact with the circular section, or the foot of the normal, we have seen that the molecular movement from any other point of contact in this plane must be toward Q; that is, the movement at q must be in the direction qQ, that at r in the direction rQ, that at p in the direction pQ, &c. Suppose the direction of molecular movement in the analyzer to be AA, parallel to aQa. The vibrations in the ray at Q have the same direction. The analyzer allows that ray therefore freely to pass; but it is crossed on the ray at P whose direction of vibration is PQ. The radius, CP, will therefore be dark. Now let the analyzer take a position in azimuth A′A′, parallel to $a'a'$, tangent at r. Draw the diameter rCp. Draw Qsq perpendicular to rC. It will be parallel to $a'a'$, and it will be the direction of vibration of a ray at q. The analyzer in the position A′A′, is therefore in harmony with the ray q, and it is crossed on a ray whose molecular movements are in Cr, at right angles to Qq Join Qr, Qp, rq. The angle Qpr is equal to the angle Qqr, since both stand on the same arc, rQ. And the triangle rqs is similar to the triangle Qpr, the one being right angled by construction, and the other because it is inscribed in a semicircle. The triangle rqs is therefore similar and also equal to rQs, or the arc Qq is twice the arc Qr. Draw the diameter qp', and join Qp'. The angle qQp' is a right angle; consequently the molecular movements at p', being in the direction p'Q, parallel to Cr, and at right angles to qQ, or to $a'a'$ or A′A′, will be suppressed. In turning the analyzer in azimuth, therefore, from AA to $A'A'$, or through an azimuth measured by AA′=Qr, the dark ray has advanced through an azimuth Pp'=Qq=2Qr, which was the point to be proved.

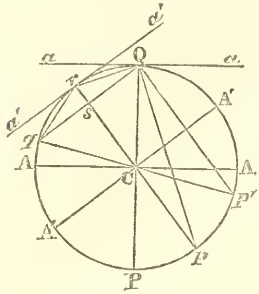

Fig. 70

In the equation of the wave surface, if b be the mean axis, and we make $c=b$, we shall have, after reduction—

$$(a^2x^2+b^2y^2+b^2z^2-a^2b^2)(x^2+y^2+z^2-b^2)=0. \qquad [57.]$$

If b and c remain unequal, and we put $b=a$, the equation is—

$$(a^2x+a^2y^2+c^2z^2-a^2c^2)(x^2+y^2+z^2-a^2)=0. \qquad [58.]$$

These are both equations of a spheroid and a sphere, touching each other at the poles. The first is that of an oblate spheroid circumscribing the sphere, and answers to the case of a negative crystal of one axis. The second is that of a prolate spheroid circumscribed by the sphere, and answers to the case of a positive crystal. The case of quartz, so remarkable on other accounts, is peculiar also in the fact that the two nappes of its wave surface are not in contact anywhere. The ellipsoid is entirely within the sphere, and there is no direction either of equal wave or of equal ray velocity.

These equations suggest the geometrical relations between the surface of elasticity and the wave surface. The larger diameters of the one are at right angles to the larger diameters of the other, and the smaller have the same relation. For crystals of one axis, the surface of elasticity is an ellipsoid of revolution. If its form is prolate, it generates an oblate wave; if it is oblate itself, the wave is prolate.

The causes of varying elasticity of the luminiferous ether within crystals are not well understood. They are dependent, in some manner, upon molecular arrangement. This is evident from the fact that variations resembling those which naturally exist in crystals may be produced, as we have seen, in homogenous bodies, by heat or by the force of pressure, flexure, or torsion. So delicate a test does the polariscope furnish of any inequality of temperature, stress, or mechanical force of any kind, that Dr. Brewster has suggested the construction of chromatic thermometers and dynamometers, founded on the principles we have endeavored to unfold, for determining differences of temperature, stress, or pressure too slight to be easily measured by ordinary instruments.

CONCLUSION.

In the review which we have now taken of the applications of the doctrine of undulation, we have encountered no optical phenomenon of which this doctrine does not furnish an explanation; we have discovered no legitimate deduction from it which has not found its verification in nature. We have seen, on the other hand, that it has served occasionally to point to facts of curious interest previously unknown, which have been subsequently confirmed by the experiments which it has suggested and directed; experiments which require for their exhibition adjustments so delicate and conditions so difficult to secure, that, but for the clew it has furnished, they would probably have remained forever unknown. This doctrine rises, therefore, above the level of a mere hypothesis; it fulfils every essential condition of a true theory; it explains all known phenomena; it anticipates the unknown, and its predictions are corroborated by experiment.

Moreover, the simplicity of the connecting link by which it binds together phenomena the most diverse in their nature, is almost without an example in the history of physical theories. In the words of Fresnel, "in order to calculate the so various phenomena of diffraction, those also of the rings produced by thin plates of air or water, or any other refracting medium, refraction itself, in which the ratio of the sine of the incident to the sine of the refracted rays is that of the lengths of the waves in the two media, the colors and the singular modes of polarization presented by crystalline laminæ, it is sufficient to know the lengths of undulation of light in the media which it traverses; this is the sole quantity which it is necessary to borrow from experiment, and it is the basis of all the formulæ. If we attend to those intimate and multiplied relations which the theory of undulations establishes between phenomena the most different, we cannot but be struck at once by its simplicity and its fecundity; and we are compelled to admit that, even though it had not the advantage over the system of emission of explaining numerous facts absolutely inconceivable in the latter, it would still merit the preference because of the means which it furnishes of connecting together all the phenomena of optics and embracing them in general formulæ."

It is not, indeed, to be denied that some embarrassments still attend this theory. There are physicists to whom the phenomena of dispersion still continue to be a stumbling-block, and the differences of opinion which exist in regard to the true relation between the direction of molecular movement in undulation and the plane of polarization of a polarized ray have been pointed out in their proper place; but these difficulties are such as, it is fairly presumable, the further progress of investigation will ultimately clear away, and are not sufficiently serious to impair confidence in the substantial truth of the theory as a whole. At any rate, whether this theory be received or not as a true representation of the operations of nature in optical phenomena, we are compelled to accept it at present as an instrument for combining and systematizing our knowledge of these facts and moulding it into a shape worthy of the name of science; since, if we reject this, there is nothing left us on which to fall back which is capable of rendering us the same service.

INDEX TO LECTURES ON THE UNDULATORY THEORY.

ERRATA.

Page 110, line 5 from bottom, for $\frac{q}{n\sqrt{1-n^2\sin^2\varphi}}$ read $\frac{q}{\sqrt{n^2-\sin^2\varphi}}$,

Page 112, line 16 from bottom, for mth read $(m+1)$st, in both instances.

LECTURES.

PHYSICAL ETHNOLOGY.

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PART I.

THE AMERICAN CRANIAL TYPE.

Among the novel questions to which the progress of science has given prominence, under aspects undreamt of in very recent years, is that of the relation of man to the inferior orders of being, and his true place in nature as one of the animal creation. In this respect, the investigations of the craniologist, and the whole bearings of physical ethnology, are now acquiring an interest and importance very partially accorded to them before. The geologist, who long ignored all that related to man and his works, as recent, and therefore without the pale of his comprehensive researches, now recognizes both his remains and his works of art as pertaining to the department of palæontology; and disputes with the archæologist the appropriation of the primitive flint-implements of the drift, once claimed exclusively by him for his Age of Stone. This has materially affected the aspects of the study of physical ethnology; for until very recently the differences between man and all other animals have been assumed to be so clearly defined, that the naturalist was long induced to overlook those which distinguish different races of men, and to regard any diversities of structure or relative proportions in the human form as mere variations from one common or ideal type. Nevertheless the craniologist, at the very commencement of his investigations, is led to recognize certain essential varieties of form; though still tempted, like Blumenbach, to refer all these to some "Caucasian" or other assumed highest type. Before, however, the ethnologist directed his attention to such researches, the artist had sought this type in the beautiful realizations of Greek sculpture; and by such means he determined the long-accepted statuary scale of the human head and figure. Nor can the influence of this artistic ideal be overlooked in the direction it gave to some of the speculations of the craniologist, and to the theoretical conception of the fully-developed man. It guided Camper in assigning the laws of his facial angle; controlled Blumenbach in his determination of the cranial peculiarities of leading races of men; and even influenced Prichard in his definition of the symmetrical or oval form of skull which he ascribed to his first division. Against the ideal canons of an antique statuary scale, however, some of the greatest modern masters protested; foremost of whom was Leonardo da Vinci, of whom Bossi remarks, "He thought but little of any general measure of the species. The true proportion admitted by him, and acknowledged to be of difficult investigation, is solely the proportion of an individual in regard to himself, which, according to true imitation, should be different in all the individuals of a species, as is the case in nature." In the features of the face there are the endless varieties of portraiture, controlled by family and national affinities, and so also in the varying proportions of the skull there appears to be an approximation in each race towards a special form. The craniologist accordingly finds in nature his brachycephalic or short skull; his dolichocephalic or long skull; his kumbecephalic or elongated (boat-shaped) skull; his pyramidal or acrocephalic; his platycephalic or flattened; his truncated, oval, and spherical skulls; as well as many intermediate forms. An idea, however, has long prevailed with reference to the aborigines of the New World, the origin of which is traceable to this distinguished American craniologist, Dr. Morton, that throughout the vast continent, from the arctic circle to the icy shores of Terra del Fuego, the Esquimaux constitutes the one exception to a predominant uniform cranial type.

The opinions advanced by one so distinguished and indefatigable in his study of the science as the author of the *Crania Americana* well merited the attention they have received, and might even seem to justify the assumption of them as indisputable scientific canons. Only one other authority could have carried any corresponding weight, and that is produced to confirm the conclusion referred to. "The nations of America," says Humboldt, "except those which border on the polar circle, form a single race, characterized by the formation of the skull, the color of the skin, the extreme thinness of the beard, and straight glossy hair."

Very few and partial exceptions can be quoted to the general unanimity of American writers—some of them justly regarded as authorities in ethnology—in reference to this view of the nations of the whole American continent, north and south, as one nearly homogeneous race, varying within very narrow limits from the prevailing type, and agreeing in so many essentially distinctive features, as to prove them a well-defined variety, if not a distinct species, of the genus *Homo*. Lawrence, Wiseman, Agassiz, Squier, Gliddon, Nott and Meigs, might each be referred to in confirmation of this opinion, and especially of the prevailing uniformity of certain strongly-marked cranial characteristics; but in reality the most of them only echo the opinions of Dr. Morton, and reproduce conclusions which his laboriously-accumulated evidence was supposed to have established beyond dispute. His views underwent considerable modification on some points relating to the singular cranial conformation observable in certain skulls found in ancient American graves; especially in reference to the influence of artificial means in perpetuating changes of form essentially different from the normal type; but the tendencies of his matured opinions all went to confirm his original idea of universal approximation to one cranial type throughout the New World. In his final contribution to his favorite science, on "The Physical Type of the American Indians,"* his matured conclusions relative to the cranial type of the American continent are thus defined: "The Indian skull is of a decidedly rounded form. The occipital portion is flattened in the upward direction, and the transverse diameter, as measured between the parietal bones, is remarkably wide, and often exceeds the longitudinal line.† The forehead is low and receding, and rarely arched, as in the other races; a feature that is regarded by Humboldt, Lund, and other naturalists, as a characteristic of the American race, and serving to distinguish it from the Mongolian. The cheek-bones are high, but not much expanded; the maxillary region is salient and ponderous, with teeth of a corresponding size, and singularly free from decay. The orbits are large and squared, the nasal orifice wide, and the bones that protect it arched and expanded. The lower jaw is massive and wide between the condyles; but not-

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* Schoolcraft's History of Indians, vol. II, p. 316.

† No such excess of the parietal over the longitudinal diameter is ever found except in a greatly distorted flathead or other artificially deformed skull; and of this only one example occurs in the *Crania Americana*.

withstanding the prominent position of the face, the teeth are for the most part vertical."

The views thus set forth, on such high authority, have exercised an important influence on all subsequent investigations; of which, perhaps, no instance is more illustrative than that of Stephens, who submitted to Dr. Morton the broken fragments of a skull rescued by him from an ancient grave in the ruins of Ticul. and though the observant traveller had already noted essential differences of ethnical characteristics between the physiognomies and head-forms sculptured on the ruins of Central America and those of the living race of Indians, he appears to have implicitly resigned his judgment to the homogeneous theory of Dr. Morton, and reproduces his opinion of the skull as that of a female, presenting "the same physical conformation which has been bestowed with amazing uniformity upon all the tribes on the continent, from Canada to Patagonia, and from the Atlantic to the Pacific ocean"*

This supposed prevalence of a remarkable uniformity of cranial conformation throughout tribes occupying forest, prairie, mountain plateau, or oceanic archipelago, and ranging from the arctic circle through every degree of latitude almost to the antarctic circle, being assumed as an established truth, has furnished the basis for further deductions of an equally comprehensive kind. Professor Agassiz, in discussing the provinces of the animal world and their relation to the different types of man, points out certain physical features of the western hemisphere which tend to adapt it for a much more uniform distribution of fauna than the European, Asiatic, and African regions present in corresponding latitudes. "The range of mountains which extends in almost unbroken continuity from the Arctic to Cape Horn, establishes a similarity between North and South America which may be traced also to a great degree in its plants and animals. Entire families which are peculiar to this continent have their representatives in North as well as South America—the cactus and didelphis, for instance; some species, as the puma or American lion, may even be traced from Canada to Patagonia. Thus, with due qualification, it may be said that the whole continent of America, when compared with the corresponding twin continents of Europe—Africa or Asia—Australia is characterized by a much greater uniformity of its natural productions, combined with a special localization of many of its subordinate types. With these facts before us, we may expect that there should be no great diversity among the tribes of man inhabiting this continent; and, indeed, the most extensive investigation of their peculiarities has led Dr. Morton to consider them as constituting but a single race, from the confines of the Esquimaux down to the southernmost extremity of the continent."† That the elements of diversity dependent on physical geography and the consequent influences of climate on food, temperature, &c., by which the distribution of the fauna of every region is controlled, are much less varied throughout the American continent than elsewhere is indisputable. But the effects of this comparative uniformity, or rather smaller range of diversity of climate and physical influences, in so far as they control the distribution of plants and animals, differ essentially from the operation of the same causes in producing an apparent uniformity among the tribes of men inhabiting the same continent. Fin, Celt, German, Sclave, Magyar, and Turk, all present as great a superficial resemblance as the diverse tribes and nations of the New World, where they have been long subjected to the same equalizing influences of climate, social intercourse, and intermingling of blood. But the ethnologist still finds the osteological indices of diversity of race unobliterated; and the results of the investigations set forth here have sufficed to satisfy me that the same diversity is still traceable among the ancient and living tribes and nations of this continent.

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* Stephens's Yucatan, vol. I., p. 284

† Provinces of the Animal World, &c. Types of Mankind, p. lxix.

Whilst, however, the supposed unity in physical form asserted by Dr. Morton, and accepted as an established scientific truth in relation to the races of man in the New World, has been reiterated on many occasions, its originator was not unaware that it was, at most, only an approximation to his assumed type, and was subject to variations of a very marked kind; although he did not allow their just weight to these when determining the conclusions which seemed legitimately to result from his carefully accumulated data. He thus remarks, in his *Crania Americana*, on certain unmistakable diversities of form into which the assumed American cranial type may be subdivided, when classing the so-called *barbarous nations:* "After examining a great number of skulls, I find that the nations east of the Alleghany mountains, together with the cognate tribes, have the head more elongated than any other Americans. This remark applies especially to the great Lenapé stock, the Iroquois, and the Cherokees. To the west of the Mississippi we again meet with the elongated head in the Mandans, Ricaras, Assinaboins, and some other tribes."* The Minetaries, Crows, Blackfeet, and Ottoes are named along with those in his latest reference to the subject, thereby transferring the Ottoes from the brachycephalic to the dolichocephalic class, in which he had previously placed them; for, to his earlier statement, Dr. Morton superadds the further remark: "Yet even in these instances the characteristic truncature of the occiput is more or less obvious, while many nations east of the Rocky mountains have the rounded head so characteristic of the race, as the Osages, Ottoes, Missouris, Dacotas, and numerous others. The same conformation is common in Florida; but some of these nations are evidently of the Toltecan family, as both their characteristics and traditions testify. The heads of the Caribs, as well of the Antilles as of *terra firma*, are also naturally rounded; and we trace this character, as far as we have had opportunity for examination, through the nations east of the Andes, the Patagonians, and the tribes of Chili. In fact, the flatness of the occipital portion of the cranium will probably be found to characterize a greater or less number of individuals in every existing tribe from Terra del Fuego to the Canadas. If their skulls be viewed from behind, we observe the occipital outline to be moderately curved outward, wide at the occipital protuberances, and full from those points to the opening of the ear. From the parietal protuberances there is a slightly curved slope to the vertex, producing a conical or rather a wedge-shaped outline." These opinions are still more strongly advanced in Dr. Morton's most matured views, where he affirms the American race to be essentially separate and peculiar, and with no obvious links, such as he could discern, between them and the people of the Old World, but a race distinct from all others.

Some of the uniform features above referred to, and especially the flattened occiput, are the product, as I believe, not of the approximation to any typical form of skull, but of the subjection of the living head to the same artificial compression, with a nearly uniform result. But this department of the subject will come under review at a later stage. The views now set forth relative to the American cranial type are founded on an extensive series of observations originally commenced in Canada, without any design to challenge the opinions set forth by the author of the *Crania Americana*, and subsequently reiterated by other distinguished American ethnologists. After having devoted minute attention to some departments of primitive British craniology, my removal to Canada placed within my reach opportunities of judging for myself of the physical characteristics of the aboriginal occupants of the American forests and prairies, and I availed myself at first of those in the full anticipation of meeting with such evidences of a general approximation to the assigned normal American cranial type, as would confirm the deductions of previous observers. My chief

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* Crania Americana, p. 65; Physical Type of the American Indians; History of Indina Tribes, vol. ii, p. 317.

aim, indeed, originally was to acquire specimens of skulls approximating to the peculiar brachycephalic type found in one important class of early British graves. It was, accordingly, simply with a sense of disappointment, that I observed the results of repeated explorations in different cemeteries furnish crania which, though undoubtedly Indian, exhibited little or no traces of the rounded form with short longitudinal diameter, strikingly apparent in certain ancient Mexican and Peruvian skulls, as well as in the rare examples hitherto recovered from the mounds of the Mississippi valley. Slowly, however, the conviction forced itself upon me that to whatever extent this assigned typical skull may be found in other parts of the continent, those most frequently met with along the north shores of the great lakes are deficient in some of its most essential elements. Similar conclusions have been recorded by different observers. They are indicated by Dr. Latham, when comparing the Esquimaux and American Indian forms of skull, as determined by Dr. Morton;* and have since been strongly affirmed by Dr. Retzius, who states that it is scarcely possible to find a more distinct separation into dolichocephalic and brachycephalic races than in America;† nor should the remark of Professor Agassiz be overlooked, when, after referring to Dr. Morton's single American race, he adds: "But it should be remembered that in accordance with the zoological character of the whole realm, this race is divided into an infinite number of small tribes, presenting more or less difference one from another." It is indeed necessary to determine what must be regarded as the essential requisites of Dr. Morton's American typical cranium; for neither he nor his successors have overlooked the fact of some deviation from this supposed normal type, not only occurring occasionally, but existing as a permanent characteristic of some tribes. As has been already shown, Dr. Morton recognized a more elongated head as pertaining to certain of the northern tribes, but this he speaks of as a mere slight variation from the more perfect form of the normal skull; and he adds: "Even in these instances the characteristic truncation of the occiput is more or less obvious."‡ So also Dr. Nott, after defining the typical characteristics of the American cranium, remarks: "Such are more universal in the Toltecan than the barbarous tribes. Among the Iroquois, for instance, the heads were often of a somewhat more elongated form; but the Cherokees and Choctaws, who, of all barbarous tribes, display greater aptitude for civilization, present the genuine type in a remarkable degree. My birth and long residence in southern States have permitted the study of many of these living tribes, and they exhibit this conformation almost without exception. I have also scrutinized many Mexicans, besides Catawbas of South Carolina, and tribes on the Canada lakes, and can bear witness that the living tribes everywhere confirm Morton's type."§

In selecting a skull, which seemed to Dr. Morton in all respects to fulfil the theoretical requirements of his typical cranium, we are guided, under his directions, to that ancient people who, in centuries long prior to the advent of Europeans, originated some remarkable traits of a native civilization in the valleys of the eastern tributaries of the Mississippi. It will, therefore, coincide with his choice of an example of the true American head, if, starting from that ancient race, we pursue our comparisons downward to the nations and tribes familiar to Europeans by direct intercourse and personal observation.

Among the most prized crania in the collection of the Academy of Natural Sciences at Philadelphia is the celebrated Scioto mound skull, familiarly known to many by means of the views of it introduced among the illustrations of

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[*] Natural History of the Varieties of Man, p. 453.

[†] Arch des Sciences Naturelles, Geneva, 1860. The views set forth here were first published by the author at the meeting of the American Association for the Advancement of Science, in 1857. *Vide* Edin. Philosoph. Journal N. S., vol. vii.

[‡] Crania Americana, p. 69; History of Indian Tribes, vol. ii, p. 317.

[§] Types of Mankind, p. 441.

Messrs. Squier and Davis's "Ancient Monuments of the Mississippi Valley." A careful examination of the original, however, brings out features of this remarkable skull, by no means apparent in the engravings. The vertical view, especially, is inaccurate. In the original it presents the peculiar characteristics of what I have before designated the truncated form: passing abruptly from a broad flattened occiput to its extreme parietal breadth, and then tapering, with slight lateral swell, until it reaches its least breadth, immediately behind the external angular processes of the frontal bone. The occiput has been subjected to the flattening process to a much greater extent than is apparent from the drawings; but at the same time it is accompanied by no corresponding affection of the frontal bone, such as inevitably results from the procedure of the Chinooks and other Flathead tribes; among whom the desired cranial deformation is effected by bandages crossing the forehead and consequently modifying the frontal as much as the parietal and occipital bones. On this account, great as is the amount of flattening in this remarkable skull, it is probably due solely to the undesigned pressure of the cradle-board acting on a head of markedly brachycephalic proportions and great natural posterior breadth. The forehead is fully arched, the glabella prominent, and the whole character of the frontal bone is essentially different from the Indian type. The sutures are very much ossified, and even to some extent obliterated.

The "Scioto mound cranium," the best authenticated and most characteristic of the crania of the mound-builders, when discovered, lay embedded in a compact mass of carbonaceous matter, intermingled with a few detached bones of the skeleton and some fresh-water shells. Over this had been heaped a mound of rough stones, on the top of which, incovered by the outer layer of clay, lay a large plate of mica, that favourite material of the ancient mound-builders. This is the skull which, according to the description of Dr. Morton, furnishes the best example of the true typical American head. It is produced as such by Dr. Nott, in the *Types of Mankind*, and, as described by Dr. Morton, "it is, perhaps, the most admirably formed head of the American race hitherto discovered. It possesses the national characteristics in perfection, as seen in the elevated

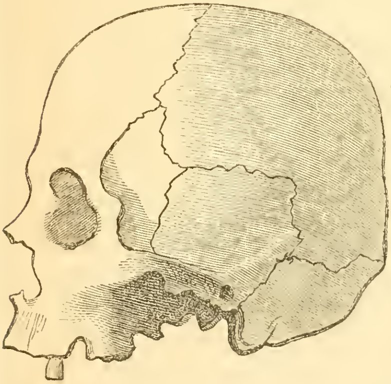

Fig. 1.

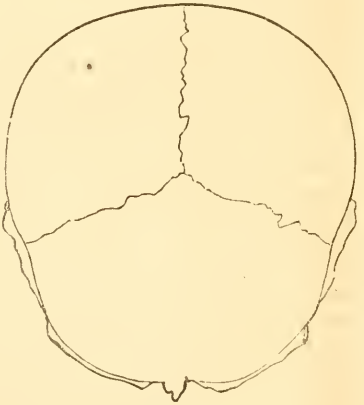

Fig. 2.

vertex, flattened occiput, great interparietal diameter, ponderous bony structure, salient nose, large jaws and broad face. It is the perfect type of Indian conformation, to which the skulls of all the tribes from Cape Horn to Canada more or less approximate."

Of this skull the measurements which involve the most essential typical elements, and so furnish precise materials for comparison, are—

Longitudinal diameter	6.5	inches.
Parietal diameter	6.	"
Vertical diameter	6.2	"
Inter-mastoid arch	16.	"
Horizontal circumference	19.8	"

So that, in fact, the cranium very closely corresponds in its measurements, in length, breadth, and height. Still further, it may be noted that the singular longitudinal abbreviation of this skull is nearly all posteriorly. A line drawn through the auditory foramen in profile, parallel to the elevated forehead, divides it into two unequal parts, of which the anterior and posterior parts are nearly in the ratio of three to two. If, however, we turn from the definition of the American typical form, as recorded in relation to this particular skull, and reduce it to the general formulæ derived by its originator from the examination of numerous examples, it amounts to this: A small receding forehead, somewhat broad at the base, but with a greatly depressed frontal bone; a flattened or nearly vertical occiput; viewed from behind, an occipital outline which curves moderately outwards, wide at the occipital protuberances, and full from these points to the opening of the ear; from the parietal protuberances a slightly curved slope to the vertex, producing a wedge-shaped outline; a great vertical diameter, and the predominant relative interparietal diameter of the brachycephalic cranium. If to those are added the large quadrangular orbits, the cheek-bones high and massive, the maxillary region salient and ponderous, and the nose prominent, we have, nearly in Dr. Morton's own words, the characteristic features of that American cranium which prevails among both ancient and modern tribes of the brachycephalic type, and has been assumed by him as universal.

It is with great diffidence that I venture to challenge conclusions adopted after mature consideration by the distinguished author of the *Crania Americana*. The frontal bone of the Scioto mound skull is by no means depressed, but well arched, and the flattened occiput bears unmistakable evidence of an artificial origin. The conical or wedge-shaped vertex of the Indian head is very partially traceable in the original, even when viewed from behind, and, altogether, when tried by Morton's own standard, it differs greatly from the American typical cranium. The same skull has been selected, by Dr. J. C. Nott,* for the purpose of instituting a comparison with the well developed and characteristic head of a modern Indian, a Cherokee chief, who died while a prisoner at Mobile in 1837, and the two crania are there engraved side by side, with other examples, "to show, through faithful copies, that the type attributed to the American races is found among tribes the most scattered; among the semi-civilized and the barbarous; among living as well as among extinct races; and that no foreign race has intruded itself into their midst, even in the smallest appreciable degree."†. But, judging merely by the reduced profile drawings, placed in juxtaposition, without reference to precise measurements, the points of agree-

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* *Types of Mankind*, p. 442.

† Dr. Nott's definition is as follows: "The most striking anatomical characters of the American crania are, small size; low, receding forehead; short antero-posterior diameter; great inter-parietal diameter; flattened occiput; prominent vertex; high cheek-bones; ponderous and somewhat prominent jaws."—*Types of Mankind*, p. 441.

ment are very partial. The vertical occiput of the ancient skull rounds somewhat abruptly into a flat horizontal vertex, and with the well developed forehead and short longitudinal diameter, gives a peculiar square form to it in profile. In the modern skull, on the contrary, the occipital flattening is not so much that of the occiput proper as of the posterior part of the parietal, together with the upper angle of the occipital bone; thereby uniting with the receding forehead of the latter, to produce a conoid outline, in striking contrast to the square form of the other. Still further, a vertical line drawn through the auditory foramen shows a remarkable preponderance of posterior cerebral development in the ancient skull, constituting indeed its most striking peculiarity. But a comparison of the measurements of the two skulls serves no less effectually to refute the supposed correspondence adduced in proof of a typical unity traceable throughout tribes and nations of the western hemisphere the most widely separated alike by time and space.

	Ancient.	Modern.
Longitudinal diameter	6.5	6.9
Parietal	6.0	5.7
Vertical	6.2	5.4
Frontal	4.5	4.6
Inter-mastoid arch	16.0	15.5
Inter-mastoid line	4.5	4.75
Occipito-frontal arch	13.8	14.4
Horizontal circumference	19.8	20.4

It is not to be supposed that any single skull can be selected as the embodiment of all the essential typical characteristics either of the ancient or the modern cranial conformation; nor can we deduce general conclusions as to the physical characteristics of the ancient mound-builders from the remarkable example above referred to. We lack, indeed, sufficient data as yet for any absolute determination of the cranial type of the mounds; but the Scioto mound skull cannot with propriety be designated as "the only skull incontestably belonging to an individual of that race." The Grave creek Mound cranium, figured by Dr. Morton, belongs no less indisputably to the same race, and presents in its arched forehead, prominent superciliary ridges, and compact, uniformly rounded profile, a general correspondence to the previous example.* In 1853 Dr. J. C. Warren exhibited to the Boston Natural History Society the cast of a second and more perfect skull from the same mound,† which I have since examined and measured in the collection of Dr. J. Mason Warren. It is also worthy of note that several inferior maxillary bones of the mound skeletons have been recovered nearly entire. They are remarkable for their massiveness, but are described as less projecting than those pertaining to the skeletons of a later date.‡ Another skull figured by Dr. Morton, from a mound on the Upper Mississippi, was obtained from an elevated site bearing considerable resemblance to that where the Scioto valley cranium was found, but the evidence is insufficient to remove the doubts which its proportions suggest, that in this, as in so many other cases, we have only one of those later interments habitually made by the modern Indians in the superficial soil of the mounds. It is better, meanwhile, to reject all doubtful specimens than to incur the risk of cumbering such well-authenticated evidence as we may anticipate with uncertainty and confusion. The following table includes a series of measurements of mound and ancient cave crania, mostly taken by myself from the originals in the collection of the Academy of Natural Sciences at Philadelphia and elsewhere:

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* *Crania Americana*, pl. liii, p. 223.

† *Proceedings of Boston Natural History Society*, vol. iv, p. 331.

‡ *Ancient Monuments of the Mississippi Valley*, p. 290.

TABLE I.—MOUND AND CAVE CRANIA.

	Locality.	L. D.	P. D.	F. D.	V. D.	I. A.	I. L.	O. F. A.	H. C.
1	Scioto Mound..........	6.5	6.0	4.5	6.2	16.0	4.5	13.8	19.8
2	Grave Creek Mound...	6.6?	6.0	......	5.0	....	......	14.2?	....
3	....do................	6.6	6.0	4.0	5.4	15.6	4.3	....	20.2
4	Tennessee Mound.....	6.6	5.6	4.1	5.6	15.2	4.4	14.0	19.5
5	Huron River, Ohio....	6.7	5.7	4.0	......	14.8	4.4	14.?	19.8
6	....do......... (Fem.).	6.7	5.4	4.0	5.4	14.0	4.2	13.7	19.9
7	Ohio Mound.. (Fem.).	6.4	5.3	4.0	5.0	14.2?	4.?	....	19.0
8	Alabama Mound.......	6.2	5.4	4.3	4.9	14.6	3.8	13.3	18.5
9	Golconda Cave	6.7	5.4	4.3	5.5	14.5	4.1	14.0	19.3
10	Steubenville Cave......	7.0	6.1	4.6	5.6	15.5	4.3	14.0	20.5
11	....do................	6.8	5.9	4.4	5.7	15.5	4.5	14.4	20.5
12	....do................	6.3	5.9	4.9	5.7	15.8	5.0	14.1	20.0
13	....do................	6.6	6.0	4.6	5.1	14.6	4.2	13.3	20.0
14	....do..............F ..	6.6	5.4	4.3	5.1	14.?	4.3	13.9	19.0
15	....do................	7.0	5.8	4.5	5.5	14.9	4.5	14.4	20.3
16	....do................	6.7	6.0	4.5	5.7	15.4	4.7	14.1	20.3
17	....do..............F ..	6.2	6.1	4.5	4.9	15.?	4.?	13.3	19.4
18	....do................	7.1	5.7	4.6	5.0	15.0	4.4	14.2	20.2
19	....do................	6.2	6.0	4.5	5.5	14.8	4.0	13.2	19.4
20	Kentucky Cave........	6.1	5.4	4.4	5.6	14.5	4.4	13.6	18.4
21	....do................	6.7	5.5	4.5	6.2	13.5	5.0	....	19.7
	Mound Crania mean..	6.54	5.67	4.13	5.36	14.91	4.23	13.83	19.53
	Cave Crania mean....	6.62	5.78	4.51	5.47	14.85	4.42	13.87	19.77
	Total mean...........	6.58	5.74	4.37	5.43	14.87	4.35	13.86	19.68

Of the series embraced in this table, though all are ancient, only the first four can be relied upon as undoubted examples of the crania of the mounds. In comparing them with others, there are indications of a peculiar cranial type partially approximating to the brachycephalic Peruvian cranium; but this assumed correspondence has been exaggerated, and some important differences have been slighted or ignored in the zeal to establish the affinities which such an agreement would seem to imply. In vertical elevation the Peruvian cranium is decidedly inferior; and another point of distinction, borne out by the few well-authenticated mound crania, is the well-formed and arched frontal bone, unaffected by the pressure to which the flattened occiput must be in part ascribed, and accompanied by great prominence of the superciliary ridges. These differences were overlooked by Dr. J. C. Warren, who pronounced the Mound and Peruvian crania to be identical. A greater correspondence seems to me to be traceable between the most ancient crania of the Mexican valley and those of the mounds. But, tempting as are the conclusions which such analogies suggest, any final decision on the subject must be reserved until further discoveries place within our reach a sufficient number of skulls of the ancient Mound-builders as well authenticated as those of the Scioto valley and Grave creek mounds. This there is little hope of achieving, until a systematic exploration is instituted under the direction of a carefully constituted scientific commission, the organization of which would reflect credit on the government of the United States. The Cave crania, Nos. 9–21, are a remarkable series of undoubted antiquity, and present a nearer approximation to those of the Mounds than any other class. Their most notable divergence from the mound type, in the parietal diameter, disappears if the doubtful examples of the latter, Nos. 5–8, are excluded, as in Table X.

Turning from this review of the meagre data hitherto recovered from the ancient sepulchral mounds, let us next consider the two great civilized nations of the New World, the Peruvians and Mexicans. Their civilization had an independent origin and growth. The scenes of its development were distinct; and each exhibited special characteristics of intellectual progress. Nevertheless, they had so much in common, that the determination of the physical type peculiar to each will be best secured by ascertaining what is common to both.

When Dr. Morton first undertook the investigation of the cranial characteristics of the American races, he admitted the force of the evidence presented to him in the examination of a number of ancient Peruvian skulls, and has recorded in his *Crania Americana* a distinct recognition of the traces of well-defined brachycephalic and dolichocephalic races among the ancient Peruvians.* But the seductive charms of his comprehensive theory of an American ethnic unity ultimately prevailed over the earlier opinion, which, even in the *Crania Americana*, was stated as the legitimate deduction from the evidence in question, without being incorporated into the author's concluding propositions; and he accordingly states his conviction that all the extremest varieties of the Peruvian head were naturally of the same rounded shape, and owe their diversities of form to artificial deformation. In this, as in others of the deductions drawn by Dr. Morton from the carefully accumulated data which his well-directed industry contributed to the science, it is obvious that his mind dwelt too exclusively on one or two of the leading characteristics of the more numerous varieties of American crania; and, like others who have satisfied their minds in regard to one central type, he evaded every variation from it, by assuming it as a mere exceptional aberration.

A revision of the evidence accumulated by Dr. Morton, along with additional illustrations derived from other sources, suggests conclusions in reference to Peruvian cranial forms at variance with the idea of a universally prevalent rounded, or brachycephalic Peruvian head. In pursuing my researches on this subject, I have enjoyed the advantage of minutely studying and measuring an interesting collection of crania and mummied bodies, brought by John H. Blake, esq., of Boston, from ancient Peruvian cemeteries on the shores of the Bay of Chacota, in latitude 18° 30′ S. In addition to those the following tables of Peruvian crania include measurements made from others, in the collections of Dr. J. M. Warren and the Natural History Society of Boston; in that of the Academy of Natural Sciences of Philadelphia, and of the Smithsonian Institution, Washington. The materials upon which Dr. Morton based his final opinion that the dolichocephalic crania found in ancient Peruvian graves derive their form and proportions from artificial causes, and consequently that these have no ethnical significance, are still accessible; and the bearings of the additional evidence since accumulated justify a reconsideration of the proofs. Since the subject was taken up by him the effects, not only of designed, but also of undesigned artificial compression, and of posthumous distortion, on cranial forms, have been minutely studied. The application of continuous pressure on the skull during infancy can be carried so far as to obliterate nearly every trace of its normal proportions. But it cannot substitute for them a symmetrical artificial conformation. Even comparatively slight pressure is betrayed by a corresponding amount of inequality in the opposite sides of the head; and when the compression is such as would be required to convert a brachycephalic head, averaging 6.3 in longitudinal diameter, by 5.3 in parietal diameter, into a dolichocephalic head of 7.3 by 4.9 in diameter, the retention of anything like the normal symmetry is impossible. The following table of measurements illustrates the proportions of the Peruvian brachycephalic skull:

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* *Crania Americana*, p. 98.

TABLE II.—PERUVIAN BRACHYCEPHALIC CRANIA.

	Locality.	L. D.	P. D.	F. D.	V. D.	I. A.	I. L.	O. F. A.	H. C.
1	Atacama	6.0	5.2	3.5	5.2	......	......	......	......
2	do	6.3	5.0	3.5	5.3	......	......	......	......
3	do	6.6	5.3	3.4	5.3	......	......	......	......
4	do	6.7	5.6	3.6	5.4	......	......	......	......
5	S. of Arica	6.1	5.6	3.4	5.1	14.6	4.1	......	18.4
6	do	6.4	5.1	3.2	5.1	14.5	4.1	......	19.0
7	Peru	6.2	5.8	3.7	5.6	15.1	4.2	......	19.1
8	Lima	6.3	5.8	3.6	5.4	15.6	4.2	......	19.7
9	Titicaca	6.3	5.9	4.0	5.3	16.0	4.1	......	19.2
10	do (145)	6.2	5.9	3.4	5.0	14.7	4.3	......	20.1
11	do (146)	6.5	5.9	4.0	5.3	15.5	4.9	......	19.5
12	Arica	6.5	5.2	4.3	5.1	14.5	4.0	13.8	18.5
13	Temple of Sun, F	5.8	5.7	4.4	5.1	14.5	4.1	12.7	18.4
14	do	6.1	6.0	4.7	5.5	16.0	4.5	14.1	19.5
15	Pachacamac	6.7	6.0	4.5	5.6	16.2	4.5	14.5	20.2
16	do	6.3	5.8	4.5	5.3	15.0	4.0	13.2	19.0
17	Santa	6.2	5.4	4.3	4.9	14.6	3.8	13.3	18.5
18	Rimac	6.5	5.6	4.5	5.0	14.7	3.8	13.2	19.2
19	Pachacamac, F	6.6	6.0	4.6	5.1	15.5	4.1	13.5	19.8
20	do	6.6	5.7	4.2	5.2	15.5	4.4	13.0	19.4
21	do F	6.3	5.5	4.2	5.0	14.5	3.7	13.2	18.5
22	do	6.3	5.3	4.4	4.6	14.0	3.9	13.0	18.7
23	do	6.4	5.5	4.3	5.2	14.8	4.0	13.2	19.0
24	do F	6.2	5.5	4.4	5.0	13.6	3.8	12.6	18.7
25	do F	6.1	5.9	4.6	5.2	15.2	4.1	13.2	19.2
26	do	6.2	5.8	4.3	4.9	14.5	4.1	12.6	18.7
	Mean	6.32	5.62	4.06	5.18	14.96	4.12	13.27	19.10

Of the diverse, elongated type of skull, undoubted examples have been repeatedly recovered from Peruvian cemeteries, both in their normal condition and modified by artificial means. They are nearly all small, narrow, and with a marked predominance of the longitudinal diameter. Several of those measured by me showed the average distance from a vertical line drawn from the external auditory foramen to the most prominent part of the frontal bone to be only 2.7 inches, while from the same line to the most prominent part of the occipital bone it was 4.3 inches. Fully two-thirds of the cavity occupied by the brain lies behind the occipital foramen, and the skull, when supported on the condyles, falls backward. Compared with brachycephalic skulls, the forehead is low and retreating; the temporal ridges approach near each other at the top of the head, a much larger space being occupied by the temporal muscles, between which the skull seems to be compressed. The zygoma is larger, stronger, and more capacious, and the whole bones of the face are more developed. The superior maxillary bone is prolonged in front, and the incisor teeth are in an oblique position. The bones of the nose are prominent, the orifices larger, and the cribriform lamella more extensive; the bony substance of the skull is thicker, and the weight greater.

Among the numerous interesting illustrations of Peruvian characteristics obtained by Mr. Blake from ancient cemeteries on the Pacific coast, the most valuable for the purpose now in view are the skulls of two children, both of the dolichocephalic or elongated type; but the one evidently in a normal condition, while the other betrays manifest traces of artificial deformation. It is impossible to examine the former without feeling convinced that it illustrates a type of head entirely distinct from the more common brachycephalic crania, while the latter shows the changes wrought by compression. Figure 3 exhibits the unaltered skull. It is that of a child, which, judging chiefly from the state of the dentition, may be pronounced to have been about seven years of age. It is a well-proportioned, symmetrical skull, unaltered by any artificial appliances, and will be observed to present the most striking typical contrast, if compared with an unaltered juvenile skull of the brachycephalic type from the Peruvian cemetery of Santa, engraved in the *Crania Americana*, Plate vii. The other elongated skull, exhibited in Figure 4, is manifestly of the same elongated type as Figure 3, but considerably altered by compression. The forehead is depressed, and the frontal suture remains open. It is that of a child of about five years of age; so that both examples are long past the age when the form of the head admits of material alteration by artificial means.

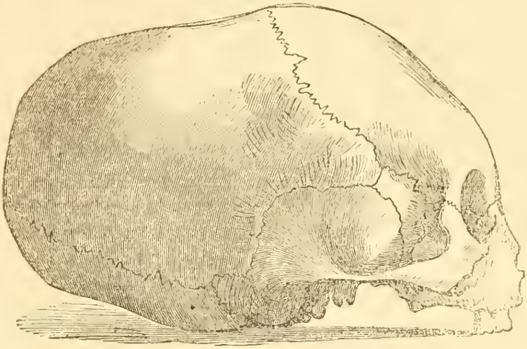

Fig. 3.

Fig. 4.

The following measurements give the comparative proportions of the normal and abnormal skulls figured above; and of two other children's skulls, in the Morton collection, figured in the *Crania Americana*, Plates ii and vii. They are marked, A, normal child's skull; B, abnormal do.; C and D, the Atacama and Santa skulls of the *Crania Americana*:

	A.	B.	C.	D.
Longitudinal diameter	6.6	6.1	6,9	5.4
Parietal diameter	4.6	4.4	4.5	5.4
Frontal diameter	3.3	3.1	3.7	4.
Vertical diameter	4.8?	4.3?	4.3	4.6

From observations carried on in the cemeteries of Peru, Mr. Blake was led to the conclusion that the distinguishing traits thus far noted between two classes of the ancient Peruvians are not limited to the crania, but may be discerned in other traces of their physical organization. In describing those of the rounded or brachycephalic type of cranium, he adds: "The bones of the latter struck me as larger, heavier, and less rounded than those of the former, (the elongated crania,) and in the larger size of the hands and feet they also present a noticeable difference. The remarkable narrowness and delicacy of the hands, and the long and regularly-formed finger-nails of the former, are strong evidence that they were unaccustomed to severe manual labor, such as must have been required for the construction of the great works of which the ruins remain. In all the cemeteries examined, where skulls of the rounded form have been found, those which are elongated have also been obtained." Remembering, however, that the sepulchral rites of the royal and noble Inca race were commonly accompanied by the same human sacrifices traceable among so many semi-civilized as well as barbarous nations, it is in no degree surprising that the crania of the two distinct classes, noble and serf, should be found deposited together in the same grave. After a minute comparison of all the brachycephalic Peruvian crania in the Morton collection, it appears to me that these also admit of subdivision into two classes distinguished by marked physiognomical diversity. The bones of the face in the one are small and delicate, while the other exhibits the characteristic Mongol maxillary development and prominent cheek-bones. The following table of measurements illustrates the proportions of the Peruvian dolichocephalic skull, as shown in examples brought by Mr. Blake from Peru, and in others preserved in the collections of Boston and Philadelphia:

TABLE III.—PERUVIAN DOLICHOCEPHALIC CRANIA.

	Locality.	L. D.	P. D.	F. D.	V. D.	I. A.	I. L.	O. F. A.	H. C.
1	Atacama	7.2	5.2	3.6	5.1	....	......	....	......
2	do.	7.3	4.9	3.3	4.9	....	......	....	......
3	do	7.0	4.7	3.2	5.1	....	......	....	......
4	do	7.1	5.2	3.2	5.0	14.1	4.0	15.0	20.0
5	S. of Arica	6.9	5.3	3.6	5.2	14.6	4.1	....	19.8
6	Peru	7.2	5.3	3.5	5.6	14.6	4.0	....	20.0
7	do	7.0	4.9	3.0	5.3	14.0	4.1	....	19.0
8	do ...... F	7.2	5.1	3.5	5.2	13.9	4.0	....	20.0
9	Arica	7.3	5.3	4.3	5.3	14.0	4.3	15.0	19.8
10	Atacama	7.2	5.5	4.4	5.1	14.8	4.1	13.7	20.2
11	Titicaca	6.8	5.4	4.8	5.3	14.8	4.2	....	19.4
12	Royal Tombs, F	6.8	5.2	3.8	5.3	14.1	4.0	....	19.4
13	Pachacamac	6.8	5.4	4.5	5.3	14.7	4.2	14.1	19.5
	Mean	7.06	5.18	3.80	5.21	14.36	4.10	14.45	19.71

In an inquiry into the physical characteristics of the Peruvian nation, we are by no means limited to the cranial or the mere osteological remains recoverable from its ancient cemeteries. Like the Egyptians, the Peruvians employed their ingenious skill in rendering the bodies of their dead invulnerable to the assaults of "decay's effacing fingers;" and, like the inhabitants of the Nile Valley, they were able to do so under peculiarly favorable circumstances of soil and climate. The colors on Egyptian paintings, and the texture of their finer handiwork, which have shown no trace of decay through all the centuries during which they have lain entombed in their native soil or catacombs, fade and perish almost in a single generation when transferred to the humid climates of Paris or London. The natural impediments to decay probably contributed, alike in Egypt and Peru, to the origination of the practice of embalming. The cemeteries already referred to are situated in a region where rain seldom or never falls; and the dryness alike of the soil and atmosphere, when added to the natural impregnation of the sand with nitrous salts, almost precludes the decay of animal or vegetable matter, and preserves the finest woollen and cotton textures, with their brilliant dyes undimmed by time. By the same means we are enabled to judge of the color and texture of the hair, the proportions and delicacy of the hands and feet, and the comparative physical development of two seemingly different races at various stages, from infancy to mature age. When we pass from the southern continent of America to the seats of ancient native civilization lying to the north of the Isthmus, a different class of evidence, in like manner, enlarges our range of observation. The artistic ingenuity of the ancient Peruvian potter has left valuable memorials of native portraiture, and the Mexican picture-writing, with the sculptures and terra-cottas, the products of Toltec and Aztec ceramic art, in like manner contribute important evidence illustrative of the physiognomy and physical characteristics of the ancient races of Anahuac. Still more, the elaborate sculptures and stuccoed bas-reliefs of Central America perpetuate in unmistakable characters the records of an ancient race, differing essentially from the modern Indian; and the study of their cranial characteristics serves to confirm the deductions derived from those other independent sources.

The traditions of the Mexican plateau pointed to the comparatively recent intrusion of the fierce Mexican on older and more civilized races; and various independent observers have at different times been tempted to trace associations between the ancient Mound-builders of the Ohio, the elder civilized race of Mexico, and the Peruvians, whose peculiar remains are recovered from the tombs around Lake Titicaca. The predominant Mexican race at the era of the conquest appears from evidence of various kinds, including the portraiture in ancient Mexican paintings, to have been derived from one of the great stocks of the Red Indians of the northern continent. The features represented in the paintings are thoroughly Indian, and strikingly contrast with those of the older native race of Central America, as illustrated by their sculptures, bas-reliefs, and pottery. No doubt, however, the population of the Mexican plateau in the time of Montezuma included descendants of very different races. All the traditions of Mexico point to intrusion and conquest by successive invaders; and the cranial evidence, as produced in the following tables, shows that there also, very distinctive types of skull-forms appear to perpetuate the evidence of diverse races, and of a mixed stock intermingling the characteristics of the conquering and the subject people. The same valuable American collections have furnished the materials for the following comparative tables:

TABLE IV.—MEXICAN DOLICHOCEPHALIC CRANIA.

	Locality.	L. D.	P. D.	F. D.	V. D.	I. A.	I. L.	O. F. A.	H. C.
1	Mexico	7.1	5.0	3.8	5.5	....	4.2	....	19.8
2	Otumba	7.1	5.6	4.6	5.5	15.5	4.1	15.0	20.2
3	Cerro de Quesilas	7.1	5.7	4.4	5.2	15.9	4.0	14.0	20.5
4	Acapacingo, F	6.9	5.2	4.2	5.4	14.5	4.1	14.0	19.2
5	Tacuba	7.1	5.6	4.5	5.4	15.2	4.3	14.2	20.0
6	....do	7.0	5.3	4.3	5.3	14.5	4.1	14.0	20.0
7	Mexico	7.0	5.4	4.3	5.3	15.0	4.1	14.0	19.8
8	....do	7.1	5.5	4.4	5.2	15.8	4.1	14.0	20.4
	Mean	7.05	5.41	4.31	5.35	15.20	4.12	14.17	19.99

TABLE V.—MEXICAN BRACHYCEPHALIC CRANIA.

	Locality.	L. D.	P. D.	F. D.	V. D.	I. A.	I. L.	O. F. A.	H. C.
1	Mexico	6.6	5.8	3.9	5.9	14.7	4.3	....	20.0
2	....do	6.6	5.7	4.0	......	15.0	......	14.5	20.0
3	Otumba	6.3	5.3	4.4	5.4	14.3	4.2	13.5	19.2
4	....do	6.6	5.3	4.4	5.4	14.0	4.0	14.0	19.3
5	Tacuba	6.8	5.5	4.6	6.0	15.6	4.4	14.6	19.9
6	San Lorenzo	6.4	5.7	4.5	5.4	14.6	4.5	13.5	20.2
7	Mexico, modern	6.6	5.3	4.3	5.2	14.6	4.1	13.6	19.0
	Mean	6.56	5.51	4.30	5.55	14.69	4.25	13.95	19.66

The Peruvians and Mexicans, with the ancient populations of Central America and Yucatan, constitute the Toltecan family of the two great divisions into which Dr. Morton divided his one American "race or species." The nations lying to the north of those seats of a native civilization were all classed by him into one family of the barbarous tribes, resembling the other in physical, but differing from it in intellectual characteristics. Yet, as we have seen, even Dr. Morton recognized some differences among them; and Professor Agassiz speaks of their tendency to split into minor groups, though running really one into the other. The following tables, however, will show that the differences are of a far more clearly defined nature, and in reality embrace well-marked brachycephalic and dolichocephalic forms; while of these, the latter seems decidedly the most predominant. The examples are chiefly derived from the Philadelphia collection, though with additional illustrations from the Boston cabinets already referred to, as well as from Canadian collections. Table VI, which illustrates the form of head most widely diverging in proportions from the theoretical type, shows in reality the prevailing characteristics of the north-eastern tribes, and could be greatly extended. The opposite or brachycephalic cranial formation is illustrated in Table VII.

TABLE VI.—AMERICAN DOLICHOCEPHALIC CRANIA.

	Tribe.	L. D.	P. D.	F. D.	V. D.	I. A.	I. L.	O. F. A.	H. C.
1	Seminole	7.1	5.6	4.7	5.5	15.0	4.1	14.8	20.3
2	....do	7.3	5.9	4.6	5.8	15.9	4.4	15.3	20.7
3	....do	7.0	5.6	4.7	5.4	15.0	4.1	14.7	20.2
4	....do	7.3	5.6	4.2	5.6	15.2	4.7	15.0	20.4
5	....do	7.0	5.9	4.5	5.8	14.7	4.6	14.2	20.5
6	Cherokee, F	7.2	5.2	4.2	5.5	14.5	4.0	14.6	20.2
7	....do.. F	7.0	5.3	4.1	5.4	14.5	4.0	14.0	19.5
8	....do	7.2	5.3	4.3	5.3	14.1	4.5	14.0	19.1
9	Choctaw	7.2	5.0	4.2	5.5	14.6	3.9	14.7	19.2
10	Sauk....F	7.4	5.9	4.6	5.5	15.3	4.7	14.2	20.2
11	Ottigamie	7.0	5.9	4.7	5.5	15.0	4.2	14.2	20.9
12	Chippewa	7.3	5.8	4.8	5.5	15.1	4.6	14.2	20.9
13	....do	7.2	5.5	4.3	5.5	14.8	4.1	14.6	20.2
14	Pottowatomie	7.8	5.7	4.4	5.3	16.0	4.0	15.8	22.1
15	Mississaga	7.0	5.2	4.3	5.2	13.8	4.1	14.2	19.5
16	Delaware	7.8	5.4	4.6	5.1	14.4	4.2	14.5	20.0
17	....do	7.0	5.5	4.4	6.2	15.6	4.3	16.0	21.5
18	Miami	7.6	5.3	4.3	5.5	15.0	4.1	15.5	20.5
19	....do	7.3	5.5	4.3	5.5	14.6	4.6	14.9	21.0

TABLE VI.—Continued.

	Tribe.	L. D.	P. D.	F. D.	V. D.	I. A.	I. L.	O. F. A.	H. C.
20	Naumkeag	7.4	5.5	4.4	5.9	15.0	4.3	14.0	......
21	....do	6.9	5.0	4.2	5.3	14.3	3.9	14.4	19.8
22	Assinaboine	7.6	5.8	4.6	5.1	14.9	4.3	14.9	21.2
23	....do	7.5	5.7	4.4	5.2	14.7	4.6	14.7	20.8
24	Mandan, F	7.1	5.4	4.3	5.1	14.2	3.8	14.6	20.0
25	....do... F	7.0	5.3	4.1	5.3	13.9	4.2	14.1	19.8
26	Ricari	7.0	5.2	4.1	5.1	13.5	4.0	14.0	19.5
27	Mingo	7.1	5.5	4.5	5.2	14.7	4.1	14.5	20.2
28	Menominee	7.1	5.8	4.1	5.5	14.7	4.0	....	20.3
29	....do	7.1	5.4	3.9	5.2	13.3	4.4	....	19.3
30	....do	7.5	5.4	4.0	5.5	14.5	4.2	....	20.6
31	Minetari, F	7.3	4.4	4.4	5.1	14.1	4.1	14.7	20.2
	Mean	7.24	5.47	4.36	5.42	14.67	4.23	14.62	20.29

TABLE VII.—AMERICAN BRACHYCEPHALIC CRANIA.

	Tribe.	L. D.	P. D.	F. D.	V. D.	I. A.	I. L.	O. F. A.	H. C.
1	Muskogee	6.8	5.8	4.2	5.6	15.4	4.3	15.0	20.0
2	....do	6.6	5.7	4.5	5.3	15.3	4.5	14.0	20.4
3	Ucheo	6.8	5.4	4.3	5.5	15.0	4.4	14.3	20.1
4	Minsi	6.7	5.0	4.2	5.3	14.0	4.1	13.8	19.3
5	Natick	6.7	5.2	4.1	5.7	14.5	4.1	14.3	19.0
6	....do	6.7	5.2	4.3	5.3	14.2	3.9	14.1	19.1
7	Dacota	6.7	5.7	4.2	5.4	14.7	4.4	13.5	19.8
8	....do	6.8	5.7	4.3	5.5	15.1	4.4	14.4	20.1
9	Pawnee, F	6.6	5.4	4.4	4.9	13.7	4.3	13.0	19.1
10	....do	6.6	5.5	4.1	5.4	15.0	4.4	14.0	19.5
11	....do	6.5	5.5	4.0	5.4	14.8	4.4	14.1	19.3
12	....do	6.7	5.6	4.3	5.5	15.1	4.4	14.2	19.6
13	Chetimachee	6.5	5.7	4.3	5.9	15.5	4.1	14.0	19.1
14	Chimuyan	6.5	5.4	4.2	5.2	14.3	3.8	13.4	18.8
15	Osage	6.6	5.7	4.3	5.2	14.8	4.7	13.8	19.5
16	....do	6.5	5.9	4.6	5.3	15.1	4.1	13.4	19.5
17	Creek	6.9	5.7	4.6	5.4	15.5	4.7	14.4	20.4
18	Choctaw	6.5	5.1	4.0	4.7	12.5	4.1	13.0	18.7
19	....do	6.4	5.1	4.0	5.1	14.0	4.0	....	19.7
20	"Ohio Mound," F	6.4	5.3	3.9	5.0	14.2	4.0	....	19.0
21	Goajiro	6.7	5.3	......	5.2	....	......	13.4	19.3
22	....do	6.5	5.1	......	4.9	....	......	13.0	18.5
	Mean	6.62	5.45	4.24	5.30	14.63	4.25	13.85	19.44

But I now turn to the region around the northern lakes, where opportunities of personal observation first suggested to me the obvious discrepancies between the actual evidence disclosed by exhumation on the sites of native sepulture, and the theory of a typical unity manifested in the physical and peculiar cranial characteristics of the most widely-separated tribes and nations of the American continent. The Scioto Mound skull, characterized by Dr. Morton as "the perfect type of Indian conformation to which the skulls of all the tribes from Cape Horn to Canada more or less approximate," presents the remarkable anterior development of a cranium whereof two-thirds of the cerebral mass was in front of the *meatus auditorius externus;* whereas in the elongated Peruvian skull, unaltered by artificial means, this is almost exactly reversed, showing by the proportions of the cerebral cavity that fully two-thirds of the brain lay behind the *meatus auditorius.* These may be considered as representing the two extremes; but both of the two great stocks, between whom the northern region around the great lakes has been chiefly divided since the first intrusion of Europeans, belong to the dolichocephalic division. These are the Algonquins and the Iroquois, including in the latter the Hurons, who, with the Petuns, Neuters, and Eries, all belonged to the same stock, though involved in deadly enmity with each other. In the supposed typical Scioto Mound skull the longitudinal, parietal, and vertical diameters vary very slightly; and as the Mexican and Peruvian crania chiefly attracted Dr. Morton's attention, and are illustrated minutely, as a series, in his great work, it only required the further theory, which referred all the elongated skulls to an artificially modified class, to confirm in his mind that idea of a peculiarly formed cranium pertaining uniformly and exclusively to the New World. To the theoretical type of a head very nearly corresponding in length and breadth, though not in height, the most numerous class of Peruvian and Mexican brachycephalic crania unquestionably approximate. Of one of the former, from the Temple of the Sun, (Plate xi,) Dr. Morton remarks: "A strikingly characteristic Peruvian head. As is common in this series of skulls, the parietal and longitudinal diameter is nearly the same," viz.: longitudinal, 6.1; parietal, 6.0; and, tested by this standard, he was even more justified in recognizing marked points of correspondence between the Mound skulls and what he calls "the Toltecan branch of the American race," than might seem reasonable from the miscellaneous character of the crania referred to by him as "Mound skulls." But the moment we test by actual measurement, a very wide difference is apparent between the brachycephalic crania of the class referred to, and the prevailing form of the head in many of the northern tribes, as among the Algonquins, Hurons, and Iroquois. The Algonquin stock are represented by Ottawas, Mississagas, Chippewas, and other tribes, within the area of Upper Canada and along the shores of Lake Superior. Of living Indians belonging to Iroquois and Algonquin tribes I have examined, and compared by the eye, many at widely-scattered places: on the Thames and Grand rivers, Rice lake, Lake Simcoe and the Georgian bay; at Mackinaw in Lake Huron, and at Sault Ste. Marie; at Ontonagon, La Point, the Apostle islands, and the St. Louis river, on Lake Superior; and on the Saguenay, St. Charles, St. Maurice, and Ottawa rivers, in Lower Canada; as well as on such chance opportunities as occur in the neighborhood of American and Canadian towns and villages. Physiognomically they present the large and prominent mouth, high cheek-bones, and broad face, so universally characteristic of the American Indian; but they by no means possess in a remarkable degree the wide and massive lower jaw, which has been noted as of universal occurrence among the Red Indians; and the aquiline nose is also absent in most of them.

The crania found in ancient cemeteries and ossuaries around Lakes Ontario, Erie, and Huron, chiefly belong to the two families referred to; and of the nation whose name is perpetuated in that of the last-named lake, the region occupied by it when first brought under the notice of the French Jesuit fathers is well defined; so that there is little risk of error in the determination of the race to which the remains found in its ancient graves belong. A partial difference in their relative proportions appears also to aid in the classification of the two ethnic divisions. The Algonquin cranium, though less markedly dolichocephalic than the Huron or Iroquois skulls, belongs to the same class; and to one or other of those nearly all the Canadian crania may with little hesitation be assigned.

Of Indian skulls chiefly dug up within the district once pertaining to the

Huron or Wyandot branch of the Iroquois stock, I had observed and cursorily examined a considerable number, before my attention was especially drawn to the peculiar characteristics now under consideration, owing to repeated rejection of those which turned up, as failing to furnish specimens of the assigned typical American head. Since then I have carefully examined and measured seventy Indian skulls belonging, as I believe, to the Wyandot or the Algonquin stocks, with the following results:

1. Only five exhibit such an agreement with the assigned American type, as, judged by the eye, to justify their classification as true brachycephalic crania. One very remarkable and massive skull was turned up at Barrie, on Lake Simcoe, within the Huron region, with upwards of two hundred others. It differs from all the others in exhibiting the vertical occiput so very strikingly, that when resting on it, it stands more firmly than in any other position. This is, without doubt, the result of artificial compression; and in so far as fashion regulated the varying forms thus superinduced on the natural cranial conformation, it is suggestive of an intruder from the country lying towards the mouth of the Mississippi, where the ancient graves of the Natchez tribes disclose many skulls moulded into this singular form. In some respects, indeed, it presents features strongly suggestive of comparison with the Scioto Mound skull, while the smallness of the lower jaw increases its divergence from the Huron or other northern Indian type. No note has been preserved of the general character of the crania discovered at the same time; but this one no doubt owed its selection to its peculiar form. The whole subject of occipital and varied cranial compression is deserving of minuter consideration than is admissible in reference to the Huron crania, which exhibit in general no traces of an abnormal formation. Nor is Dr. Morton's assignment of the vertical occiput as one of the most characteristic features of the true American cranium borne out by an examination of those found in Canadian cemeteries. On the contrary, I have been struck with the evidence afforded by the majority of skulls examined by me, that such was certainly no prevailing characteristic of the Huron or other tribes, by whom Upper Canada was occupied prior to its European settlement. Many of them, indeed, exhibit a total absence of any approximation to the flattened occiput. Twenty of the crania referred to show a more or less decided posterior projection of the occiput: eighteen of these being markedly so; and ten of them present such a prolongation of it, as constituted one of the most striking features in one class of ancient Scottish crania, which chiefly led to the suggestion of the term kumbecephalic, as a distinctive term for them. But since my observations on this subject were first published,* the special question of the prevailing form of the occiput has been taken up in a valuable monograph contributed by Dr. J. Aitken Meigs to the Transactions of the Academy of Natural Sciences of Philadelphia.† The conclusions he arrives at are: that the form of the human occiput is not constant, but varies even among individuals of the same race or tribe. He divides the different forms into three primary classes: 1st. The protuberant occiput, which is exhibited among the nations of the New World by the Esquimaux, Chippewas, Hurons, and more or less among thirty-six different American tribes or nations. 2d. The vertically flattened occiput he assigns as more or less prevalent among sixteen tribes, and characteristic of the majority of the Mound-builders. 3d. The full and rounded or globular occiput characterizes nine American nations or tribes, and occurs occasionally in a greater number. But the final summary of Dr. Meigs goes even further than this; and, treating as it does, not solely of the American, but of human

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* "Supposed prevalence of one Cranial Type throughout the American Aborigines."—*Canadian Journal*, November, 1857; *Edinburgh New Philosophical Journal*, January, 1858.

† *Observations upon the Form of the Occiput in the Various Races of Men*, by J. Aitken Meigs, M. D. Philadelphia, 1860.

occipital formation generally, it very effectually deals with all theories of radical diversities of human varieties or distinct species, in so far as this important subdivision of osteological evidence is concerned, by affirming, as the result of observations made on eleven hundred and twenty-five human crania, "that there is a marked tendency of these forms to graduate into each other, more or less insensibly. None of these forms can be said to belong exclusively to any race or tribe. None of them, therefore, can be regarded as strictly typical: for a character or form to be typical should be exclusive and constant." In his elaborate observations, Dr. Meigs has still left untouched the peculiarities which distinguish the female occiput. One elongated protuberant form appears to me to be found only in the female head; but a comparative estimate of the occipital variations in the two sexes, as exhibited in the different races, is necessary to complete this interesting inquiry.

2. The tendency to the pyramidal form, occasioned by the angular junction of the parietal bones, is apparent in the majority of the skulls examined. I have noted its occurrence as a prominent characteristic in twenty-three Canadian crania, of which ten exhibit a strongly marked pyramidal form, extending to the frontal bone. Nevertheless, it is by no means constant. Both in the Morton collection, and in the examples specially noted here, it is only slightly indicated in some, while in others it is entirely wanting.

3. I am further struck with the very partial projection, and in some male skulls with the total absence of the superciliary ridge: a characteristic which, so far as I am aware, has not been noted by other observers. In some the prominent ridge stretches entirely across the brow, forming a deep hollow at the junction of the os frontis and the bones of the nose; and this appears to be the case in the best authenticated Mound skulls. In the Scioto mound cranium it is markedly so, and it is little less apparent in the Grave creek mound, Tennessee, and Mississippi skulls. In this respect they differ from the majority of the Peruvian crania, with which in other respects they have been supposed so nearly to agree, that, overlooking this prominent physiognomical feature, the lost Mound-builders have been thought to reappear as the ancient architects of Peru. In the great majority of the crania figured by Morton, the very slight development, and in some, the total absence of a projecting superciliary ridge, is very noticeable. In thirteen of the Canadian skulls the same feature is particularly manifest. In the majority of these the os frontis slopes without any indentation to the edges of the orbits; and when taken into consideration along with the pyramidal vertex and predominant longitudinal diameter, suggests affinities, hitherto overlooked, with the Esquimaux form of skull.

4. It is also worthy of note that, whereas Dr. Morton states, as the result of his experience, that the most distant points of the parietal bones are for the most part the parietal protuberances: out of fifty-one Canadian skulls, I have only found such to be the case in three, all of which were female. The widest parietal measurement is generally a little above the squamous suture, and in some examples a still wider diameter is given between the temporal bones. Somewhat minute observations, accompanied in part with measurements, of numerous examples in the unrivalled collection of the Academy of Sciences of Philadelphia, and elsewhere, incline me to believe that this is a common characteristic of American crania.

The following tables (Tables VIII, IX) exhibit the relative proportions of the crania found in Upper Canada, in so far as they can be shown by such a series of measurements. Embracing, as they do, the comparative length, breadth, height, and circumference of sixty-nine skulls, procured without any special selection from Indian cemeteries, lying, with only four exceptions, to the north of Lakes Erie and Ontario, they supply a series derived from a sufficient number to indicate some constant proportions, and to mark certain elements of contrast instead of comparison, when placed alongside of the corresponding relative proportions in the tables of brachycephalic crania.

The measurements in Table VIII are derived from thirty-seven crania obtained from Indian graves in the region around Lake Simcoe, and on the Georgian bay, the ancient country of the Hurons.

TABLE VIII.—WESTERN CANADA: HURONS.

	Locality.	L. D.	P. D.	F. D.	V. D.	I. A.	I. L.	O. F. A.	H. C.
1	Orillia	7.5	5.7	4.5	5.6	15.6	4.2	15.0	21.1
2	do	7.4	5.5	4.4	5.4	14.7	4.5	....	20.6
3	do	7.3	5.7	4.2	5.7	15.3	4.3	14.1	20.5
4	do	7.5	5.6	4.2	5.4	14.7	4.3	14.6	21.1
5	do	7.2	5.3	4.3	5.3	14.5	4.3	14.3	20.3
6	do F	7.3	5.5	4.3	5.1	13.7	4.2	14.3	20.5
7	Owen Sound	7.0	5.5	4.2	5.0	13.8	4.0	14.0	19.8
8	do	7.3	5.3	4.3	5.3	14.4	4.2	14.2	20.4
9	do	7.2	5.4	3.8	5.2	14.5	3.9	14.2	19.9
10	do	7.7	5.4	4.7	5.6	14.6	4.2	15.0	21.4
11	do	7.5	5.9	5.1	5.5	15.0	4.3	15.6	21.8
12	do	7.6	5.5	4.5	5.4	14.6	4.5	14.9	21.3
13	Georgian Bay	7.6	5.6	4.2	5.4	14.6	4.7	15.0	21.1
14	do F	6.8	5.2	4.0	5.2	13.3	3.8	13.7	19.0
15	do F	7.4	4.9	4.2	5.3	13.3	......	14.1	20.0
16	Oro	7.5	5.6	4.4	5.5	15.6	4.3	15.2	21.4
17	do	7.4	5.4	......	4.3	15.2	4.0	14.9	20.4
18	Medonte	7.6	5.2	3.9	5.6	14.8	4.5	15.2	20.5
19	do	7.2	5.5	4.4	5.8	15.2	4.5	14.5	20.2
20	do	7.6	5.6	4.5	5.6	15.4	4.2	15.0	21.4
21	do	7.3	5.3	4.2	5.4	14.2	4.1	14.4	20.4
22	Penetanguishene	7.8	5.6	4.6	5.9	15.5	4.5	15.6	21.3
23	do	6.9	5.5	4.1	5.1	14.0	4.1	....	19.7
24	do	7.4	5.4	4.2	5.2	14.5	4.4	....	20.7
25	do	7.3	5.3	4.2	5.4	14.6	4.1	14.4	20.5
26	Tecumseth	7.3	5.6	4.4	5.5	14.5	4.9	14.4	20.2
27	do F	7.2	5.2	3.9	5.0	14.1	3.6	14.2	19.7
28	do	7.9	6.0	4.6	5.7	16.0	3.4	16.1	20.0
29	do F	7.6	5.3	4.3	5.6	14.0	4.1	14.3	20.2
30	do F	7.5	5.2	4.1	5.1	13.4	4.2	14.8	20.5
31	do	7.4	5.6	4.6	5.5	15.0	4.4	15.0	20.9
32	do	7.6	5.4	4.2	5.7	15.1	4.4	15.3	20.9
33	Whitchurch	7.5	5.3	4.2	5.7	15.1	4.2	14.6	20.4
34	Newmarket	7.2	5.6	4.6	6.7	15.7	4.2	15.0	20.3
35	do F	7.6	5.2	4.1	5.3	14.7	4.0	14.1	19.5
36	Oakridges	7.6	5.5	4.7	6.0	15.7	4.6	15.0	21.2
37	do F	6.8	4.8	4.2	5.0	13.6	4.0	13.2	18.9
	Mean	7.40	5.43	4.35	5.43	14.66	4.23	14.85	20.48

The localities specified in the following table show the wider region from whence the skulls have been procured which are assumed to illustrate the cranial characteristics of the Algonquin stock. The table includes the measurements of thirty-two Canadian skulls, the whole of which have been obtained from graves lying to the south and east of the true Huron country, towards the shores of Lakes Erie and Ontario, or on the north bank of the St. Lawrence. Some portions of Western Canada, including localities referred to, were occupied in the early part of the seventeenth century by tribes allied to the Hurons; but on their deserted areas the Algonquins from the north and west have everywhere preceded the English settlers, and the greater number of the crania introduced in this table may be assigned without hesitation to Algonquin tribes. No. 23

is designated by Dr Morton a Mississaga skull, and probably most, if not all, of those numbered consecutively from 16 to 28 belong to the same tribe. Nos. 28 to 32 are from Abenakis graves on the St. Maurice. As a whole, the examples thus grouped together present a sufficient number to furnish some adequate approximation to the prevailing typical specialties of the Algonquin head.

Table IX.—CANADA: ALGONQUINS.

	Locality.	L. D.	P. D.	F. D.	V. D.	I. A.	I. L.	O. F. A.	H. C,
1	Windsor	7.0	5.7	4.7	5.7	15.2	4.3	14.5	20.1
2	....do	7.0	5.7	4.5	5.7	16.1	4.0	14.4	20.1
3	....do	7.4	6.1	4.9	5.7	....	4.5	15.5	21.4
4	....do	6.6	5.3	4.2	5.5	14.5	4.2	13.5	19.0
5	Burford	6.5	5.2	4.1	5.0	13.4	4.0	13.0	18.4
6	Grand River	6.7	5.4	4.2	5.2	14.3	4.0	13.5	19.3
7	....do	7.5	5.6	4.4	5.4	15.0	4.1	15.2	21.0
8	Burlington Bay	7.0	5.3	4.4	5.3	14.0	4.0	13.6	19.5
9	....do	7.6	5.6	4.4	5.4	15.2	4.2	14.9	20.9
10	Nelson, F	7.5	5.2	4.2	5.5	14.0	4.6	15.0	20.4
11	....do	8.2	5.5	4.3	5.5	14.9	4.3	15.5	21.0
12	....do	7.7	5.9	5.3	5.4	15.0	4.7	15.3	21.5
13	....do.. F	7.3	5.5	4.1	5.1	14.0	4.3	14.7	20.5
14	....do.. F	7.3	5.4	4.0	5.2	14.4	4.3	14.4	20.5
15	....do.. F	7.2	5.4	3.7	5.3	14.3	4.0	14.3	19.8
16	River Humber	7.6	5.9	5.7	5.5	15.4	4.7	14.2	21.1
17	....do	6.8	5.6	4.5	5.1	14.1	4.5	13.9	19.9
18	....do	7.5	5.5	4.2	5.3	14.5	4.2	14.3	20.3
19	Burwick	7.5	5.7	4.2	5.6	15.3	4.5	14.9	21.0
20	....do	7.2	5.1	4.4	5.6	14.3	4.3	14.7	21.0
21	Peterboro'	7.7	5.5	4.9	5.3	15.4	4.6	15.0	21.1
22	....do	7.4	5.3	4.2	5.3	13.8	4.2	14.1	20.6
23	....do	6.5	5.2	3.9	4.9	13.3	3.8	13.7	19.2
24	....do	7.0	5.2	4.3	5.2	13.8	4.1	14.2	19.3
25	Rice Lake	7.1	6.5	3.9	6.3	14.5	4.3	14.2	20.0
26	Bay of Quinte	7.9	5.8	4.5	5.3	14.3	4.9	14.8	21.7
27	....do	7.0	5.5	4.2	5.0	14.0	4.6	13.9	20.5
28	....do	7.4	6.0	4.8	5.3	14.6	4.7	14.5	20.9
29	St. Maurice	7.0	5.3	4.1	5.3	13.0	4.4	14.0	20.5
30	....do	7.5	5.7	5.0	5.5	14.2	5.0	14.4	21.0
31	....do	7.0	5.5	4.7	5.5	14.0	4.2	14.5	20.7
32	Three Rivers	7.4	6.5	5.0	5.1	14.2	4.6	15.0	21.9
	Mean	7.25	5.58	4.43	5.37	14.42	4.35	14.42	20.44

But the term Algonquin, though apparently specially employed originally in reference to Canadian tribes, is now used as a generic appellation of a very comprehensive kind, and embraces ancient and modern tribes extending from the Labrador and New England coasts to far beyond the head of Lake Superior. In this comprehensive use of the term, its application is chiefly based on philological evidence; and it points thereby to affinities of language connecting numerous and widely-severed nations throughout the whole area lying between the Rocky Mountains and the Atlantic.

The New England tribes are described as having all presented a very uniform correspondence in their predominant characteristics. Dwight, in his *Travels in New England*, says of them: "They were tall, straight, of a red complexion, with black eyes, and of a vacant look when unimpassioned;" but he ascribes to them a good natural understanding and considerable sagacity and wit. They are not, even now, entirely extinct; but, like others of the eastern tribes that have been long in contact with the whites, it is difficult to find a pure-breed Indian among the remnants that still linger on some of their ancient sites. Judging, however, from the examples I have seen, it is probable that the red complexion, which Dwight assigns to the New England tribes, may have much more accurately justified the application of the term Red Indian to the aborigines first seen by European voyagers along the northern shores of the American continent than is now apparent when observing the olive-complexioned Chippewas, Crees, and other tribes of the west. Gallatin has grouped the New England Indians along with the Delawares, the Powhattans, the Pamlicoes, and other tribes of the Atlantic sea-board, extending as far south as North Carolina, under the comprehensive title of Algonquin-Lenapé. There is no doubt that important philological relations serve to indicate affinities running through the whole, and to connect them with the great Algonquin stock; while the essentially diverse Iroquois and Huron nations were interposed between them. The result of a careful examination and comparison of measurements of thirty-two New England crania, chiefly in the Boston and Philadelphia collections, has been to determine their classification as decidedly dolichocephalic, and is shown in the mean measurements as given in Table X.

Under the double title of Algonquin-Lenapé have been included all the Indian nations originally occupying the vast tract of the North American continent, extending from beyond the Gulf of the St. Lawrence to the area of the Florida tribes, and claiming the whole territory between the Mississippi and the sea, excepting where the Hurons and the aggressive Iroquois held the country around the lower lakes, and the Five Nations were already extending their hunting-grounds at the cost of Algonquin and Lenapé tribes. The mean of the latter, as given in Table X, is derived from twenty-three crania, chiefly in the Mortonian collection; and the mean of the Iroquois crania is based on measurements of forty-eight skulls from Canadian and other collections.

Thus far the various ethnical groups referred to are all embraced within the true American stock, of which Dr. Morton and others affirm a nearly absolute uniformity of cranial type, or such an approximation to it as serves, in their estimation, to indicate no less clearly the unity of the American race, and its specific separation, by radical diversity of ethnical characteristics, from all the races of the Old World. "Identical characters," says Dr. Nott, "pervade all the American race, ancient and modern, over the whole continent."* Again he says, "American crania, antique as well as modern, are unlike those of any other race of ancient or recent times;" and, "at the time of its discovery, this continent was populated by millions of people resembling each other, possessing peculiar moral and physical characteristics, and in utter contrast with any people of the Old World."† These may suffice to illustrate the opinions on this subject reiterated in a variety of forms by various writers, including men of high authority in questions of science. All, however, concur in excepting from this otherwise universal uniformity of ethnical characteristics those which pertain to the Esquimaux. They are referred to by Dr. Morton as "the only people possessing Asiatic characteristics on the American continent;" and the very contrast thus exhibited between them and all the other races of the western hemisphere has been assumed as a confirmation of the indigenous unity of the others. But, while this abrupt contrast in physical form is insisted on, it is acknowledged that no such philological line of demarkation can be traced; but, on the contrary, in language the Esquimaux are thoroughly American.

My opportunities for examining Esquimaux crania have been sufficient to furnish me with very satisfactory data for forming an opinion on the true Arctic skull form. In addition to the measurements of thirty-eight skulls, from which

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* *Types of Mankind*, p. 291.
† *Ibid*, p. 296.

the Esquimaux mean of Table X is derived, I have recently compared and carefully measured six Tchuktchi skulls, in the collection of the Smithsonian Institution, exhumed from the burial-place of a village called Tergnyune, on the island of Arikamcheche, at Glassnappe harbor, west of Bhering's straits; and, during a recent visit to Philadelphia, I enjoyed the advantage of examining, in company with Dr. J. Aitken Meigs, a series of one hundred and twenty-five Esquimaux crania, obtained by Dr. Hayes during his Arctic journey of 1860. The comparison between the Tchuktchi and the true Esquimaux skull is interesting. Without being identical, the correspondence in form is such as their languages and other affinities would suggest. Of the former, moreover, the number is too few, and the derivation of all of them from one cemetery adds to the chances of exceptional family features; but, on carefully examining the Hayes collection with a view to this comparison, I found it was quite possible to select an equal number of Esquimaux crania closely corresponding to the Tchuktchi type: which indeed presents the most prominent characteristics of the former, only less strongly marked. In both the skull is long, high, and pyramidal, and, in the Esquimaux especially, the junction of the parietals is frequently in a keel-like ridge, which extends into the depressed and narrow frontal bone.

But the same mode of comparison which confirms the ethnical affinities between the Esquimaux and their insular or Asiatic congeners, reveals, in some respects, analogies rather than contrast between the dolichocephalic Indian crania and those of the hyperborean race. The most characteristic features of the latter, as established by such a comparison, belong to the face, including the small nasal bones and the prognathous jaw, neither of which pertain to the true American Indian. The desired comparison may easily be made between the Iroquois or Huron cranium and that of the Esquimaux, from the vertical and occipital diagrams furnished in the *Crania Americana*, (pp. 192, 194, 248.) Both are elongated, pyramidal, and with a tendency towards a conoid, rather than a flattened or vertical occipital form; and when placed alongside of the most markedly typical Mexican or Peruvian heads, the one differs little less widely from these than the other. The contrast between the Huron and Esquimaux, obvious as it is, may be defined as physiognomical rather than cerebral; while some of the elements of calvarial correspondence are striking. The characteristics of the Esquimaux skull are defined by Dr. Meigs as "large, long, narrow, pyramidal; greatest breadth near the base; sagittal suture prominent and keel like, in consequence of the junction of the parietal and two halves of the frontal bones; proportion between length of head and height of face as seven to five; .... forehead flat and receding; occiput full and salient; face broad and lozenge shaped, the greatest breadth being just below the orbits; malar bones broad, high, and prominent, zygomatic arches massive and widely separated; nasal bones flat, narrow, and united at an obtuse angle, sometimes lying in the same plane as the naso-maxillary processes"[*] But, in reference to the nasal development, wherein it differs so decidedly from the true Indian physiognomy, the remarks of Dr J. Barnard Davis are worthy of note. In the Esquimaux of the eastern shores of Baffin's bay, he observes, the nasal bones are scarcely broader, though frequently longer than in some Chinese skulls, where they are so narrow as to be reduced to two short linear bones. "In those of the opposite, or American shores of Baffin's bay, they are very different, presenting a length, breadth, and angle of position almost equal to those of European races having aquiline noses."[†] This slight yet striking anatomical difference seems to supply a link of considerable value as indicative of a trait of physiognomical character in the more southern Esquimaux, tending, if confirmed by further observation, like other physical characteristics already noticed, to modify the abrupt transi-

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[*] *Catalogue of Human Crania*, A.N.S., 1857, p. 50.

[†] *Crania Britannica*, p. 30.

tion assumed heretofore as clearly defining the line of separation between the contrasting Arctic and Red Indian races. In all the arguments based on the assumed predominance of one uniform cranial type throughout the whole western hemisphere, the Arctic American, or Esquimaux, has invariably been excluded; and he has been regarded either as the exceptional example of an Asiatic intruder on the American continent, or as the hyperborean autochthones of the Arctic realm, as essentially indigenous there as the reindeer or the polar bear. An examination of Arctic crania, and a comparison of them with those of some of the most characteristic among the true Indian tribes, seems rather to suggest affinities and intermixture; while the same test applied to determine the amount of diversity among Indian races shows that they also intermingle very clearly defined elements of ethnical diversity. Dr. Latham, after commenting on the differences recognizable between the Esquimaux of the Atlantic and the Indians, adds: "It is not so with the Eskimos of Russian America and the parts that look upon the Pacific. These are so far from being separated by any broad and trenchant line of demarkation from the proper Indian, or the so-called Red race, that they pass gradually into it; and that in respect to their habits, manner, and appearance, equally. So far is this the case that he would be a bold man who should venture, in speaking of the southern tribes of Russian America, to say, Here the Eskimo area ends, and here a different area begins."* The diverse geographical conformation of the continent, which admits, on its western side, of frequent intercourse and consequent opportunities for intermixture of races, while, on its eastern side, the Esquimaux is entirely isolated, may account, in part, for the difference; but, in doing so, it also accounts for the amount of uniformity in the physical characteristics of tribes and nations separated by few geographical barriers, or well-defined limits, throughout the whole continent; but among whom, nevertheless, such marked cranial differences are found as the following table indicates. The mean of only four Mound crania is given, as they constitute in reality all of the authenticity of which I feel well assured; and, as their proportions are affected by artificial changes of form, the true characteristics of the ancient Mound-builders must be held as still depending on further evidence. The Cave crania, derived from an ancient cave at Steubenville, Ohio, and from the more celebrated Mammoth cave, Kentucky, are included in Table I.

TABLE X.—COMPARATIVE MEAN CRANIAL MEASUREMENTS.

		L. D.	P. D.	F. D.	V. D.	I. A.	I. L.	O. F. A.	H. C.
1	Mound Crania	6.57	5.90	4.20	5.55	15.60	4.40	14.00	19.83
2	Cave Crania	6.62	5.78	4.51	5.47	14.85	4.42	13.87	19.77
3	Peruvian B. C	6.32	5.62	4.06	5.18	14.96	4.12	13.27	19.10
4	Peruvian D. C	7.06	5.18	3.80	5.21	14.36	4.10	14.45	19.71
5	Mexican B. C	6.56	5.51	4.30	5.55	14.69	4.25	13.95	19.66
6	Mexican D. C	7.05	5.41	4.31	5.35	14.20	4.12	14.17	19.93
7	American B. C	6.62	5.45	4.24	5.30	14.63	4.25	13.85	19.44
8	American D. C	7.24	5.47	4.36	5.42	14.67	4.23	14.62	20.29
9	Iroquois	7.35	5.47	4.35	5.44	14.65	4.24	14.62	20.49
10	Algonquin	7.25	5.58	4.43	5.37	14.42	4.35	14.42	20.44
11	Algonquin-Lenapé	7.12	5.53	4.37	5.42	14.77	4.22	14.42	20.30
12	Esquimaux	7.28	5.22	4.31	5.46	14.48	4.18	14.82	20.42

The data from which the above results have been deduced are derived from the measurements of two hundred and eighty-nine skulls, along with the examination and comparison of a much larger number. A careful study of Peruvian

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* *Varieties of Man*, p. 291.

crania seems to prove that both classes are small, indicating a people of inferior size and stature, and presenting essential differences, even in the brachycephalic class, from those of the mounds. Their small vertical diameter is specially noticeable. In this, as well as in other respects, the greater correspondence between the Mexican brachycephali and the Mound crania is suggestive, and calculated to increase our desire for the acquisition of a sufficient number of examples of both, whereby to test the evidence of physical correspondence between the elder races of Anahuac and the people who have left such remarkable evidences of a partially developed civilization in the Mississippi valley. The two extremes are the Peruvian brachycephali and the Esquimaux—

	Length.	Breadth.	Height.	O. F. arch.
Peruvian	6.32	5.62	5.18	13.27
Esquimaux	7.28	5.22	5.46	14.82

But between these the range of variations sufficiently illustrates the fallacy of the supposed uniform cranial type affirmed to prevail throughout the whole western hemisphere from the Arctic circle to Cape Horn.

If the data thus selected as examples of the different groups furnish any approximation to their relative cranial measurements, it seems scarcely possible to evade the conclusion that the ideal American typical head has no existence in nature, and that, if a line of separation between the Peruvian, or so-called Toltecan head, and other American forms is to be drawn, it cannot be introduced as heretofore to cut off the Esquimaux, and rank the remainder under varieties of one type, but must rather group the hyperborean American cranium in the same class with others derived from widely separated regions, extending into the tropics and beyond the equator. In reality, however, the results of such attempts at a comparative analysis of the cranial characteristics of the American races go far beyond this, and prove that the form of the human skull is just as little constant among the different tribes or races of the New World as of the Old; and that, so far from any simple subdivision into two or three groups sufficing for American craniology, there are abundant traces of a tendency of development into the extremes of brachycephalic and dolichocephalic or kumbecephalic forms, and again of the intermediate gradations by which the one passes into the other. A much larger number of examples would be required to illustrate all the intermediate forms, but sufficient data are furnished here to point in no unmistakable manner to the conclusion indicated. If crania measuring upwards of two inches in excess in the longitudinal over the parietal and vertical diameters, or nearly approximating to such relative measurements—without further reference here to other variations of occipital conformation—may be affirmed, without challenge, to be of the same type as others where the longitudinal, parietal, and vertical diameters vary only by minute fractional differences: then the distinction between the brachycephalic and the dolichocephalic type of head is, for all purposes of science, at an end, and the labors of Blumenbach, Retzius, Nilsson, and all who have trod in their footsteps, have been wasted in pursuit of an idle fancy. If differences of cranial conformation of so strongly defined a character, as are thus shown to exist between various ancient and modern people of America, amount to no more than variations within the normal range of the common type, then all the important distinctions between the crania of ancient European barrows and those of living races amount to little, and the more delicate details, such as those, for example, which have been supposed to distinguish the Celtic from the Germanic cranium, the ancient Roman from the Etruscan or Greek, the Slave from the Magyar or Turk, or the Gothic Spaniard from the Basque or Morisco, must be utterly valueless. But the legitimate deduction from such a recognition, alike of extreme diversities of cranial form and of many intermediate gradations, characterizing the nations of the New World as well as of the Old, is not that cranial formation has no ethnic value, but that the truths embodied in such physiological data are as little to be eliminated by ignoring or slighting all diversities from the predominant form, and assigning it as the sole normal type, as by neglecting the many intermediate gradations, and dwelling exclusively on the examples of extreme divergence from any prevailing type.

PART II.

DESIGNED AND UNDESIGNED SOURCES OF CHANGE IN CRANIAL FORMS.

Among the characteristics of the American typical cranium, as defined by the author of the *Crania Americana*, and deduced by others from the evidence accumulated in that valuable contribution to physical ethnology, considerable importance is attached to the flattened occiput, which was assumed by him to be a purely natural feature of the American race. While he recognizes the elongated type of head pertaining to certain tribes, as the Osages, Missouris, Mandans, and Blackfeet, he adds: "Even in these instances the characteristic truncature of the occiput is more or less obvious;" and in his latest definition of the specialties of the American skull, he remarks: "In fact, the flatness of the occipital portion of the cranium will probably be found to characterize a greater or less number of individuals in every existing tribe from Terra del Fuego to the Canadas." The celebrated Scioto Mound skull has already been described, and the artificial origin of its greatly flattened occiput referred to, which even Dr. Morton appears to have recognized as surpassing the limits of his supposed typical conformation. "Similar forms," he remarks, "are common in the Peruvian tombs, and have the occiput, as in this instance, so flattened and vertical as to give the idea of artificial compression; yet this is only an exaggeration of the natural form, caused by the pressure of the cradle-board in common use among the American nations." My own observations on American crania led me, at an early period, to adopt the opinion not only that such extreme examples of the vertical occiput as are seen in the Scioto Mound and the Barrie skulls, are the results of artificial pressure, but, as I remarked in 1857, when submitting my views on Dr. Morton's supposed American cranial type, to the ethnological section of the American Association for the Advancement of Science, it is extremely probable that further investigation will tend to the conclusion that the vertical or flattened occiput instead of being a typical characteristic, pertains entirely to the class of artificial modifications of the natural cranium familiar to the American ethnologist, alike in the disclosures of ancient graves and in the customs of widely separated living tribes.* The idea thus expressed received further confirmation from noticing the almost invariable accompaniment of such traces of artificial modification, with more or less inequality in the two sides of the head. In the extremely transformed skulls of the Flathead Indians, and of the Natchez, Peruvians, and other ancient nations by whom the same barbarous practice was encouraged, the extent of this deformity is frequently such as to excite surprise that it could have proved compatible with the healthful exercise of any vital functions. But now that the general subject of artificial compression of the human cranium begins to receive some degree of minute attention from craniologists, it becomes obvious that such changes wrought on the natural form of the head are by no means peculiar to the American continent, either in ancient or modern times. The Macrocephali were known to Hippocrates in the fifth century before the Christian era, as a people who elongated the heads of their infants by artificial means.

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* Edin. Philosoph. Journal N. S., vol. vii, p. 24. Canadian Journal, vol. ii, p. 406.

Strabo, Pliny, and Pomponius Mela refer to various Asiatic localities where the same practice of moulding the head into favored abnormal forms was in use in their day; and repeated discoveries in modern times in the Crimea, in the Austrian valley of the Danube, and even in Switzerland, of similarly distorted crania, show how widely the practice had been followed in ancient times. The European examples have been generally referred to the Avarian Huns, but it affords a striking confirmation of the correspondence between the mode of practicing this barbarous process in the Old and the New World, that at the very time when the attention of Retzius and other European craniologists was specially directed to the subject, an American origin was assigned even to the European crania. Dr. Tschudi, guided by his extensive experience as a traveller, undertook to prove, in a memoir communicated to Müller's *Archiv für Anatomie*, that a skull found near Grafenegg, in Austria, and assigned by Professor Retzius to the Avars, was in reality an ancient Peruvian relic brought over in the sixteenth century, when the empire of Charles V. embraced both Austria and Peru in the same vast dominion. But repeated discoveries of similar artificially deformed crania, both on European and Asiatic sites, have placed beyond doubt that the very same processes of malformation practiced by the Peruvians, the Natchez, and by the barbarous tribes of Oregon, were in use among ancient European and Asiatic races. But the artificial changes of the human head are traceable to a variety of causes, all of which require to be maturely considered in order to rightly estimate the significance of national skull forms. These causes may be classified thus:

I. Undesigned changes of form superinduced in infancy by bandaging or other custom of head-dress; by the form of pillow or cradle-board; and by persistent adherence to any unvarying position in suckling and nursing.

II. Artificial deformation undesignedly resulting from the habitual carrying of burdens on the head, or by means of straps or bandages pressing on any part of the skull, when such is continued from very early youth.

III. Artificial configuration designedly resulting from the application of mechanical pressure in infancy.

IV. Deformation resulting from posthumous compression, or any mechanical force brought into operation after death.

To each of those causes I have directed some attention in different memoirs;* but I now propose to limit my remarks chiefly to one of the aspects of undesigned artificial compression in its relation to certain European skull forms. The influence of such causes in producing some peculiar features of the brachycephalic cranium found in ancient British barrows, was first suggested by me, in any accessible form, when pointing out the mistake into which Dr. Morton had fallen in supposing that the irregularity and unsymmetrical conformation observable in many skulls, but especially in those which have been subjected to any extreme amount of compression, is peculiar to American crania. The latter remark, I then observed, is too wide a generalization. I have repeatedly noted the like unsymmetrical characteristics in the brachycephalic crania of Scottish barrows; and it has occurred to my mind, on more than one occasion, whether such may not furnish an indication of some partial compression, dependent, it may be, on the mode of nurture in infancy, having tended, in their case also, if not to produce, to exaggerate the short longitudinal diameter, which constitutes one of their most remarkable characteristics.† The idea thus expressed was founded on observations carried out for some years on the crania of Scottish tumuli in relation to the general archæology of the country, preparatory to the embodying of the whole in the "*Prehistoric Annals of Scotland.*" Some of

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* Edin. Philosoph. Journ. N. S., vol. vii, 24; xiv, 269. Canadian Journal, vol. ii, 406; vi, 414; viii, 76, 127. Athenæum, Sep. 20th, 1862. Prehistoric Man, vol. ii, 294.

† Canadian Journal, Nov., 1857.

the special views derived from the study of ancient Scottish crania, were submitted to the ethnological section of the British Association in 1850;* and the general facts and deductions in reference to their ethnical significance are embraced in one of the sections of the above-named work. The subject continued to occupy my attention so long as I remained in Scotland, and I availed myself of every opportunity for adding to the rare materials for its illustration. While thus engaged my attention was repeatedly drawn to the unsymmetrical proportions of ancient brachycephalic skulls, and to their peculiar truncated form, accompanied, as in the Mound skull of the Scioto valley, by an abrupt flattening of the occiput, which I soon began to suspect was due to artificial causes. Since then the facilities derived from repeated examinations of American collections have familiarized me not only with the extreme varieties of form of which the human head is susceptible under the influence of artificial compression, but also with the less-marked changes undesignedly resulting from such seemingly slight causes as the constant pressure of the Indian cradle-board. The examination and measurement of several hundred specimens of American crania, as well as of the living head in representatives of various Indian tribes, have also satisfied me not only of the existence of dolichocephalic and brachycephalic heads as tribal or national characteristics, but of the common occurrence of the same exaggerated brachycephalic form, accompanied with the vertical or obliquely flattened occiput, which had seemed to be characteristic of the crania of the Scottish tumuli. There are indeed ethnical differences apparent, as in the frontal and malar bones, but so far as the posterior region of the head is concerned, both appear to exhibit the same undesigned deformation resulting from the process of nursing still practiced among many Indian tribes.

The light thus thrown on the habits of the British mother of prehistoric times, by skull-forms found in ancient barrows, is replete with interest, from the suggestions it furnishes of ancient customs hitherto undreamt of. But it has also another and higher value to the craniologist, from its thus showing that some, at least, of the peculiar forms hitherto accepted as ethnical distinctions, may be more correctly traced to causes operating after birth.

The first example of this peculiar cranial conformation which attracted my attention, as possibly traceable to other causes than inherited characteristics, or natural deviations from the typical skull form of an extinct race, occurred on the opening of a stone cist at Juniper Green, near Edinburgh, on the 17th of May, 1851. A slight elevation probably marked the nearly levelled barrow which had long covered the catacomb and its enclosed memorials of a remote past, within sight of the Scottish capital. A shallow grave, formed of unhewn slabs of sandstone, enclosed a space measuring three feet eleven inches in length, by two feet one inch in breadth at the head, and one foot eleven inches at foot. The joints fitted to each other with sufficient regularity to admit of their being closed by a few stone chips inserted at the junction, after which they appeared to have been carefully cemented with wet loam or clay. The slab which covered the whole projected over the sides, so as effectually to protect the sepulchral chamber from any infiltration of earth. It lay in a sandy soil, within little more than two feet of the surface; but it had probably been covered until a comparatively recent period by a greater depth of earth, as its site was higher than the surrounding surface, and possibly thus marked the traces of the nearly levelled tumulus. Slight as this elevation was, it had proved sufficient to prevent the lodgment of water, and hence the cist was found perfectly free from damp. Within this a male skeleton lay on its left side. The arms appeared to have been folded over the breast, and the knees drawn up so as to touch the elbows. The head had been supported by a flat water-worn stone for its pillow; but from this it had fallen to the bottom of the cist, on its being detached by

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* British Association Report, 1850, p. 142.

the decomposition of the fleshly ligatures; and, as is common in crania discovered under similar circumstances, it had completely decayed at the part in contact with the ground. A portion of the left side is thus wanting; but with this exception the skull was not only nearly perfect when found, but the bones are solid and heavy; and the whole skeleton appeared to me so well preserved as to have admitted of articulation Above the right shoulder, a neat earthen vase had been placed, probably with food or drink. It contained only a little sand and black dust when recovered, uninjured, from the spot where it had been deposited by affectionate hands many centuries before, and is now preserved along with the skull in the Scottish Museum of Antiquities.

As the peculiar forms of certain skulls, such as one described by Dr. Thurnam, from an Anglo-Saxon cemetery at Stone, in Buckinghamshire,* and another from an Indian cemetery at Montreal, in Lower Canada,† as well as those of numerous distorted crania, from the Roman site of Uriconium and other ancient cemeteries, have been ascribed to posthumous compression: the precise circumstances attendant on the discovery of the Juniper Green cist are important, from the proof they afford that the body originally deposited within it, had lain there undisturbed and entirely unaffected by any superincumbent pressure from the day of its interment. Two, if not three, classes of skulls have been recovered from early British graves. One with a predominant longitudinal diameter, in the most marked examples differs so essentially in its elongated and narrow forehead, and occiput from the modern dolichocephalic head, that I was early led to assign to it a separate class under the name of kumbecephalic or boat-shaped.‡ Another has the longitudinal diameter little in excess of the greatest parietal breadth. In its general proportions, its occipital formation, and even in some of its facial developments, it presents analogies to the American brachycephalic skull; though it lacks the characteristic flattened and receding forehead. This British brachycephalic skull occupies an intermediate place in its relative proportions among ancient British crania, and is no less strikingly distinguished from the prevailing modern head, whether of Celtic or Saxon areas, by its shortness, than the other is by its length, when viewed either in profile or vertically. The Anglo-Saxon type of skull appears to be intermediate between those two forms, with a more symmetrical oval, such as is of common occurrence in modern English heads.

The significance of the skull-forms of ancient British graves has been studied with intelligent zeal in recent years; and the discovery of essentially distinct types, suggests the inquiry as to traces of the existence of older races in Britain than the Celtæ found in occupation of the islands at the period of Roman invasion. The result of my own observations on such examples of ancient British crania as were accessible to me, before the interruption of my researches in this department of craniology, by my removal to Canada, was to impress me with the conviction that the evidence pointed to the existence of more than one early race; and that traces seemed to be recognizable, suggestive of one characterized by great length and narrowness of head, a remarkable prolongation of the occiput, and poor frontal development. To this another appeared to have succeeded with a short or brachycephalic head, prominent parietal development, and truncated occiput. Accordingly, when the questions involved in such researches and speculations were brought under the notice of ethnologists in a paper read by me before the British Association in 1850, I there remarked: "Not the least interesting of the indications which this course of investigation seems to establish in relation to the primitive races of Scotland, are the evidences of the existence of primitive British races prior to the Celtæ; and also the probability of these races having succeeded each other in a different order from the primitive

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*[Crania Britannica](#), Dec. I. p. 38.

†Edin. Philosoph. Journal. N. S. XVI. p. 269.

‡Prehistoric Annals of Scotland, pp. 169, 177.

colonists of the north of Europe. Meanwhile, however, these data, and the conclusions derived from them, are produced chiefly with a view to induce more extended research. A much greater accumulation of evidence is requisite to establish any absolute or certain conclusions; and this can only be obtained by a general interest in the inquiry leading to the observation of such, where the researches of the archæologist, or the chance operations of the agriculturist afford the desired means."* To suggest the possibilty of primitive races of men, not of Celtic origin, having been the earlier occupants of Scotland, appeared, in 1850, a sufficiently daring extravagance. But the *Antiquités Celtiques et Antédiluviennes* of M. Boucher de Perthes, had just issued from the French press; and already, after so brief an interval, we read in familiar phraseology of the prehistoric man of the Pfahlbauten of Switzerland and France, or of the Crannoges of Ireland and Scotland, and the Kjokkenmöddings of Denmark; and are no longer startled even to hear of the Flint-Folk of the pre-glacial period, the contemporaries of the *Elephas primigenius* and the *Rhinoceros tichorinus*. In 1851, before this wonderful revolution in opinion had been brought about, my ideas on the prehistoric races of Scotland, and inferentially of Britain, were set forth in greater detail;† but still necessarily accompanied with expressions of regret at the inadequate data available for investigations on a subject then altogether novel. Since then, however, the labors of intelligent students of science have been rewarded by large and valuable additions to the materials required for determining the questions dependent on craniological research; and special gratitude is due to Dr. J. Barnard Davis and Dr. Thurnam, who have accomplished in their admirable *Crania Britannica* the same accumulation of the requisite data for Britain which Dr. Morton had previously done for America.

With the materials thus furnished for application to the purposes of the ethnologist, the question has naturally been revived as to the true typical form of the Celtic cranium, and the possibility of reconciling the existence of such diverse forms as have already been referred to with the assumed aboriginal character of the Celtæ, and the assignment of all crania of an older date than the Roman period to that race. Besides the Saxon skull, with its tribal variations, including, as Dr. J. B. Davis conceives, the peculiar low and broad form to which he has given the name of platycephalic, there are, as already stated, two forms, the one as much shorter as the other is longer and higher than the average Saxon skull; both of which, on the theory of a primary Celtic aborigines, must be included among varieties of the same ethnical group.

If cranial conformation has any significance, it appears to me inconceivable that two such extreme forms can pertain to the same race; and the circumstances under which the most characteristic examples of the opposite types have been found, confirm me in the belief which I advocated when the evidence was much less conclusive, that the older dolichocephalic or kumbecephalic skull illustrates the physical characteristics of a race which preceded the advent of the Celtæ in Britain, and gradually disappeared before their aggressions. As, however, the opposite opinion is maintained by so high an authority as Dr. J. Barnard Davis, the comparison of the following measurements, illustrative of the three types of head, will best exhibit the amount of deviation in opposite directions from the intermediate form. No. 1, like the majority of the same class, is derived from a megalithic chambered barrow, and has been selected by Dr. Davis as a characteristic example of the class to which it belongs;‡ though, according to him, that is one of aberrant deviation from the typical British form. No. 2, obtained from a barrow at Codford, in Wiltshire, has also been selected by Dr. Davis as

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* *Inquiry into the Evidence of Primitive Races in Scotland prior to the Celtæ.* Report of Brit. Assoc. 1850, p. 144.

† *Archæology and Prehistoric Annals of Scotland.*

‡ Proceedings of the *Acad. Nat. Sciences*, Philadelphia, 1857, p. 42.

one of three typical British crania. It is of the same type as the Juniper Green skull, and its strongly marked characteristics are thus defined by him: "Its most interesting peculiarities are its small size, and its decidedly brachycephalic conformation. This latter character, which commonly appertains to the ancient British cranium, and even to that form which we regard as typical, is seldom met with expressed in so marked a manner."[*] No. 3 is a skull from an Anglo-Saxon cemetery near Litlington, Sussex, one of two of which Dr. Davis remarks: "There is a general indication of good form in these fine capacious skulls, which is apparent in every aspect. . . On a review of the whole series of Anglo-Saxon crania which have come under our notice, we are led to conclude that this pleasing oval, rather dolichocephalic form, may best be deserving the epithet of typical among them."[†] All the three examples are male skulls. The measurements embrace the longitudinal frontal, parietal and occipital diameters, with the parietal height and the horizontal circumference:

	L. D.	F. D.	P. D.	O. D.	P. H.	H. C.
1. Uley chambered Barrow skull	8.1	4.7	5.7	5.	5.1	21.7
2. Codford skull	6.8	4.6	5.7	5.1	4.7	20.
3. Litlington skull	7.5	4.7	5.3	4.6	4.9	20.9

Each of the above examples presents the features of the type to which it belongs with more than usual prominence, so that if the mean of a large series were taken, the elements of difference between the three would be less strongly defined. The differences are, however, those on which their separate classification depends; and they thus illustrate the special points on which any craniological comparison for ethnological purposes must be based. Of the three skulls, the era and race of one of them (No. 3) are well determined. It is that of a Saxon, probably of the seventh or eighth century, of the race of the South Saxons, descended from Ælla and his followers, and recovered in a district where the permanency of the same ethnic type is illustrated by its predominance among the rural population at the present day. Another of the selected examples (No. 2) is assumed by Dr. Davis, perhaps on satisfactory grounds, to be an ancient British, *i. e.*, Celtic skull. It is, indeed, a difficulty, which has still to be satisfactorily explained, how it is that if this brachycephalic type be the true British head-form, no such prevalence of it on modern Celtic areas is to be found, as in the case of Saxon Sussex connects the race of its ancient Pagan and Christian cemeteries, by means of the characteristic ovoid skull, with the Anglo-Saxon population of the present day. The historical race and era with which Dr. Davis appears to connect the Barrow-builders of Wiltshire, is thus indicated in the *Crania Britannica:* "Region of the Belgæ, Temp. Ptolemæi, A.D. 120." The Belgæ of that era—then apparently comparatively recent intruders, and by some regarded as not Celtic but Germanic—were displaced, if not exterminated; but the modern Britons of Wales are undoubted descendants of British Celts of Ptolemy's age. Though doubtless mingling Saxon and Norman with pure British blood, they probably preserve the native British type as little modified by foreign admixture as is that of its supplanters in the most thoroughly Saxon or Anglish districts of England. It is, therefore, a question of some importance how far the extreme brachycephalic proportions of the so-called British type may be traceable to other than inherited ethnical characteristics; whether, in fact, it is not entirely due to the undesigned flattening of the

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[*] *Crania Britannica*, Dec. ii, pl. 14.

[†] *Crania Britannica*, Dec. iv, pls. 39, 40.

occiput, and lateral expansion of the brain and skull, consequent on the use of the cradle-board.

Meanwhile, turning from this supposed British skull of Roman times to the one derived from Uley chambered barrow, (No. 1,) the most ancient of the series, and assuming their chronological order to be undisputed, as it appears to be, we find no gradation from an abbreviated to an elongated form, but, on the contrary, an extreme brachycephalic type interposed between the ovoid dolichocephalic Anglo-Saxon or Christian era and the extreme dolichocephalic or kumbecephalic one belonging to a period seemingly so remote that Dr. Thurnam, when noting the recurrence of the same type in another chambered barrow at Littleton Drew, Wiltshire, remarked: "It is not necessary to admit the existence of any pre-Celtic race, as the skulls described may be those of Gaelic, as distinguished from Cymric, Celts; or the long headed builders of these long, chambered, stone barrows, may have been an intrusive people, who entered Britain from the southwest. Can they have been some ancient Iberian or Ibero Phœnician settlers?"*

By whatever theory the difference is ultimately accounted for, it is manifestly one of a nature well calculated to suggest Iberian, Phœnician, Finnic, or any other diverse origin for the older race, rather than to admit of the belief of Celtic affinities for it, if the brachycephalic be the true British form. The divergence from the intermediate form, it will be seen, exceeds that of the extreme varieties already referred to among American crania, even when the exceptional Esquimaux mean is included, as in the following comparative proportions:

	L. D.	F. D.	P. D.	V. D.	I. A.	I. L.	O. F. A.	H. C.
Scioto mound skull	6.50	4.50	60.0	6.20	16.00	4.50	13.80	19.80
Barrie skull	6.60	5.20	64.0	5.30	16.00	4.60	14.40	20.70
Huron mean..................	7.40	4.35	54.3	5.43	14.66	4.23	14.65	20.48
Esquimaux mean	7.28	4.31	52.2	5.46	14.48	4.18	14.82	20.42

If no artificial element were supposed to affect any of those forms, the Barrie skull would naturally be classed with the former in any such comparison; and even with a full recognition of the artificial influences to which, as has been shown, both appear to have been subjected, it is scarcely conceivable that any amount of artificial deformation could be employed to transform the long, narrow, and high Esquimaux cranium into either form. The markedly brachycephalic proportions of each are traceable in part to the parieto-occipital flattening; but the symmetrical uniformity which characterizes both proves that they are only modified examples of naturally short and broad crania. But the vertical or obliquely flattened occiput, which even Dr. Morton recognized as, in its extreme manifestations, traceable to artificial compression, is by no means peculiar to the New World; and the importance of determining whether it is to be regarded as an ethnical characteristic, or merely an artificial result of external influences applied designedly or in the practice of some common usage, will be apparent when its prevalence has been recognized. Meanwhile, the suggestion of Dr. Thurnam, that the long-headed race of the British isles may possibly be traceable to Iberian or Phœnician intruders, invites attention to whatever materials may be available for the determination of the skull-forms of those ancient races.

Among the rarer crania of the Morton collection is one to which a peculiar interest attaches, and which may possibly have some significance in reference to this inquiry. Its history is thus narrated in Dr. Henry S. Patterson's Memoir

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* *Crania Britannica*, Dec. iii, pl. 24, (4.)

of Dr. Morton: During a visit of Mr. Gliddon to Paris, in 1846, he presented a copy of the *Crania Ægyptiaca* to the celebrated oriental scholar, M. Fresnel, and excited his interest in the labors of its author. Upwards of a year after he received, at Philadelphia, a box containing a skull, forwarded from Naples, but without any information relative to it. "It was handed over to Morton," says Dr. Patterson, "who at once perceived its dissimilarity to any in his possession. It was evidently very old, the animal matter having almost entirely disappeared. Day after day would Morton be found absorbed in its contemplation. At last he announced his conclusion. He had never seen a Phœnician skull, and he had no idea where this one came from; but it was what he conceived a Phœnician skull should be, and it could be no other."* Six months afterwards Mr. Gliddon received, along with other letters and papers forwarded to him from Naples, a slip of paper, in the handwriting of M. Fresnel, containing the history of the skull, which had been discovered by him during his exploration of an ancient tomb at Malta. Dr. Meigs refers to this in his catalogue of the collection, (No. 1352,) as an illustration of the "wonderful power of discrimination, the *tactus visus*, acquired by Dr. Morton in his long and critical study of craniology." Such was my own impression on first reading it; but I confess the longer I reflect on it the more am I puzzled to guess by what classical or other data, or process short of absolute intuition, the ideal type of the Phœnician head could be determined. I suspect, therefore, if we had the statement of Dr. Morton's own words, it would fall short of such an absolute craniological induction. The following is the sole entry made by him in his catalogue: "Ancient Phœnician! I received this highly interesting relic from M. S. Fresnel, the distinguished French archæologist and traveller, with the following memorandum, A. D. 1847:—Crâne provenant des caves sépulchrales de Ben-Djemma, dans l'île de Malte. Ce crâne paraît avoir appartenu à un individu de la race qui, dans les temps les plus anciens, occupait la côté septentrionale de l'Afrique, et les îles adjacentes." The sepulchral caves of Ben-Djemma are a series of galleries with lateral chambers or catacombs hewn in the face of the cliffs on the southwest side of the island of Malta. Other traces besides the rock-hewn tombs indicate the existence of an ancient town there, although no record of its name or history survives. M. Frédérick Lacroix remarks, in his *Malte et le Goze*, "Whoever the inhabitants of this city may have been, it is manifest from what remains of their works that they were not strangers to the processes of art. The sepulchral caves, amounting to a hundred in number, receive light by means of little apertures, some of which are decorated like a finished doorway. In others, time and the action of the humid atmosphere have obliterated all traces of such ornaments, and left only the weathered rock. . . The chambers set apart for sepulture are excavated at a considerable distance from the entrance, in the inmost recesses of the subterranean galleries. The tombs are of admirable design and style of art, and the details of their execution exhibit remarkable ingenuity and purity of taste. The author of the *Voyage Pitoresque de Sicile* does not hesitate to declare that they surpass in elegance any that he has seen executed on the same scale. What hand has hewn out these gloomy recesses in the rock? To that we can give no reply. The chronicles of Malta are silent on this point. Time has defaced the vestiges which might otherwise have helped to the solution of the problem." †

Other and very remarkable remains of antiquity abound in Malta and the neighboring island of Goza, including the cyclopean ruins styled *La tour des Géants*, which have also been assigned by some writers to a Phœnician or Punic origin, as a temple dedicated to Astarte; and the *Tadarnadur Isrira*, a megalithic structure for which a Pelasgic origin is assumed. But in drawing any comparison between the chambered galleries of Ben-Djemma and the megalithic

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* Memoir of Samuel G. Morton, p. xl.

† *Malte et le Goze*, p. 21.

chambered barrows or cairns of the British Islands, we are at best reasoning from the little known to the less known indices of prehistoric races; between which the points in common may amount to no more than those which admit of a comparison being drawn between the Brachycephali of the British Stone-Period, and the corresponding physical form and rude arts of American grave-mounds.

Nevertheless the Ben-Djemma skull in the Mortonian collection is not improbably what it has been assumed it to be; and it is in many respects a remarkable one. A deep indentation at the nasal suture gives the idea of an overhanging forehead, but the superciliary ridges are not prominent, and the peculiar character of the frontal bone is most strikingly apparent in the vertical view, where it is seen to retreat on either side, almost in a straight line from the centre of the glabella to the external angular processes of the frontal bone. The contour of the coronal region is described by Dr. Meigs as "a long oval, which recalls to mind the kumbecephalic form of Wilson."* It is of more importance, perhaps, to note that the remarkable skull recovered by Dr. Schmerling, from the Engis Cavern, on the left bank of the Meuse, buried five feet in a breccia, along with the tooth of a rhinoceros and other fossil bones, appears to be of the same elongated dolichocephalic type. Its frontal development is long and narrow; and its greatest relative proportion, in length and breadth, are 7.7 by 5.25 inches, so that it closely corresponds in those respects to the most characteristic British kumbecephalic crania."†

Whatever be the final conclusion of ethnologists as to the evidence which led me to adopt that name to indicate the characteristics of a pre-Celtic British race, the necessity appears to be acknowledged for some term to distinguish this form from the ordinary dolichocephalic type. The Ben-Djemma skull is narrow throughout, with its greatest breadth a little behind the coronal suture, from whence it narrows gradually towards front and rear. The lower jaw is large and massive, but with less of the prognathous development than in the superior maxillary. The skull is apparently that of a woman. The nose has been prominent; but the zygomatic arches are delicate, and the whole face is long, narrow, and tapering towards the chin. The parietals meet at an angle, with a bulging of the sagittal suture, and a slight but distinctly defined pyramidal form, running into the frontal bone. The occiput is full, round, and projecting a little more on the left side than the right. The measurements are as follows:

Longitudinal diameter	7.4
Parietal diameter	5.1
Frontal diameter	4
Vertical diameter	5.3
Intermeatoid arch	12.3
Intermastoid arch	15 (?)
Intermastoid line	4.3(?)
Occipito-frontal arch	14.2
Horizontal circumference	20.2

I have been thus particular in describing this interesting skull, because it furnishes some points of comparison with British kumbecephalic crania, bearing on the inquiry whether we may not thus recover traces of the Phœnician explorers of the Cassiterides in the long-headed builders of the chambered barrows. When contrasting the wide and nearly virgin area with which Dr. Morton had to deal, with that embraced in the scheme of the *Crania Britannica*, I remarked in 1857:—Compared with such a wide field of investigation, the little island home of the Saxons may well seem narrow ground for exploration.

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* Catalogue of Human Crania in the Academy of Nat. Sciences of Philadelphia, p. 29.

† Lyell's Antiquity of Man, p. 81.

But to the ethnologist it is not so. There, amid the rudest traces of primeval arts, he seeks, and probably not in vain, for the remains of primitive European allophyliæ. There it is not improbable that both Phœnicians and early Greek navigators have left behind them evidences of their presence, such as he alone can discriminate.* The Phœnicians stand, for northern Europe, as the oldest of all the ancient civilized nations of the world, to whom its seas, ports, and mineral treasures were known. Not unnaturally, therefore, there is a disposition to turn to them as a means of explaining all mysteries. Professor Nilsson, in the new edition of his *Skandinaviska Nordens Urinvänare*, ascribes to a supposed Phœnician occupation of the North the whole of the characteristic works of art of its Bronze period; and the temptation is still stronger for the British archæologist and craniologist to resort to a similar theory. The intercourse between Phœnicia and the ancient Cassiterides, by indirect, if not by direct, traffic, is undisputed. But the evidence of any Phœnician settlements in Britain rests on inferences from very vague allusions; and Sir George Cornwall Lewis has done his best to invalidate them. Summing up the results of his inquiry as to the nature of the classical evidence in favor of the Phœnicians having directly traded with Britain for its mineral wealth, and especially its tin, he remarks: "On the whole, the accounts preserved by the Greek and Latin writers lead to the inference that the tin supplied in early times to the nations in the east of the Mediterranean came by the overland route across Gaul, and that the Phœnician ships brought it from the mouth of the Rhone, without sailing as far as Britain." † British antiquaries will not willingly adopt such an opinion; but it serves at any rate to indicate how slight is the evidence on which to base any theory of a Phœnician origin for the ancient long-headed kumbecephali of the British Isles. Moreover, such a theory, in so far as it has any craniological basis, rests only on the recognition of the general analogy of form between certain British crania and the supposed Punic one brought from Malta; while it derives no confirmation from the discovery of works of art in the chambered barrows, or other sepulchres of the long-headed British race, such as can be ascribed to a Phœnician origin, or indicate any trace of Punic influence.

But there is another and more important aspect of the question. Before we can abandon ourselves to the temptations which the Punic theory offers, it has to be borne in remembrance that it is still disputed with reference to this class of British dolichocephalic crania. Are they examples of an essentially distinct type, preserving evidence of the characteristics of a different race, or are they mere exceptional aberrant deviations from the supposed brachycephalic Celtic or British type? Much stress is laid on the fact that the two forms of skull have occasionally been recovered from the same barrow; from which it may be inferred that the two races to which I conceive them to have belonged were, for a more or less limited period, contemporaneous. More than this I cannot regard as a legitimate induction from such premises, in relation to crania of such extremely diverse types. But this amounts to little, for the same is undoubtedly true of the ancient British and the modern Anglo-Saxon race; and the discovery of Celtic and Saxon skulls in a common barrow or tumulus of the 6th century is no proof that the latter race was not preceded by many centuries in the occupation of the country by the Britons, among whom they then mingled as conquerors and supplanters.

But the elongated skulls of the Uley barrow type are no rare and exceptional forms. They have been most frequently found in tombs of a peculiar character, designated chambered barrows, from the galleries and catacombs of large unhewn stones which they contain. To these tombs archæologists are unani-

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* Canadian Journal, vol. ii, p. 445.

† Historical Survey of the Astronomy of the Ancients, p. 455.

mous in assigning great antiquity. The late Mr. Thomas Bateman, of Lomberdale House, Derbyshire, soon after the publication of my first views relative to the pre-Celtic era of the long-headed race, or kumbecephali of Scotland, stated that in the Derbyshire long barrows, explored by him, "the boat-shaped skull had uniformly been found, rarely accompanied by any instrument, but, in one or two cases, with arrow-points of flint."* To this opinion subsequent researches, extending through successive years to 1858, appeared to him to lend confirmation; and, in his "Ten Years' Diggings in Celtic and Saxon Grave Hills," published in 1861, much additional evidence is produced. In describing some remarkable disclosures in Longlow barrow, he remarks: "This is the first opportunity we have had of exploring an undisturbed cist in a chambered cairn of this peculiar structure. It is, on this account, a discovery of unusual interest, and, when compared with the results of previous or subsequent excavations in similar grave-hills, yields to none in importance. The mound, composed of stone, inclosing a chamber or cist formed of immense slabs of stone, occasionally double or galleried, indicates, in this part of the country at least, a period when the use of metal was unknown, the sole material for the spear and arrow being flint, which is often carefully chipped into leaf-shaped weapons of great beauty. The interments within these cists have in every case been numerous, and apparently long continued. They are marked by a strongly defined type of skull, styled by Dr. Wilson kumbe-kephalic, or boat-shaped, the more obvious features being excessive elongation, flattening of the parietal bones, and squareness of the base, producing, when viewed from behind, a laterally compressed appearance, which is enhanced by the sagittal suture being sometimes elevated into a ridge. The adult male skull found in the centre of the Longlow cist has been selected to appear in the *Crania Britannica* as a typical example of this form. The crania of a female and of a girl about seven years old, from the same cist, exhibit the same form in a remarkable degree, as do the others which are more imperfect."† In the majority of cases the like imperfection has prevented more than the deduction of such general correspondence. Nevertheless, the number already obtained in a sufficiently perfect state to admit of detailed measurement is remarkable, when their great age and the circumstances of their recovery are fully considered. Of this the following enumeration will afford satisfactory proof. Only two perfect crania from the chambered tumulus of Uley, in Gloucestershire, of which the proportions of one are cited above, have been preserved. But, in the later search of Mr. Freeman and Dr. Thurnam, in 1854, the fragments of eight or nine other skulls were recovered, and of these the latter remarks: "The fragments are interesting, as proving that the characters observed in the more perfect crania were common to the individuals interred in this tumulus. Three or four calvaria are sufficiently complete to show that in them likewise the length of the skulls had been great in proportion to the breadth."‡ Again, in the megalithic tumulus of Littleton Drew, North Wilts, at least twenty-six skeletons appear to have been found, from several of which imperfect crania were recovered, and of those Dr. Thurnam remarks: "Eight or nine crania were sufficiently perfect for comparison. With one exception, in which a lengthened oval form is not marked, they are of the dolichocephalic class."§ So also the four nearly perfect skulls from West Kennet are described as "more or less of the lengthened oval form, with the occiput expanded and projecting, and presenting a strong contrast to skulls from the circular barrows of Wilts and Dorset."‖ To these may be added those of Stoney Littleton, Somersetshire, first pointed out by Sir R. C. Hoare;¶ and examples from barrows in Derby, Stafford, and Yorkshire, de-

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* *Journal of Archæol. Association*, Vol. VII, p. 211.

† Ten Years' Diggings in Celtic and Saxon Grave Hills, p. 95.

‡ *Archæol. Journal*, vol. xi. p. 313. *Crania Britannica*, Dec. I, pl. 5, (5.)

§ *Crania Britannica*, Dec. III, pl. 24, (3.)

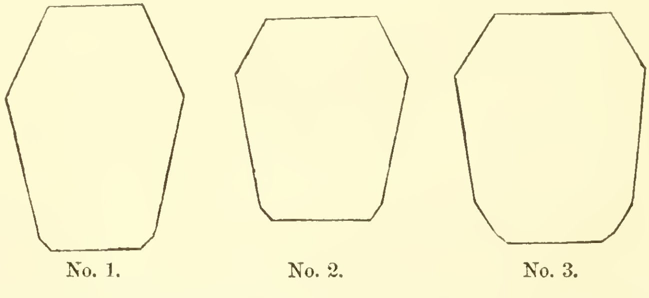
‖ *Ibid*, Dec. V, pl. 50, (4.)

¶ *Archæologia*, vol. xix, p. 47.

scribed by Mr. Thomas Bateman in his "Ten Years' Diggings in Celtic and Saxon Grave Hills;" including those from Bolehill, Longlow, and Ringham Low, Derbyshire; from the galleries of the tumulus on Five Wells Hill; and from the Yorkshire barrow, near Heslerton-on-the-Wolds. Several of the above contained a number of skulls, and, of the last, in which fifteen human skeletons lay heaped together, Mr. Bateman remarks: "The crania that have been preserved are all more or less mutilated, but about six remain sufficiently entire to indicate the prevailing conformation to be of the long or kumbecephalic type of Dr. Wilson."* The crania occurring in graves of this class, mentioned by Mr. Bateman alone, exceed fifty in number, of which the majority are either of the elongated type or too imperfect to be determined. The others include between thirty and forty well-determined examples, besides a greater number in too imperfect a state to supply more than indications of their correspondence to the same characteristic form. Alongside of some of these are also found brachycephalic crania; but, in the most ancient barrows, the elongated skull appears to be the predominant, and, in some cases, the sole type; and of the examples found in Scotland, several have been recovered from peat bogs, or others under circumstances more definitely marking their great antiquity.

The variations of cranial form are thus, it appears, no gradual transition, or partial modification, but an abrupt change from an extreme dolichocephalic to an extreme brachycephalic type; which, on the intrusion of the new and essentially distinct Anglo-Saxon race, gives place once more to a dolichocephalic form of medium proportions. The three forms may be represented, reduced to an abstract ideal of their essential diversities, by means of the following diagrams: No. 1, the kumbecephalic head of the chambered barrows; No. 2, the dolichocephalic, or supposed British type; and No. 3, the ovoid Anglo-Saxon head, still predominant.

Fig. 5.



Leaving, meanwhile, the consideration of the question of distinct races indicated by such evidence, it will be well to determine first if such variations of skull-form can be traced to other than a transmitted ethnical source. One of these, No. 2, presents many unmistakeable analogies to the most common American form; in so much so that, before I was familiar with the latter, otherwise than through the pages of the *Crania Americana*, I selected two of the most characteristic brachycephalic crania figured and described there, as the fittest for illustrating the typical characteristics of the Scottish skulls of short longitudinal diameter.† Of the same characteristic brachycephalic form the Barrie skull, (Fig. 6,) is a well defined example. Found in an Indian cemetery, on a continent where the craniologist is familiar with examples of the human head flattened and contorted into the extremest abnormal shapes, and where the influence of the Indian cradle-board in increasing the flattened occiput had long since been

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* *Ten Years' Diggings in Celtic and Saxon Grave Hills*, p. 230.
† Prehistoric Annals of Scotland, p. 167.

pointed out by Dr. Morton: the peculiar contour of the Barrie skull excited no more notice than the recognition of it as one well-known variety of American cranial forms. But, when almost precisely the same form is found in British graves, it is suggestive of ancient customs hitherto undreamt of, on which the familiar source of corresponding American examples is calculated to throw a novel light.

Fig. 6.

Fig. 7.

Of this form the Juniper Green skull, previously referred to as discovered in the immediate vicinity of Edinburg, is a striking example. It has been engraved the full size in the *Crania Britannica*, and, as will be seen, it presents in profile the square and compact proportions characteristic of British brachycephalic crania. It also exhibits, in the vertical outline, the truncated wedge form of the type indicated in Fig. 5, No. 2. In the most strongly marked examples of this form, the vertical or flattened occiput is a prominent feature, accompanied generally with great parietal breadth, from which it abruptly narrows at the occiput. The proportions of this class of crania were already familiar to me before the discovery of the Juniper Green example, but it had not before occurred to me to ascribe any of their features to other than natural causes. But the circumstances attending its exhumation gave peculiar interest to whatever was characteristic in the skull and its accompanying relics, handled for the first time as evidences of the race and age of the freshly opened cist, discovered almost within sight of the Scottish capital, and yet pertaining to prehistoric times. This interesting skull was deposited in the Museum of the Scottish Antiquaries, along with the urn which had lain beside it in the rude cist, and I accompanied its presentation with the first expression of my suspicion—for it scarcely then amounted to more—that the flattened occiput was due to some artificial compression, by means of which the abbreviated form so common in crania of the Scottish tumuli had been exaggerated if not entirely produced.

Another skull in the same collection, found under somewhat similar circumstances in a cist at Lesmurdie, Banffshire, has the vertical occiput accompanied by an unusual parietal expansion and want of height, suggestive of the idea of a combined coronal and occipital compression.* A third Scottish skull, procured from one of a group of cists near Kinaldie, Aberdeenshire, also exhibits the posterior vertical flattening. But a more striking example than any of those appears in the one from Codford, South Wiltshire, selected here to illustrate this type. Dr. Davis remarks, in his description of it: "The zygomatic arches are

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* Vide *Crania Britannica*, Dec. II plate 16.

short, a character which appertains to the entire calvarium, but is most concentrated in the parietals, to which the abruptly ascending portion of the occipital lends its influence. The widest part of the calvarium is about an inch behind, and as much above the auditory foramen, and, when we view it in front, we perceive it gradually to expand from the outer angular process of the frontal to the point now indicated." The entire parieto-occipital region presents in profile an abrupt vertical line; but, when viewed vertically, it tapers considerably more towards the occiput than is usual in crania of the same class.

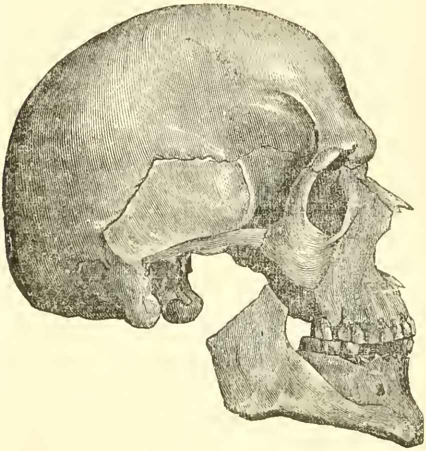

Fig. 8.

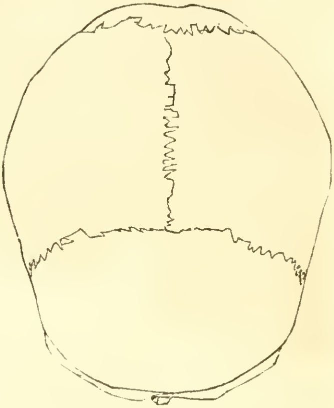

Fig. 9.

A comparison of this skull, recovered from an ancient British grave, with the one obtained in an Indian ossuary in Barrie, in Upper Canada, shows the squarer form of the British skull, when seen in profile, dependent in part on the more elevated and well arched frontal bone. But, in the vertical view, the Indian skull shows its extreme brachycephalic character; being at once shorter and broader than the British one, though the latter is one of the most strongly marked of its class. The vertical character of the occiput is also strikingly displayed. In other examples the flattening chiefly affects the parietal bones extending in an oblique line towards the coronal suture.

The origin of both, as artificial forms superinduced on a naturally short and broad type of skull, I feel no hesitation in believing to be traceable to the same kind of rigid cradle-board as is in constant use among many of the Indian tribes of America, and which produces precisely similar results. Its mode of operation, in effecting the various forms of oblique and vertical occiputs, will be best considered when describing some of the phenomena of compressed Indian crania; but another feature of the Juniper Green skull, which is even more obvious in that from Lesmurdie, in the same collection, is an irregularity amounting to a marked inequality in the development of the two sides. This occurs in skulls which have been altered by posthumous compression; but the recovery of both the examples referred to from stone cists precludes the idea of their having been affected by the latter cause; and since I was first led to suspect the modification of the occiput, and the exaggeration of the characteristic proportions of British brachycephalic crania by artificial means, familiarity with those of the Flathead Indians, as well as other ancient and modern artificially distorted American crania, has led me to recognize in them the constant occurrence of the same unsymmetrical inequality in opposite sides of the head.

The inequality in the development of the opposite sides of the above skulls belongs to the same class of deformations as the well-known distortions produced on many American crania, both by the undesigned action of the cradle-board, and by protracted compression purposely applied with a view to change the form, merits the careful attention of craniologists. The normal human head may be assumed to present a perfect correspondence in its two hemispheres; but very slight investigation will suffice to convince the observer that few living examples satisfy the requirements of such a theoretical standard. Not only is inequality in the two sides frequent, but a perfectly symmetrical head is the exception rather than the rule. The plastic condition of the cranial bones in infancy, which admits of all the strange malformations of ancient Macrocephali and modern Flatheads, also renders the infant head liable to many undesigned changes. From minute personal examination I have satisfied myself of the repeated occurrence of inequality in the two sides of the head, arising from the mother being able to suckle her child only at one breast, so that the head was subjected to a slight but constantly renewed pressure in the same direction. It is surprising, indeed, to how great an extent such unsymmetrical irregularity is found to prevail, when once the attention has been drawn to it. The only example of the Greek head possessed by Dr. Morton was a cast presented to him by Dr. Retzius, and which, from its selection by the distinguished Swedish craniologist for such a purpose, might reasonably be assumed to illustrate the Greek type. It is accordingly described by Dr. J. Aitken Meigs, in his "Cranial Characteristics of the Race of Man," as very much resembling that of Constantine Demetriades, a Greek native of Corfu, and long a teacher of the modern Greek language at Oxford, as engraved in Dr. Prichard's *Researches.* Its cranial characteristics are thus defined in the Catalogue of the Mortonian Collection, (No. 1354.) "The calvarial region is well developed, the frontal line expansive and prominent, the facial line departs but slightly from the perpendicular." On recently visiting Philadelphia for the purpose of renewed examination of its valuable collections, I was surprised to find this head—instead of being either oval, or, as Blumenback describes the example selected by him, sub-globular—presenting the truncated form, with extreme breadth at the parietal protuberances, and then abruptly passing to a flattened occiput. It measures 6.5 longitudinal diameter; 5.7 parietal diameter; and 19.2 horizontal circumference. But the most noticeable feature is the great inequality of the two sides. The right side is less tumid than the left, while it projects more to the rear, and the whole is fully as unsymmetrical as many American crania. Were it not that this feature appears to have wholly escaped Dr. Morton's attention, as he merely enters it in his catalogue as a "cast of the skull of a young Greek: Prof. Retzius," I should be tempted to suppose it had been purposely sent to him to illustrate the phenomena of unsymmetrical development, and of the influence of undesigned artificial causes on other crania besides those of the New World.

The strongly marked deformation of many flattened Indian skulls so clearly separates them as a class from all others, including those modified by partial or undesigned compression, as in the British examples referred to, that the very familiarity with the former is calculated to lead the American craniologist to overlook the artificial source of slighter changes. Nevertheless, Dr. Morton was not unobservant of such indications of the frequent dissimilarity between opposite sides of the skull, nor did he entertain any doubt as to its cause when occurring as the accompaniment of other artificial changes, though he entirely overlooked its more general prevalence. When first noticing the probable origin of the flattened occiput of certain British skulls, I drew attention to the fact that he had already recognized undesigned artificial compression as one source of abnormal cranial conformation, and that he accompanied its demonstration with a reference to the predominant unsymmetrical form in all such skulls. "This irregularity," he observes, "chiefly consists in the greater projection of the occiput to one side than the other," and "is not to be attributed to the intentional application of mechanical force." Such want of uniformity in the two sides of the head is much more strongly marked in the Flathead skulls, which have been subjected to great compression. It is clearly traceable to the difficulty of subjecting the living and growing head to a perfectly uniform and equable pressure, and to the cerebral mass forcing the skull to expand with it in the direction of least resistance. Hence the unsymmetrical form accompanying the vertical occiput in the Lesmurdie and Juniper Green skulls, and, as I conceive, also in the Greek skull of Retzius. The study of the latter skull-form has tended to confirm my belief that the extreme abbreviated proportions of many naturally brachycephalic crania are due to artificial causes. Wherever a very noticeable inequality exists between the two sides of a skull, it may be ascribed with much probability to the indirect results of designed or accidental compression in infancy; and by its frequent occurrence in any uniform aspect, may, quite as much as the flattened occiput, furnish a clue to customs or modes of nurture among the people to whom it pertains.

But besides the practices referred to, many minor causes tend to produce peculiar forms and irregularity of development in the human head. Among those, I have become familiar with a number of cases, where, owing to the inability of the mother to suckle her child at one breast, the constant pressure in one direction which this led to has produced a marked flattening on the corresponding side of the child's head, with tumid expansion on the other. The mere practice of the nurse constantly carrying the child on one arm, or systematically laying it to sleep on one side, must have a tendency to produce similar results; for the bones of the infant's head during the first year are exceedingly soft and pliable, and, as the processes pursued by the Flathead Indians show, may be moulded into almost any form by moderate pressure. The normal human head may be assumed to present a perfect correspondence in its two hemispheres; but very slight investigation will suffice to convince the observer that few living examples satisfy the requirements of such a theoretical standard. Not only is inequality in the two sides of frequent occurrence, but a perfectly symmetrical head is the exception rather than the rule. The plastic condition of the cranial bones in infancy also renders the infant head liable to many undesigned changes of form. The obstetric practitioner is also familiar with the extreme deviations from the normal or congenital form of head produced at birth, where instruments have to be used; but which, from the plastic condition of the bones, speedily disappear, or are greatly modified by the growth of the brain.

In connexion with this branch of the subject the following observations of Sir Robert H. Schomburgk on the Maopityans, or Frog Indians, of British Guiana, are well worthy of consideration. They are the remnant of a nearly extinct tribe. Of their cranial formation he remarks: "The flatness of the head, and consequently the long face and short circumference, is peculiar to the tribe. I have not been able to learn, upon the most minute inquiries, that the form is given to the head by artificial means. The occiput of the men is high, and almost perpendicular above the front; the frontal bone is small with regard to extent, and in no comparison to the face below the eyes; the cheek bones are harsh and prominent; but the most remarkable part of the head is the great extent between ear and ear, if measured from the upper part of that organ, and the line continued above the eyebrows to the commencement of the other ear. It surpasses the measurement of other Indians generally by an inch or two." Notwithstanding the inability of this intelligent and observant traveller to recover any traces of artificial causes influencing so remarkable a form of head, we might still be tempted to refer it to a source so familiar to the American craniologist. But three days after his arrival at the settlement, one of the women, a Maopityan, but the wife of a Taruma—a neighboring tribe characterized by an unusually small and differently formed head—was delivered of a male child. Sir Robert Schomburgk states: "The Indians invited me to see the infant, and accordingly, provided with some suitable presents, I went. The newborn child had all the characterictics of the mother's tribe. It was not quite an hour old when I saw it, and the flatness of its head as compared with the heads of other tribes, was very remarkable."* Such a narrative, resting as it does on unquestionable authority, shows the danger of error in referring all seemingly abnormal cranial forms to artificial causes, and might almost tempt the theorist to recur to the idea entertained by Hippocrates, relative to the Macrocephali of the Crimea, that long heads ultimately became so natural among them that the favorite form was perpetuated by ordinary generation. To have rendered the observations complete, however, it would have been desirable to have had a further report on the shape of the infant's head some time after birth, so as to determine if it were entirely due to the inherited typical head-form of the mother's tribe, and not to an unusual amount of compression incident to the circumstances of its birth.

When the pressure is not, as in the processes operating at birth, temporary, but continuous or repeatedly applied in the same direction, at brief intervals, as in nursing entirely at one breast, a want of uniformity is certain to result. The dissimilarity in the two sides of the head is strongly marked in Flathead skulls which have been subjected to great compression. This is clearly trreeable to the difficulty of subjecting the living and growing head to a perfectly uniform and equable pressure, and to the cerebral mass forcing the skull to expand with it in the direction of least resistance. Hence the unsymmetrical form accompanying the vertical occiput in the Lesmurdie and Juniper Green skulls. Wherever therefore a very noticeable inequality exists between the two sides of a skull, it may be traced with much probability to designed or accidental compression in infancy, and by its frequent occurrence in any uniform aspect, may, quite as much as the flattened occiput, furnish a clue to customs or modes of nurture among the people to whom it pertains.

Irregular head-forms are so much concealed by the hair and head-dress that it is only in very marked cases they attract the attention of ordinary observers. But, as I have shown in former publications on this subject,† they are familiar to hat-makers, and frequently include extremely unsymmetrical developments and great inequality in opposite sides of the head. A modern skull in the collection of Dr. Struthers, of Edinburg, exhibits an interesting combination of the proportions of the ancient brachycephalic type, with unsymmetrical conformation. It measures 7.5 longitudinal diameter, 6.5 parietal diameter, 21.4 horizontal circumference, and its greatest breadth is so near the occiput that the truncated form observable in the vertical view of many ancient British crania is produced in its most marked character by the abrupt flattening immediately behind the parietal protuberances, accompanied with inequality in the two sides of the head. It was obtained from a grave-digger in Dundee, who stated it to be that of a middle-aged female whom he had known during life. There was nothing particular about her mental development.

The novel forms thus occurring in modern heads, though chiefly traceable, as I believe, to artificial causes, are not the result of design. But the same is true of the prevalent vertical and obliquely flattened occiput of many ancient and modern American crania, as well as of the British brachycephalic class already described. Nor are such changes of the natural form necessarily limited to skulls of short longitudinal diameter, in which this typical characteristic is exaggerated by the pressure of the cradle-board in infancy. Now that this source of modification begins to receive general recognition among craniologists, its influence is assumed as a probable source of the most diverse aberrant forms. Dr. Thurnam, when referring to two skulls of different shapes, recovered from the same group of British barrows, of "a somewhat late though pre-Roman

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Journal of the Royal Geographical Society, vol. xvi, pp. 53, 57.

† Prehistoric Man, vol. ii, p. 312 ; Canadian Journal, vol. vii, 414.

period," on Roundway hill, North Wiltshire, thus indicates their contrasting characteristics, and suggests the probable source of such divergence from the supposed British type: "The general form of the cranium (pl. 43) differs greatly from that from the adjoining barrow, (pl. 42.) That approaches an acrocephalic, this a platycephalic form; that is eminently brachycephalic, this more nearly of a dolichocephalic character. As the eye at once detects, the difference is much greater than would be inferred from a mere comparison of the measurements. The respective peculiarities of form in the two skulls may possibly be explained by supposing that both have been subject to artificial deformation, though of a different kind—the one appearing to have been flattened on the occiput, the other showing a depression immediately behind the coronal suture, over the parietal bones, which seems to indicate that this part of the skull was subject to some habitual pressure and constriction, perhaps from the use of a bandage or ligature tightly bound across the head and tied under the chin, such as to this day is employed in certain parts of the west of France, producing that form of distortion named, by Dr. Gosse, the sincipital, or *tête bilobée*."*

The gradual recognition of this secondary source of undesigned artificial changes in the form of the skull may be traced through various works, from the vague perception of its occasional influence on the occipital form of American crania, indicated by Dr. Morton, to the full appreciation of its varied effects in the production of the most diverse exaggerations of normal or abnormal shapes, in the later decades of the *Crania Britannica*. Dr. J. B. Davis devotes a chapter in the first decade to the subject of "Distortions of the human skull," in which he minutely discusses the influence of artificial causes in modifying and transforming its natural shape in a wonderful and frequently very fantastic manner. But the only class of changes which attracts his attention, in addition to those expressly resulting from design, are the examples of the fourth class, where the deformation is clearly traceable to posthumous compression. But during the progress of the work the attention of various observers was directed to the secondary sources of change of form, and especially to such as may be ascribed to the use of the cradle-board, or some corresponding nursing usage. In the fifth decade of the *Crania Britannica* accordingly may be traced very clearly the influence of the full recognition of such causes in modifying the views of its joint authors as to the significance of certain peculiar skull-forms. An extremely brachycephalic skull of a youth, obtained from a barrow on Ballard Down, Isle of Purbeck, is described as unsymmetrical, and as affording "tolerably clear evidence that this form, if not always produced, was at least liable to be exaggerated by an artificial flattening of the occiput, such as is practiced by many American and Polynesian tribes."† In the same decade another skull of the type, most dissimilar to this, is described and illustrated. It was recovered in fragments from the remarkable chambered barrow at West Kennet, Wiltshire; and its most characteristic features are thus defined by Dr. Thurnam: "It is decidedly dolichocephalic, narrow, and very flat at the sides, and realizes more nearly than any we have yet had to figure the kumbecephalic or boat-shaped form described by Dr. D. Wilson. The frontal region is narrow, moderately arched, and elevated at the vertex, but slopes away on each side. The parietal region is long, and marked by a prominent ridge or *carina* in the line of the sagittal suture, which is far advanced towards obliteration, whilst the other sutures are quite as perfect as usual. The occiput is full and prominent; the supra-occipital ridges only moderately marked. There is a deep digastric groove, and a slight parocipital process on each side. The external auditory openings are somewhat behind the middle of the skull, and very much behind a vertical line drawn from the junction of the coronal and sagittal

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* Crania Britannica, Dec. v, pl. 43.

† Crania Britannica, Dec. v, pl. 45.

sutures." Its extreme length and breadth are 7.7 and 5.1, and an inequality in the development of the two sides is obvious in the vertical view. As the brachycephalic skull recalls certain American and Polynesian forms, so such examples of the opposite type suggest the narrow and elongated skulls of the Australians and Esquimaux; and he thus proceeds: "The Ballard Down skull bears marks of artificial flattening of the occiput; this calls to mind the artificial lateral flattening of the skull characteristic of the ancient people called Macrocephali, or long heads, of whom Hippocrates tells us that 'while the head of the child is still tender, they fashion it with their hands, and constrain it to assume a lengthened shape by applying bandages and other suitable contrivances, whereby the spherical form of the head is destroyed, and it is made to increase in length.' This mode of distortion is called by Dr. Gosse the *temporo-parietal* or '*tête aplatie sur les côtés.*' It appears to have been practiced by various people, both of the ancient and modern world, and in Europe as well as the east. The so-called Moors, or Arabs of North Africa, affected this form of skull; and even in modern times the women of Belgium and Hamburg are both described as compressing the heads of their infants into an elongate form. Our own observations lead at least to a presumption that this form of artificial distortion may have been practiced by certain primeval British tribes, particularly those who buried their distinguished dead in long chambered tumuli."

In connexion with this class of head-forms, as the result of compression, Dr. Thurnam draws attention to the obliteration of the sagittal suture in the elongated skull. I have noted this in many Flathead crania, and shall recur to the subject in referring to those in the Washington and Philadelphia collections. If the artificial forms result from compression, the flattened occiput and artificially abbreviated skull should show a tendency to ossification and obliteration of the coronal and the lambdoidal sutures; while in the elongated skull the sagittal suture will be the one affected, as is the case in one figured and described by Blumenbach, under the name of "Asiatic Macrocephali." But in all cases of an artificial change of form, the natural proportions necessarily exercise some influence on the result; and Dr. Thurnam, accordingly, when referring to the obliteration of the sagittal suture as a result of the artificial elongation of the West Kennet skull, expresses his belief that this "has been produced by pressure or manipulations of the sides of the head in infancy, by which it was sought to favor the development of a lengthened form of skull; to which, however, there was probably, in the present instance, at least, a natural and inherent tendency." It is perhaps worthy of note here, that a long narrow head has been observed as one of the characteristic features of Berber tribes of North Africa. Mr. J. Homer Dixon, who resided for some time at Algiers, and had repeated opportunities of visiting and closely observing the neighboring tribes, describes them to me as distinguished by their prominent, arched nose, with wide nostrils; large mouth but thin lips, and an unusual length of head. The constancy of the long head-form particularly struck him, but I could not learn from him of any nursing practice calculated to originate or increase such a development.

From the various authorities and illustrative examples referred to, it is obvious that a class of variations of the form of the human skull, which becomes more comprehensive as attention is directed to it, is wholly independent of congenital transmitted characteristics. Kumbecephalic, acrocephalic, and platycephalic, unsymmetrical, truncated, or elongated heads, may be so common as apparently to furnish distinctive ethnical forms, and yet, after all, each may be traceable to artificial causes, arising from an adherence to certain customs and usages in the nursery. It is in this direction, I conceive, that the importance of the truths resulting from the recognition of artificial causes affecting the forms of British brachycephalic or other crania chiefly lies. The contents of early British cists and barrows prove that the race with which they originated was a rude people, ignorant for the most part of the very knowledge of metals, or at best in the earliest rudimentary stage of metallurgic arts. They were, in fact, in as uncivilized a condition as the rudest forest Indians of America. To prove, therefore, that, like the Red Indian squaw, the British allophylian or Celtic mother formed the cradle for her babe of a flat board, to which she bound it, for safety and facility of nursing, in the vicissitudes of her nomade life—though interesting, like every other recovered glimpse of a long forgotten past—is not in itself a discovery of much significance. But it reminds us how essentially man, even in the most degraded state of wandering savage life, differs from all other animals. The germs of an artificial life are there. External appliances, and the conditions which we designate as domestication in the lower animals, appear to be inseparable from him. The most untutored nomades subject their offspring to many artificial influences, such as have no analogy among the marvellous instinctive operations of the lower animals. It is even not unworthy of notice that man is the only animal to whom a supine position is natural for repose; and with him, more than any other animal, the head, when recumbent, invariably assumes a position which throws the greatest pressure on the brain case, and not on the malar or maxillary bones.

It thus appears that the study of cranial forms for ethnological purposes is beset with many complex elements; and now that the operation of undesigned artificial influences begins to receive an adequate recognition, there is a danger that too much may be ascribed to them, and that the ethnical significance of congenital forms, and their traces even in the modified crania of different types, may be slighted or wholly ignored. Such was undoubtedly the effect on Dr. Morton's mind from his familiarity with the results of artificial deformation on American crania, coupled, perhaps, with the seductive influences of a favorite hypothesis. In his latest recorded opinions, when commenting on some of the abnormal forms of Peruvian crania, he remarks: "I at first found it difficult to conceive that the original rounded skull of the Indian could be changed into this fantastic form, and was led to suppose that the latter was an artificial elongation of a head remarkable for its length and narrowness. I even supposed that the long-headed Peruvians were a more ancient people than the Inca tribes, and distinguished from them by their cranial configuration. In this opinion I was mistaken. Abundant means of observation and comparison have since convinced me that all these variously-formed heads were originally of the same shape, which is characteristic of the aboriginal race from Cape Horn to Canada, and that art alone has caused the diversities among them."* It is obvious, however, that without running to the extreme of Dr. Morton, who denied, for the American continent, at least, the existence of any true dolichocephalic crania, or, indeed, any essential variation from one assumed typical form, it becomes an important point for the craniologist to determine, if possible, to what extent certain characteristic diversities may be relied upon as the inherited features of a tribe or race, or whether they are not the mere result of artificial causes originating in long perpetuated national customs and nursery usages. If the latter is indeed the case, then they pertain to the materials of archæological rather than of ethnological deduction, and can no longer be employed as elements of ethnical classification.

The idea that the peculiar forms of certain ancient European skulls is traceable to the use of the cradle-board, or other nursing usages, is rapidly gaining ground, with extended observations. My own ideas, formed at an earlier date, were first published in 1857,† but it now appears that the same idea had occurred to Dr. L. A. Gosse, and received by him a wider application. In his "Essai sur les Déformations artificielles du Crâne," he has not only illustrated the

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* Physical Type of the American Indian, p. 326.

† Edin. Philosoph. Jour., N. S., Vol. VII, p. 25; Canadian Journal, Vol. II, p. 426.

general subject of artificial causes as a means of accounting for abnormal cranial forms, but he thus incidentally notices the peculiarity referred to in Scottish and Scandinavian skulls, and traces it to the same probable source of the cradle-board. His remarks are: "Passant dans l'ancien continent, ne tardons-nous pas à reconnaitre que ce berceau plat et solide y a produit des effets analogues. Les anciens habitans de la Scandinavie et de la Calédonie devaient s'en servir. si l'on en juge par la forme de leurs crânes."* Dr. Gosse also adds: "Vésale (*Opera*, lib. I, cap. v, § 25) nous apprend que la deformation occipitale s'observait même chez les Germains de son époque: '*Germani vero compresso plerumque occipite et lato capite spectantur, quod pueri in cunis dorso semper incumbant, ac manibus fere citra fasciarum usum, cunarum lateribus utrinque alliguntur.*' De même qu'en Amerique, cette pratique, en Allemagne, devait être commune aux deux sexes.'"

More recently Dr. J. Barnard Davis has illustrated the same subject, both in the later decades of the *Crania Britannica* and in a memoir in the *Natural History Review* for July, 1862, entitled "Notes on the Distortions which present themselves in the Crania of the Ancient Britons."

Whilst the error of an undue estimate of the extent of such deforming and reforming influences must be guarded against, it is obvious that they will henceforth require to be taken into account in every attempt to determine ethnical classification by means of physical conformation. Every scheme of the craniologist for systematizing ethnical variations of cranial configuration, and every process of induction pursued by the ethnologist from such data, proceed on the assumption that such varieties in the form of cranium are constant within certain determinate limits, and originate in like natural causes with the features by which we distinguish one nation from another. By like means the comparative anatomist discriminates between the remains of the *Bos primigenius*, the *Bos longifrons*, and other kindred animal remains, frequently found alongside the human skeleton, in the barrow; and by a similar crucial comparison the craniologist aims at classifying the crania of the ancient Briton, Roman, Saxon, and Scandinavian, apart from any aid derived from the evidence of accompanying works of art. But if it be no longer disputable that the human head is liable to modification from external causes, so that one skull may have been subjected to lateral compression, resulting in the elongation and narrowing of its form, while another under the influence of occipital pressure may exhibit a consequent abbreviation in its length, accompanied by parietal expansion, it becomes indispensable to determine some data whereby to eliminate this perturbing element before we can ascertain the actual significance of national skull-forms. If, for example, as appears to be the case, the crania from British graves of Roman times reveal a different form from that of the modern Celtic Briton, the cause may be an intermixture of races, like that which is clearly traceable among the mingled descendants of Celtic and Scandinavian blood in the north of Scotland; but it may also be in part, or wholly, the mere result of a change of national customs following naturally on conquest, civilization, and the abandonment of Paganism for Christianity.

It is, is this respect, that the artificial causes tending to alter the natural conformation of the human head invite our special study. They appear at present purely as disturbing elements in the employment of craniological tests of classification. It is far from improbable, however, that when fully understood they may greatly extend our means of classification; so that when we have traced to such causes certain changes in form, in which modern races are known to differ from their ethnical precursors, we shall be able to turn the present element of disturbance to account, as an additional confirmation of truths established by inductive craniology. Certain it is, however, whatever value may attach to

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* *Essai sur les Déformations artificielles du Crâne,*" p. 74, Dr. L. A. Gosse, 1855.

the systematizing of such artificial forms, that they are of frequent occurrence, apart altogether from such configuration as is clearly referable to the application of mechanical pressure in infancy with that express object in view; or, again, as is no less obviously the result of posthumous compression. But, though the deforming processes designedly practiced among ancient and modern savage nations lie beyond the direct purpose of the present inquiry, they are calculated to throw important light on the approximate results of undesigned compression and arrested development.

Among the Flathead Indian tribes of Oregon and Columbia river, where malformation of the skull is purposely aimed at, the infant's head is tightly bound in a fixed position, and maintained under a continuous pressure for months. But it is a mistake to suppose that in the ordinary use of the cradle-board the Indian pappoose is subject to any such extreme restraint. The objects in view are facility of nursing and transport, and perfect safety for the child. But those being secured, it is nurtured with a tenderness of maternal instinct surpassing that of many savage nations. The infant is invariably laid on its back. but the head rests on a pillow or mat of moss or frayed bark, and is not further restrained in a fixed position than necessarily results from the posture in which the body is retained by the bandages securing it in the cradle. This fact I have satisfied myself of from repeated observations. But the consequence necessarily is, that the soft and pliant bones of the infant's head are subjected to a slight but constant pressure on the occiput during the whole protracted period of nursing, when they are peculiarly sensitive to external influences. Experiments have shown that at that period the bones specially affected by the action of the cradle-board are not only susceptible of changes, but liable to morbid affections, dependent on the nature of the infant's food. Lehmann supposes the *craniotabes* of Elsässer to be a form of rachitis which affects the occipital and parietal bones during the period of suckling; and Schlossberger ascertained by a series of analyses of such bones that the 63 per cent. of mineral constituents found in the normal occipital bones of healthy children during the first year diminished to 51 per cent in the thickened and spongy bone.* The fluctuations in proportion of the mineral constituents of bones are considerable, and vary in the different bones, but in the osseous tissue they may be stated at 67 to 70 per cent. It is obvious, therefore, that, under the peculiar physiological condition of the cranial bones during the period of nursing, such constant mechanical action as the occipital region of the Indian pappoose is subjected to must be productive of permanent change. The child is not removed from the cradle board when suckling and is not therefore liable to any counteracting lateral pressure against its mother's breast. Trifling as it may appear, it is not without interest to have the fact brought under our notice by the disclosures of ancient barrows and cists, that the same practice of nursing the child, and carrying it about bound to a flat cradle board, prevailed in Britain and the north of Europe long before the first notices of written history reveal the presence of man beyond the Baltic or the English channel, and that, in all probability, the same custom prevailed continuously from the shores of the German Ocean to Behring's Straits. All the effects of such a universally prevalent practice, operating to produce uniform results on the form of the skull and brain, are scarcely yet fully estimated; but that it has affected the form of the head wherever it has been practiced I entertain no doubt. One effect of the continuous pressure on the infant skull must be to bring the edges of the bones together, and thereby to retard or arrest the growth of the bone in certain directions. Where this leads to ossification at a very early period, its tendency must be to limit the direction in which the further expansion of the brain takes place, and so still further to affect the permanent shape of the head. The tendency of the pressure to produce some of the

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* Schlossberger, Arch., f. phys. Heilk. Lehmann, Physiol. Chem., Vol. III, p. 28.

results here ascribed to it is proved by the premature ossification of sutures in many of the artificially deformed American crania.

Among the numerous objects of ethnological interest brought home by the United States Exploring Expedition, and now in the possession of the Smithsonian Institution, is a collection of thirty-four Flathead skulls. These I have examined with minute care. Some of them exhibit the most diverse forms of distortion, with the forehead sloping away at an abrupt angle from the eyebrow, or flattened into a disc, so as to present in front the appearance of a hydrocephalous head, and in profile the side of a narrow wedge. Many of them are also characterized by wormian bones and other abnormal formations at the sutures, and the distinct definition of a true supra-occipital bone is repeatedly apparent. In the majority of them the premature ossification, and the occasional entire obliteration of sutures, the gaping of others, and even traces of fracture, or false sutures, may be observed.

It is marvellous to see the extraordinary amount of distortion to which the skull and brain may be subjected without seemingly affecting either health or intellect. The coveted deformity is produced partly by actual compression, and partly by the growth of the brain and skull being thereby limited to certain directions. Hale, the ethnographer of the Exploring Expedition, after describing the process as practiced among the Chinooks, remarks: "The appearance of the child when just released from this confinement is truly hideous. The transverse diameter of the head above the ears is nearly twice as great as the longitudinal, from the forehead to the occiput. The eyes, which are naturally deep-set, become protruding and appear as if squeezed partially out of the head."* Mr. Paul Kane, in describing to me the same appearance, as witnessed by him on the Columbia river, compared the eyes to those of a mouse strangled in a trap. The appearance is little less singular for some time after the child has been freed from the constricting bandages, as shown in an engraving from one of Mr. Kane's sketches of a Chinook child seen by him at Fort Astoria.†

In after years the brain, as it increases, partially recovers its shape; and in some of the deformed adult skulls one suture gapes, while all the rest are ossified; and occasionally a fracture or false suture remains open. An adult skull of the same extremely deformed shape, among those brought home by the Exploring Expedition, illustrates the great extent to which the brain may be subjected to compression and malformation without affecting the intellect. It is that of a Nisqually chief, procured from his canoe-bier in Washington Territory. (No. 4549.) The internal capacity, and consequent volume of brain, is 95 cubic inches. The head is compressed into a flattened disc, with the forehead receding in a straight line from the nasal suture to the crown of the head, while the lambdoidal suture is on the same plane with the foramen magnum. The sutures are nearly all completely ossified, and the teeth ground quite flat, as is common with many of the tribes in the same region, and especially with the Walla-Walla Indians on the Columbia river, who live chiefly on salmon, dried in the sun, and invariably impregnated with the sand which abounds in the barren waste they occupy. I assume the unimpaired intellect of the Nisqually chief from his rank. The Flathead tribes are in the constant habit of making slaves of the Roundheaded Indians; but no slave is allowed to flatten or otherwise modify the form of her child's head, that being the badge of Flathead aristocracy. As this has been systematically pursued ever since the tribes of the Pacific coast were brought under the notice of Europeans, it is obvious that if such superinduced deformity developed any general tendency to cerebral disease, or materially affected the intellect, the result would be apparent in the degeneracy or extirpation of the Flathead tribes. But so far is this from being

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* Ethnography of the U. S. Exploring Expedition, p. 216.
† Prehistoric Man, Vol. II, p. 320.

the case, that they are described by traders and voyagers as acute and intelligent. They are, moreover, an object of dread to neighboring tribes who retain the normal form of head, and they look on them with contempt as thus bearing the hereditary badge of slaves.

The child born to such strange honors is laid, soon after its birth, upon the cradle-board, an oblong piece of wood, sometimes slightly hollowed, and with a cross-board projecting beyond the head to protect it from injury. A small pad of leather, stuffed with moss or frayed cedar-bark, is placed on the forehead and tightly fastened on either side to the board; and this is rarely loosened until its final removal before the end of the first year. The skull has then received a form which is only slightly modified during the subsequent growth of the brain. But the very same kind of cradle is in use among all the Indian tribes. It is, indeed, varied as to its ornamental adjuncts and non-essential details, but practically it resolves itself, in every case, into a straight board to which the infant is bound; and as it is retained in a recumbent position, the pressure of its own weight during the period when, as has been shown, the occipital and parietal bones are peculiarly soft and compressible, is thus made to act constantly in one direction. This I assume to have been the cause of the vertical or otherwise flattened occiput in the ancient British brachycephalic crania. The same cause must tend to increase the characteristic shortness in the longitudinal diameter, to produce the premature ossification of certain sutures, and to shorten the zygoma, with probably, also, some tendency to make the arch bulge out in its effort at subsequent full growth, and so to widen the face.

Fashion regulates to some extent the special form of head aimed at among the various Flathead tribes. Some compress the whole brain into a flattened disc which presents an enormous forehead in full face; others run to the opposite extreme, and force it into an abrupt slope immediately above the eyebrows, so as to give an idiotic look to the seemingly brainless face. Individual caprice, and probably, also, clumsy manipulation, combine frequently to produce a shapeless deformity of skull, in which the opposite sides present no trace of correspondence, and every vestige of ethnical character is effaced. Among the Newatees, a tribe on the north end of Vancouver's island, the head is forced into a conical shape, by means of a cord of deer's-skin padded with the inner bark of the cedar tree, frayed into the consistency of soft tow. This forms a cord about the thickness of a man's thumb, which is wound round the infant's head, and gradually compresses it into a tapering cone. Commander Mayne, in his narrative of his visit to Columbia and Vancouver island, gives a sketch of an Indian girl, and adds in reference to it: "Those who have only seen certain tribes may be inclined to think the sketch exaggerated, but it was really drawn from measurement, and she was found to have eighteen inches of flesh from her eyes to the top of her head." From the extraordinary amount of deformity which I have seen produced by such means, it would be unwise to reject any narrative of an intelligent eye-witness relative to extreme examples of such abnormal heads. I should be inclined, however, to suspect that in the case of the girl drawn by Commander Mayne, he was deceived by her mode of dressing her hair. I have engraved in my "Prehistoric Man"* the head of a Newatee chief of the same conical form, and with the hair gathered into a knot on the top of the head. The latter practice is in constant use for increasing the apparent elevation of the favorite conoid head-form. In all such cases the volume of brain appears to remain little, if at all, affected, and the extreme proportions in length, breadth, or height of the skull must be limited by the capacity of the brain, whatever be the fantastic shape it is made to assume. In the case of the girl from Vancouver's island, part of the extreme

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* Prehistoric Man, Vol. I, p. 317.

height of her singularly formed head was probably an artificial pad over which the hair was drawn.

Compared with such extreme deformation, the traces of artificial change on the forms of British skulls are trifling. They are, however, all the more important from their liability to be confounded with true congenital forms, as in the case of the flattened occiput. Dr. Davis has applied the term "parieto-occipital flatness," where the results of artificial compression in certain British skulls extend over the parietals with the upper portion of the occipital, and he appears to regard this as something essentially distinct from the vertical occiput.* But it is a form of common occurrence in Indian skulls, and is in reality the most inartificial of all the results of the undesigned pressure of the cradle-board. This will be understood by a very simple experiment. If the observer lie down on the floor, without a pillow, and then ascertain what part of the back of the head touches the ground, he will find that it is the portion of the occiput immediately above the lambdoidal suture, and not the occipital bone. When the Indian mother places a sufficiently high pillow for her infant, the tendency of the constant pressure will be to produce the vertical occiput; but where, as is more frequently the case, the board has a mere cover of moss or soft leather, then the result will be just such an oblique parietal flattening as is shown on a British skull from the remarkable tumulus near Littleton Drew, Wiltshire.†

But there are other sources of modification of the human skull in infancy, even more common than the cradle-board. More than one of the predominant head-forms in Normandy and Belgium are now traced to artificial bandaging; and by many apparently trifling and unheeded causes, consequent on national customs, nursing usages, or the caprices of dress and fashion, the form of the head may be modified in the nursery. The constant laying of the infant to rest on its side, the pressure in the same direction in nursing it, along with the fashion of cap, hat, or wrappage, may all influence the shape of head among civilized nations, and in certain cases tend as much to exaggerate the naturally dolichocephalic skull as the Indian cradle-board increases the short diameter of the opposite type. Such artificial cranial forms as that designated by M. Foville, the *Tête annulaire*, may have predominated for many centuries throughout certain rural districts of France, solely from the unreasoning conformity with which the rustic nurse adhered to the traditional or prescriptive bandages to which he ascribes that distortion. All experience shows that such usages are among the least eradicable, and long survive the shock of revolutions that change dynasties and efface more important national characteristics.

The effect, as we have already seen, which a constant familiarity with the results of extreme artificial deformation on American crania produced on Dr. Morton's mind, was to lead him to ignore all distinctions of ethnical form, and to retract his earlier idea that the elongated Peruvian crania were artificial exaggerations of a head of great natural length. Originally he had adopted the conclusion that the long-headed Peruvians were a more ancient people than what he called the Inca tribes, and distinguished from them by their cranial configuration; but this he abandoned at a later period, and assumed that every skull found on the American continent, whatever might be the extreme variation in opposite directions from his assumed typical form, had been naturally a short globular skull, with low retreating forehead and vertical occiput. On the fallacy involved in such a conclusion it is unnecessary to make further comment, as the evidence which appears to confute it has already been produced. But the disclosures of the essentially diverse types of skull in the ancient cemeteries of Peru appear to me to present some highly interesting analogies to the discoveries made in British barrows. The repeated opportunities I have enjoyed of

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* *Nat. Hist. Review*, July, 1862. *Athenæum*, Sept. 27, 1862, p. 402.

† *Crania Britannica*, Decade III, Plate 24.

examining the Mortonian and other American collections have satisfied me of the occurrence of both dolichocephalic and brachycephalic crania, not only as the characteristics of distinct tribes, but also among the contents of the same Peruvian cemeteries, not as examples of extreme latitudes of form in a common race, but as the results of the admixture either of conquering and subject races, or of distinct classes of nobles and serfs, most generally resulting from the predominance of conquerors. Among the Peruvians the elongated cranium pertained to the dominant race; and some of the results of later researches in primitive British cemeteries, and especially the disclosures of the remarkable class of chambered barrows, seem to point to an analogous condition of races. That the Uley and West Kennet skulls may have been laterally compressed, while the Codford barrow and other brachycephalic skulls have been affected in the opposite direction, appears equally probable. But such artificial influences only very partially account for the great diversity of type; and no such causes, even if brought to bear in infancy, could possibly convert the one into the other form.

But as the cranial forms, both of the Old and New World, betray evidences of modification by such artificial means, so also we find in ancient Africa a diverse form of head, to which art may have contributed, solely by leaving it more than usually free from all extraneous influences. Such at least is a conclusion suggested to my mind from the examination of a considerable number of Egyptian skulls. Among familiar relics of domestic usages of the ancient Egyptians is the pillow designed for the neck, and not the head, to rest upon. Such pillows are found of miniature sizes, indicating that the Egyptian passed from earliest infancy without his head being subjected even to so slight a pressure as the pillow, while he rested recumbent. The Egyptian skull is long, with great breadth and fulness in the posterior region. In its prominent, rounded parieto-occipital conformation, an equally striking contrast is presented to the British brachycephalic skull, with truncated occiput, and to the opposite extreme characteristic of the primitive dolichocephalic skull; though exceptional examples are not rare. This characteristic did not escape Dr. Morton's observant eye; and is repeatedly indicated in the *Crania Ægyptiaca* under the designation, "tumid occiput." It also appeared to me, after careful examination of the fine collection formed by him, and now in the Academy of Natural Sciences of Philadelphia, that the Egyptian crania are generally characterized by considerable symmetrical uniformity: as was to be anticipated, if there is any truth in the idea of undesigned artificial compression and deformation resulting from such simple causes as accompany the mode of nurture in infancy.

The heads of the Fiji Islanders supply a means of testing the same cause, operating on a brachycephalic form of cranium; as most of the islanders of the Fiji group employ a neck pillow nearly similar to that of the ancient Egyptians, and with the same purpose in view: that of preserving their elaborately dressed hair from dishevelment. In their case, judging from an example in the collection of the Royal College of Surgeons of London, the occipital region is broad, and presents in profile a uniform, rounded conformation passing almost imperceptibly into the coronal region. Indeed, the broad, well-rounded occiput is considered by the Fijians a great beauty. The bearing of this, however, in relation to the present argument, depends on whether or not the Fiji neck-pillow is used in infancy, of which I am uncertain. The necessity which suggests its use at a later period does not then exist; but the prevalent use of any special form of pillow for adults is likely to lead to its adoption from the first. In one male Fiji skull brought home by the United States Exploring Expedition (No. 4581) the occiput exhibits the characteristic full, rounded form, with a large and well-defined supra-occipital bone. But in another skull in the same collection—that of Vendovi, Chief of Kantavu, who was taken prisoner by the United States ship Peacock, in 1840, and died at New York in 1842—the occiput, though full, is slightly vertical. The occipital development of the Fiji cranium is the more interesting as we are now familiar with the fact that an artificially flattened occiput is of common occurrence among the islanders of the Pacific ocean. "In the Malay race," says Dr. Pickering, "a more marked peculiarity, and one very generally observable, is the elevated occiput, and its slight projection beyond the line of the neck. The Mongolian traits are heightened artificially in the Chinooks; but it is less generally known that a slight pressure is often applied to the occiput by the Polynesians, in conformity with the Malay standard."* Dr. Nott, in describing the skull of a Kanaka of the Sandwich Islands who died at the Marine Hospital at Mobile, mentions his being struck by its singular occipital formation; but this he learned was due to an artificial flattening which the islander had stated to his medical attendants in the hospital, was habitually practiced in his family.† According to Dr. Davis, it is traceable to so simple a source as the Kanaka mother's habit of supporting the head of her nursling in the palm of her hand.‡ Whatever be the cause, the fact is now well established. The occipital flattening is clearly defined in at least three of the Kanaka skulls in the Mortonian collection: No. 1300, a male native of the Sandwich Islands, aged about forty; No. 1308, apparently that of a woman, from the same locality; and in No. 695, a girl of Oahu, of probably twelve years of age, which is markedly unsymmetrical, and with the flattening on the left side of the parietal and occipital bones. The Washington collection includes fourteen Kanaka skulls, besides others from various islands of the Pacific, among which several examples of the same artificial formation occur: *e. g.*, No. 4587, a large male skull, distorted and unsymmetrical; and No. 4367, (female?) from an ancient cemetery at Wailuka, Mani, in which the flattened occiput is very obvious.

The traces of purposed deformation of the head among the islanders of the Pacific have an additional interest in their relation to one possible source of South American population by oceanic migration, suggested by philological and other independent evidence. But for our present purpose the peculiar value of those modified skulls lies in the disclosures of influences operating alike undesignedly, and with a well-defined purpose, in producing the very same cranial conformation among races occupying the British Islands in ages long anterior to earliest history; and among the savage tribes of America and the simple islanders of the Pacific in the present day. They illustrate, with peculiar vividness, the primitive condition of social life out of which the civilization of modern Europe has been educed; and, while they pertain to more modern eras than the traces of human art contemporaneous with the extinct mammals of the drift, they revivify for us, with even clearer distinctness than the rude implements of flint and stone found in early graves, the condition of the British Islanders of prehistoric times.

PART III.

PHYSICAL ETHNOLOGY.

PRIMITIVE ART-TRACES.

The evidences of an assumed cranial and physical unity pervading the aborigines of the American continent disappear upon a careful scrutiny, and the

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* Pickering's Races of Man, p. 45.
†Types of Mankind, p. 436.
‡ *Crania Britannica*, Dec. III, pl. 24, (4.)

like results follow when the same critical investigation is applied to other proofs adduced in support of this attractive but unsubstantial theory. Dr. Morton, after completing his elaborate and valuable illustrations of American craniology, introduces an engraving of a mummy of a Muysca Indian of New Granada, and adds: "As an additional evidence of the unity of race and species in the American nations, I shall now adduce the singular fact that, from Patagonia to Canada, and from ocean to ocean, and equally in the civilized and uncivilized tribes, a peculiar mode of placing the body in sepulture has been practiced from immemorial time. This peculiarity consists in the sitting posture."* The author accordingly proceeds to marshal his evidence in proof of the practice of such a mode of interment among many separate and independent tribes; nor is it difficult to do so, for it was a usage of greatly more extended recognition than his theory of "unity of race and species" implies. It was a prevailing, though by no means universal mode of sepulture among the tribes of the New World, as it was among many of those of the Old, recorded by the pen of Herodotus, and proved by sepulchral disclosures pertaining to still older eras. The British cromlechs show that the very same custom was followed by their builders in primitive times. The ancient barrows of Scandinavia reveal the like fact, and abundant evidence proves the existence of such sepulchral rites, in ancient or modern times, in every quarter of the globe; so that, if the prevalence of a peculiar mode of interment of the dead may be adduced as evidence of the unity of race and species, it can only operate by reuniting the lost links which restore to the red man his common share in the genealogy of the sons of Adam. But ancient and modern discoveries also prove considerable diversity in the sepulchral rites of all nations. The skeleton has been found in a sitting posture in British cromlechs, barrows, and graves, of dates to all appearance long prior to the era of Roman invasion, and of others unquestionably subsequent to that of Saxon immigration; but evidences are found of cremation and urn-burial, in equally ancient times; of the recumbent skeleton under the cairn, and barrow, in the stone cist, and in the rude sarcophagus hewn out of the solid trunk of the oak; and in this, as in so many other respects, the British microcosm is but an epitome of the great world. Norway, Denmark, Germany, and France all supply similar evidences of varying rites; and ancient and modern customs of Asia and Africa confirm the universality of the same. In the Tonga and other islands of the Pacific, as well as in the newer world of Australia, the custom of burying the dead in a sitting posture has been repeatedly noted; but it is not universal even among them; nor was it so in America, though affirmed by Dr. Morton to be traceable throughout the northern and southern continents, and, by its universality, to afford "collateral evidence of the affiliation of all the American nations." So far is this from being the case that nearly every ancient and modern sepulchral rite appears to have had its counterpart in the New World; and in this, as in many other respects, its isolation from the older continents in affinities and corresponding characteristics, if not in actual intercourse, disappears on more extended research. To follow out all the varied indications of such analogy or parallelism would lead to a very extensive range of inquiry, which I shall not now enter upon. But one seemingly trifling analogy, traceable in the correspondence of the rude weapons and implements of flint and stone wrought and fashioned by the aborigines of America, with those recovered from the ancient barrows of northern Europe, connects the early traces of man in both hemispheres by means of arts which are acquiring a new and comprehensive significance.

The development of primitive archæology, by the labors of Thomsen and his Danish fellow-laborers, into a systematic science, laid the foundations for that novel and profoundly interesting inquiry which now tempts the ablest geologists

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* *Crania Americana*, p. 244.

from the study of the palæosoic rocks to the recent sedimentary cave deposits and the superficial drift. The investigations of British archæologists, following in the footsteps of their northern precursors, have now familiarized us with the character of that primitive art so widely diffused throughout those ages embraced within the European STONE PERIOD. That age of stone derives its special characteristic from the occurrence of numerous examples of arms and implements of flint or stone, many of which are wrought with considerable skill and finished with minute care. Others, however, are sufficiently rude and unshaped to illustrate the most artless efforts of primitive mechanical skill. These are formed from flint nodules and pieces of rock by mere blows from another stone, guided, in the case of the flint-workers, by a knowledge of the concoidal fracture of the flint and the consequent facility of its reduction to long and narrow splinters, readily convertible into wedges, chisels, knives, and lance or arrow heads. The simplest implements of this class are frequently water-worn stones, partially hewn, so as to reduce one end to a sharp or angular edge. But, while specimens of such rudimentary art are not uncommon, many more are chipped into symmetrical form with minute care and are ground to a fine edge, or even wrought into artistic forms and polished throughout the whole surface. To those it has been customary with many to apply the epithet *Celtic*, and so to assume their origin from that people who immediately preceded the Romans in the scenes of their latest European conquests. This, however, is rather an assumption than any well-grounded induction; and, though revived by M. Boucher de Perthes in his *Antiquités Celtique et Antédiluviennes*, (1849,) had been previously set aside by Thomsen, Worsaae, Nilsson, and other Scandinavian archæologists, and, at the very time, was challenged in a communication submitted by me to the ethnological section of the British Association, entitled: *An Inquiry into the Evidence of Primitive Races in Scotland prior to the Celtæ.*[*] But that which was a bold surmise in 1850 seems an insignificant and self-evident truism in the light of the well-established facts, and cautious yet comprehensive inductions, relative to the flint implements found in the same drift of England and France alongside of bones of the *Elephas primigenius*, *Rhinoceros tichorhinus*, *Equus fossilis*, *Felis spelæa*, *Hyena spelæa*, and numerous other extinct mammals.

The facts connected with the discovery of works of human art associated in undisturbed gravel with the fossil bones of extinct quadrupeds, or in corresponding diluvial strata both of France and England, are now too well known to need recapitulation. It is indeed a significant fact that some of them have been long familiar to British antiquaries, though the true bearings of their discovery are only now beginning to be recognized. So early as 1715, a weapon of flint, six and a half inches long, and rudely chipped into the form of a spear-head, was dug up at Black Mary's, near Gray's Inn Lane, London, along with an elephant's tooth, and apparently lying beside the entire skeleton of a fossil elephant.[†] This curious evidence of the remote presence of man in the most populous centre of his modern civic settlements lay unheeded in the collections of the British Museum for nearly a century and a half. Meanwhile, towards the close of the eighteenth century, another remarkable discovery of the same kind was made at Hoxne, in Suffolk, in gravel at a depth of twelve feet in a stratified soil, and immediately underneath a horizontal bed of sand mixed with shells of existing fresh-water and land mollusca, and with gigantic fossil bones. An account of this discovery was communicated by Mr. John Frere to the Society of Antiquaries of London in 1797,[‡] and specimens of the flint implements were deposited in the society's museum, where they are still preserved.

It is interesting and highly satisfactory to know that not only had such facts

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[*] British Association Report, 1850, p. 144.

[†] Archæologia, Vol. XXXVIII, p. 301.

[‡] Ibid, Vol. XIII, p. 204.

been on record thus long before their significance was appreciated, but the rude implements of the drift have been exhibited in the collections of the British Museum and the Society of Antiquaries of London as the works of man; nor was it till the corresponding discoveries in the French drift, and the minute examination of the stratified gravel in which they were found, had led Rigollot, Prestwich, Lyell, and other competent authorities to deduce evidence of a lapse of many ages between the era of the fossil implements and that to which Romano-Gaulish relics belong, that any one dreamt of questioning their human origin. More recently similar implements have been found in the same diluvial gravel and clay in which remains of the gigantic fossil mammals abound, in Suffolk, Kent, Bedfordshire, and other post-pliocene deposits of the south of England;* and not only is their artificial character undoubted, but the correspondence between the drift implements of France and England is so close as to be at once recognized by the workmen employed in the excavations both at Hoxne and Abbeville.

Such discoveries have naturally led to many startling speculations relative to the apparent lapse of a vast period of time between the era of the drift race and the earliest dates of authentic history, and it has been specially noted that while the drift implements resemble in material those most frequently found in the graves of northern Europe's stone period, they present a striking contrast to the small and well-finished implements chiefly pertaining to such sepulchral deposits, and seem to be the memorials of an age of ruder strength and still more infantile skill. Such a conclusion, if fully borne out, is all the more important as it has otherwise been noted as a highly interesting fact, that so general a correspondence is traceable between the majority of the flint and stone weapons and implements found in ancient European graves and those still manufactured by the aborigines of the Pacific islands, and throughout the American continent, that they seem like the products of the same mechanical instinct, repeating itself under similar circumstances in the arts of savage man.

When Mr. Joseph Prestwich proceeded to Abbeville, in 1859, to investigate the discoveries reported by M. Boucher de Perthes, he was accompanied by Mr. John Evans, F. S. A., who has since communicated the results of their observations to the Society of Antiquaries.† He notes that, so far as hitherto observed, the implements found in the drift are formed exclusively of flint, and these he classifies, for convenience of further reference, under three heads:

1. Flint flakes, apparently intended for arrow-heads or knives.
2. Pointed weapons, some probably lance or spear heads.
3. Oval or almond-shaped implements presenting a cutting edge all round.

The objects first named most nearly resemble a numerous class found in ancient sepulchral deposits, but they are produced by so simple a process, and betray such partial traces of artificial modification, that even when their character as works of art is indisputable, they bear so much resemblance to similar simple natural forms as to be of little value as conclusive evidence of human design or workmanship. But the case is altogether different with the two other classes; and the opinion has been repeatedly expressed that they present little or no analogy in form to any of the works described by Danish, British, or other archæologists of Europe, as pertaining to the so-called Stone Period. Accordingly, after having described the repeated discovery of flint flakes in the drift, as in the sand and gravel near Abbeville, and in the corresponding formation at Menchecourt, where Mr. Prestwich witnessed their exhumation, Mr. Evans acknowledges the uncertainty pertaining to any argument based solely on such evidence; and still further specifies as an element rendering them valueless for

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* Quarterly Journal of the Geological Society, Vol. XVII, p. 362.

† On the Occurrence of Flint Implements in Undisturbed Beds of Gravel, Sand, and Clay. By John Evans, F. S. A., F. G. S. Archæologia, Vol. XXXVIII, p. 280.

the purpose of those who are seeking for indications of man's presence in such localities at a period separated by vast ages from the earliest beginnings of history, that, "though closely resembling the flakes of flint which have been considered as affording evidence of man's existence when found in ossiferous caverns, this class is not of much importance in the inquiry, because, granting them to be of human work, *there is little by which to distinguish them from similar implements of more recent date*." Of the artificial origin and peculiar characteristics of the two other classes of implements no such doubt can be entertained, and Mr. Evans accordingly proceeds to remark: "The case is different with the implements of the second class, those analogous in form to spear or lance heads. Of these there are two varieties: the one with a rounded cutting point, its general outline presenting a sort of parabolic curve,* the other acutely pointed, with the sides curved slightly inward.† These have received from the workmen of St. Acheul the name of '*langues de chat*,' from their fancied resemblance to a cat's tongue. The sides of both kinds are brought to an edge by chipping, but are not so sharp as the point, and altogether these weapons seem better adapted for piercing than for cutting. In length they vary from about four inches to eight, or even nine inches. Both shapes are generally more convex on one side than on the other, the convexity in some cases almost amounting to a ridge. They are usually truncated at the base, and not unfrequently at that end show a portion of the original surface of the flint; in some specimens the butt end is left very thick, as if to add impetus to any blow given with the implement. The remarkable feature about them is their being adapted only to cut or pierce at the pointed end, whereas in the ordinary form of stone hatchet or celt the cutting edge is almost without exception at the broad end, while the more pointed end seems intended for insertion into the handle or socket and the sides are generally rounded or flat, and not sharp.

"These spear-shaped weapons from the drift are, on the contrary, not at all adapted for insertion into a socket, but are better calculated to be tied to a shaft or handle, with a stop or bracket behind their truncated end. Many of them, indeed, seem to have been intended for use without any handle at all, the rounded end of the flints from which they were formed having been left unchipped, and presenting a sort of natural handle. It is nearly useless to speculate on the purposes to which they were applied, but, attached to poles, they would prove formidable weapons for encounter with man, or the larger animals, either in close conflict or thrown from a distance as darts.

"It has been suggested by M. de Perthes that some of them may have been used merely as wedges for splitting wood, or, again, they may have been employed in grubbing for esculent roots, or tilling the ground, assuming that the race who formed them was sufficiently advanced in civilization. *This much, I think, may be said of them with certainty, that they are not analogous in form with any of the ordinary implements of the so-called Stone Period.*

"The same remark holds good with regard to the third class into which I have divided these implements, viz., those with a cutting edge all round.‡ In general contour they are usually oval, with one end more sharply curved than the other, and occasionally coming to a sharp point, but there is a considerable variety in their form, arising probably from defects in the flints from which they were shaped; the ruling idea is, however, that of the oval, more or less pointed. They are generally almost equally convex on the two sides, and in length vary from two to eight or nine inches, though, for the most part, only about four or five inches long. The implements of this form appear to be most abundant in

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*Archæologia, Vol. XXXVIII, pl. XV, No 1.
†Ibid, pl. XV, No. 2.
‡Archæologia, Vol. XXXVIII, pl. XV, No. 3.

the neighborhood of Abbeville, while those of the spear-shape prevail near Amiens."

Mr. Evans then points out that, among the implements discovered in Kent's Hole Cavern, there were some identical in form with the oval flints from Abbeville; but he adds, "in character they do not resemble any of the ordinary stone implements with which I am acquainted."

It is obvious that a great, if not undue stress is laid on this dissimilarity between the flint implements of the Drift and those of the more recent Stone Period, with the assumed Celtic origin of its flint and stone manufactures. Nor is this wonderful when the vast interval is considered which the geologist now assumes to intervene between the production of the two classes of works. Sir Charles Lyell, when addressing the Geological Section of the British Association, remarked, with cautious yet suggestive force, in reference to the secular phenomena indicated by the fluviatile gravel of Abbeville and Amiens:

"To explain these changes, I should infer considerable oscillations in the level of the land in that part of France; slow movements of upheaval and subsidence, deranging, but not wholly displacing, the course of the ancient river. Lastly, the disappearance of the elephant, rhinocerus, and other genera of quadrupeds now foreign to Europe, implies, in like manner, a vast lapse of ages, separating the era in which the fossil implements were framed and that of the invasion of Gaul by the Romans."

If man's place in nature, and his true relation to the inferior orders of being be still undetermined, and the possibility of his development from the anthropoid apes or others of the lower animals be admitted, then the first indices of animal instinct passing into inventive mechanical skill will possess a peculiar significance. Whether, moreover, we accept or reject the unwelcome theory of the structural interval between the organization of man and the lower animals being the practical element of difference, and one sufficiently slight to require only an adequate lapse of time for its being bridged over by secondary causes in constant operation throughout the organic world; the comparison between the arts of ages so remote as those of the Drift Folk, and the British Celt of the Roman period, or the American Indian of the nineteenth century cannot be devoid of interest. But America also has her ancient, and possibly her drift flint-implements; and as the analogies between the works of her modern aborigines and those of the later European Stone Period are obvious and remarkable, though separated by at least two thousand years; a comparison of the oldest traces of human art on the two hemispheres may involve very significant disclosures in reference to the general question of the development of the mechanical and artistic faculties of man.

So impressed was my mind with the striking bearing of the supposed fact reiterated by Prestwich, Evans, Lyell, and others, of the uniformity of character, amounting to specific typical forms, and of the massive rudeness of the works of art of the drift, that it was with some sense of disappointment I received a flint instrument believed to have been recovered from the American drift, and found it fail in any correspondence with the post-pliocene manufactures of Europe. The characteristics assigned to the former, separate their era, to all appearance, by a diversity of the mental character expressed in them, as works of human art, from the nearly uniform flint and stone implements of the most ancient European stone period seemingly of historic times and existing savage nations. The confirmation it seems to lend to the idea of a condition of human intellect more rudimentary than that of the rudest savage hitherto known gives importance to the data on which such an influence rests. In the summer of 1852 I learned from Mr. William Hay, architect, of a flint implement recovered by a gold-digger from the drift near Pike's peak, Kansas Territory, and immediately instituted inquiries about it, not without some expectation of finding in it a repetition of one of the large typical forms of Abbeville or Amiens. In this, however, I was disappointed. The interesting object which is now in my own possession is a broken knife or lance-head, measuring in its present imperfect condition only $2\frac{1}{8}$ inches. I was placed in communication with the discoverer by Mr. Hay, in whose employment he had formerly been; and on my applying to him for information as to the precise circumstances under which the flint implement had been discovered, he presented it to me, along with the desired statement. Mr. P. A. Scott is an intelligent Canadian, formerly in business as a carpenter at Cobourg, Upper Canada, who, in 1850, joined a party about to start on an expedition to the gold diggings; and while engaged in the search for gold at the Grinnell Leads, in Kansas Territory, he found the imperfect flint implement, figured here, the size of the original, at a depth of upward of fourteen feet from the surface. The spot where this discovery was made is in the Blue Range of the Rocky mountains, in an alluvial bottom, and distant several hundred feet from a small stream called Clear creek. A shaft was sunk, passing through four feet of rich, black soil, and, below this, through upward of ten feet of gravel, reddish clay, and rounded quartz. Here the flint implement was found, and its unmistakable artificial form so impressed the finder that he secured it, and carefully noted the depth and the character of the strata under which it lay. Though the actual object corresponds more to the small and slighter productions of the modern Indian tool-maker than to the rude and massive drift implement which I had conjured up in fancy, it has no claims to more artistic skill. Under any circumstances it would be rash to build up comprehensive theories on a solitary case like this; but, though small, and otherwise dissimilar to the drift implements of France and England, there is nothing in the workmanship of the Grinnell Leads flint to suggest its origin at a later period; for it is only chipped into form with such rude skill as is fully equalled by that displayed in the former; and may, therefore, very well accord with the idea of the most rudimentary traces of art being alone discoverable in the manufactures of the Drift Period.

Fig. 10.

The growing favor with which this opinion is entertained is illustrated by the attempts made by Mr. Worsaæ and other Danish antiquaries to separate that Stone Period of prehistoric times, which they have hitherto considered in connexion with the cromlechs, banta-steins, and other primitive monuments of Sweden and Denmark, from another and greatly more remote era, or Flint Period, to which the recently explored *kjockkenmœddinger*, or shell mounds and coast refuse-heaps, are assigned. In these, numerous flint wedges and other implements of the rudest workmanship have been found; but, along with them, some rare specimens of well wrought and highly finished flint tools or weapons have occurred. These, indeed, some would still regard as stray relics of a later date, like the Indian weapons and sepulchral remains superficially deposited in the ancient mounds of the Mississippi valley. But Professor Steenstrup, who has been associated with Professors Forchhammer and Worsaæ since 1847, in the exploration of the *kjockkenmœddinger*, peat bogs, and other formations which enclose the ancient traces of man, entirely rejects the idea of any interval of separation between the Kjockkenmœdding Period, and the earliest and rudest stage of the Danish Stone Age. If, therefore, the two constitute one era, the purely exceptional character of all but the coarsely-shaped flint implements in the kjockkenmœdding tends to suggest the probability of further research leading to the discovery in the drift also of some of the more delicate and carefully finished flint tools.

In reality, however, the difference is more one of material than workmanship.

A certain class of flint axes are found, especially in Denmark, not only ground to an edge, but with the whole surface polished; but these are comparatively uncommon on the continent, and are only rarely found in Britain. The natural fracture of flint brings it nearly to the required shape for knives, arrow and lance heads, and axe-blades, without grinding. But it is otherwise with the amorphous trap, granite, and other hard rocks wrought into stone axes, &c. These had to be rubbed and ground into shape, and some of them are found polished with elaborate symmetry and finish. If stone implements should hereafter be recovered under circumstances indicative of a corresponding antiquity with the flint manufactures of the drift, the more intractable material will be found to have compelled the primitive workman to employ some amount of grinding and polishing on his rudest weapons.

The varied ethnological collections of the Smithsonian Institution, when completely arranged, will be found to illustrate many interesting points of comparative ethnic art. The examination of the Indian implements already displayed in its cabinets has now sufficed to recall to mind a flint implement in my own collection, the significance of which, as a possible relic of older races than the Red Indians of this continent, was overlooked by me at the time I acquired it. When passing, some years since, through the village of Lewiston, in the State of New York, I purchased from an itinerant vender of Indian bead-work some flint implements, chiefly arrow-heads, such as are constantly ploughed up on the sites of Indian settlements; but along with those was a large disc, or spear-head, of dark flint, 4¾ inches long by 3½ broad, which I was informed had been procured in the neighborhood in the process of sinking a well. Regarding it merely as an unusually large specimen of an Indian flint spear-head, I deposited it among other relics of the same class without further inquiry. But my visit to Washington has afforded me an opportunity of examining some similar discs of flint or hornstone, found under circumstances which give a new interest to the Lewiston implement. In one of the cabinets of the Smithsonian collection two large flint implements are deposited, which attracted my eye from their apparent correspondence to the oval or almond-shaped implements of the drift, made with a fractured cutting edge all around. A label attached to one of them is as follows: "Thirty of these found at the depth of eight feet, under a peaty formation, near Racine, Wisconsin; deposited by P. R. Hoy." Dr. Hoy is a contributor to various departments of the Smithsonian collections; and his name also repeatedly occurs in Lapham's "Antiquities of Wisconsin, Surveyed and Described." At page eight of that work the following statement is made on his authority: "Some workmen, in digging a ditch through a peat swamp, near Racine, found a deposit of discs of hornstone, about thirty in number. They were immediately on the clay at the bottom of the peat, about two feet and a half below the surface. Some of the discs were quite regular. They vary from half a pound to a pound in weight." Notwithstanding the discrepancy between the two accounts of the depth at which the implements were found, both statements probably refer to the same discovery.* The larger of the two specimens measures 5¼ inches

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* In answer to a letter addressed to Dr. Hoy, on this subject, Mr. Albert H. Hoy writes, January 25, 1863: "Dr. Hoy desires me to state that the flint discs were found in digging a ditch through the bottom of a ravine near this city, (Racine, Wisconsin,) formerly the bed of Root river, which enters the lake at this point. The doctor thinks these flints had been transported from some point and buried here by the Indians, as a sort of caché, in order that they might readily find them when they wished to construct arrow-heads, spear-points, and the like. From the nature of the peaty formation, the doctor thinks that the flints were deposited after the formation of the surrounding soil. It may be that the Indians purposely buried the flints in this moist situation that they might remain damp, as it is known that in this state flint is the easier worked or chipped, Some thirty more were found at one point, and had the appearance of being deposited in a pile." As no correction is made of the later depth assigned to their discovery, I presume it to be correct.

long by 3¾ broad, and the other is only a little smaller. The discovery of similar heaps of rudely formed discs of flint has been repeatedly made under circumstances much more obviously indicating their being placed for some specific purpose in the deposit from whence they were recovered; and the immense numbers of them occasionally heaped or systematically arranged on a single spot is a fact which may have some significance in illustration of the numerous flint implements recovered from the drift on very limited areas.

The researches of Messrs. Squier and Davis, in the mounds of Ohio, have revealed the fact that large deposits of such discs repeatedly occur in those ancient earthworks; and in a manuscript account of researches carried on more recently in the same locality, which I have had an opportunity of examining while at Washington, the following narrative occurs: "On the south side of the confluence of the Racoon and the south fork of the Licking river, at McMullen's inn, is a square earthwork, with a small circle attached to the west side. Some workmen, digging for clay in a brick-yard occupying part of the square, discovered a nest of 198 flint arrow-heads about two feet below the surface, all nicely set up on end, the smaller ones within and the larger without. Some were as large as a man's open hand, all neatly made, and of the same pattern." To this the explorer adds, as a singular fact: "All the arrow-heads I have obtained from out the mounds, or in similar deposits, are of this character or pattern."

Some uncertainty as to the occurrence of the modern forms of flint arrow-heads among the genuine deposits of the mound-builders of the Ohio valley is occasioned by the practice of interment, by the forest tribes, superficially in the ancient mounds. Certain it is, however, that in those mounds a class of largely and rudely formed discs, or spear-heads, of flint, quartz, and manganese garnet is common. Others are chipped into regular form with minute care, but are also of unusually large size, and, like the ruder discs, suggest the idea of their purposed use, without the addition of any shaft or handle. Messrs. Davis and Squier remark, when describing the contents of the altar mounds explored by them: "Some of the altar or sacrificial mounds have the deposits within them almost entirely made up of finished arrow and spear points, intermixed with masses of the manufactured material. From one altar were taken several bushels of finely worked lance-heads of milky quartz, nearly all of which had been broken up by the action of fire. In another mound, an excavation six feet long and four broad, disclosed upwards of 600 spear-heads or discs of hornstone, rudely blocked out, and the deposit extended indefinitely on every side. The originals are about six inches long and four broad, and weigh not far from two pounds each."* The accompanying wood-cut (Fig. 11) illustrates the original text, and will suffice to show the prevailing forms of the rude implements; but it fails to suggest to the mind their great size, and clumsy, ponderous character, so nearly approaching, in both respects, to those of the European drift.

Some of the specimens are described as nearly round, but most of them are rudely heart-shaped. With them were found also several large nodules of similar material, from which portions had been chipped off. Estimating the whole amount from the number exposed within the limits to which the explorer's excavations extended, they supposed there must have been nearly four thousand altogether, and possibly a still greater number, under the single mound.

The peculiar circumstances of the deposit at Racine, as described by Dr. Hoy, where many discs were found lying on the clay with the accumulated peat formation above them, would, in some localities, suggest an antiquity measurable by the slow formation of the peat above them; but the extensive traces of an ancient population, and especially the numerous earthworks in the State of Wisconsin, suggest the possibility of the collection of stone imple-

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* Ancient Monuments of the Mississippi valley, p. 213.

ments having been buried where they were found. Of the purposed interment of those in the Ohio mounds no doubt can be entertained; and though a great antiquity has been ascribed to the mounds, in comparison with any works of the known races of the continent, no one will dream of assigning them to a period bearing any relation to that of the Drift Folk of Abbeville or Hoxne. Here, then, we find illustrations of one of the commonest types of the drift implements deposited in vast numbers under the earthworks of this remarkable prehistoric race of the New World, and found even in its regular sepulchral mounds. If one of the Racine discs in the Smithsonian collection be compared with the example from the valley of the Somme, selected by Mr. Evans to illustrate his third class of oval or almond-shaped implements,* they will be seen to correspond so closely that either might be selected as the illustration of the type.

Fig. 11.—LEWISTON FLINT IMPLEMENTS.

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* Archæologia, vol. xxxviii, pl. xv, fig 3

The Lewiston implement is more irregular and ruder in workmanship. It has been reduced to the required shape by comparatively few strokes, and appears to have been broken off at the one side by an ill-directed blow of the stone hammer by which it may be presumed to have been wrought. The opposite and only complete edge is chipped and fractured as if by frequent use. It is to be regretted that more minute information as to the precise locality and circumstances of this discovery has not been secured. But it may not yet be too late for the recovery of the desired data. As an undoubted relic of the American drift, it would afford startling evidence of a minute conformity between the most ancient traces of human art in both hemispheres. Even as, more probably, a stray relic of the ancient monuments of Wisconsin, or the Ohio valley, it possesses considerable interest to the American archæologist, thus found so far from the ascertained seats of the extinct Mound-Builders. But it is probable that the implements of the modern Indians include those of the very same form. In the same cabinet of the Smithsonian collection, which includes the Wisconsin examples referred to, is a roughly shaped disc, figured here, (Fig. 12) brought with other remains from Texas. It measures 4½ inches in length, and, as is shown by the accompanying illustration, it repeats one of the commonest types of the smaller drift implements, and also corresponds to them in its irregularly fractured edge and rough workmanship.

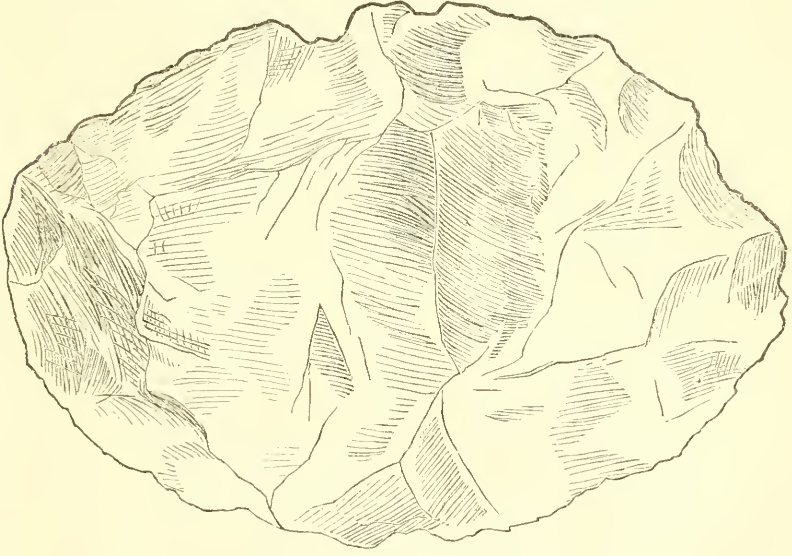

Fig. 12.—TEXAS FLINT IMPLEMENTS.

The subject selected for illustration here, from among many which I brought under the notice of my audience, though apparently trifling, has a certain significance which may justify its reproduction. A comparison of the ordinary flint and stone implements, and of the rude pottery still manufactured by the Red Indians of the American forests and prairies, with examples recovered from ancient sepulchres of Britain and the north of Europe, dating before the Christian era, proves a correspondence in many cases so striking as to admit of the one being substituted for the other without detection by the most experienced archæologist. To prove, therefore, that in the drift underneath the Gaulish and Roman graves of Abbeville and Amiens, or the British and Saxon barrows of Suffolk, lie imbedded the rude flint implements of an elder period, essentially differing from both, furnishes indications as strikingly suggestive of a different condition of life, and a diverse stage in the progress of the human race, as the bones of the mastodon or the *Ursus spelæus* which are imbedded in the same stratified gravel. That the flint-tools have certain characteristics in form and workmanship is unquestionable. Yet the difference between them and more modern implements of the same material has been exaggerated; and the results indicated by this comparison of flint implements of the New World with those of the European drift is to show, I think, that the diversity between the two is not of an essential or very important nature, and by no means such as would indicate any relative stages in a progressive development which, in the sober estimation of some of our most cautious geologists, embraces a period scarcely measurable by centuries. Their present speculations would render the interval between the Flint-worker of the British barrows of ante-Christian centuries and the modern Indian too insignificant to be taken into account, in relation to an age when man is assumed to have made his advent in Britain while it formed a part of the continent of Europe, and when the glaciers of the Scottish Grampians still contributed their Arctic floods to the valleys of southern England and France. But also some of the facts indicated here warn us that we have still to anticipate many new disclosures not less striking and unlooked for than those of the European drift; and among those is the possible discovery of America's drift-period, comprehending the traces of human art and the evidences of the presence of man in this New World, as it is called, at periods compared with which that of its Mound-Builders is modern, and even of its fancied Phœnician colonizers but of yesterday.

AN INTRODUCTORY LECTURE

TO THE

STUDY OF HIGH ANTIQUITY,

DELIVERED AT THE

ACADEMY OF LAUSANNE, SWITZERLAND, ON THE 29TH OF NOVEMBER, 1860,

BY A. MORLOT.

[Translated by the author for the Smithsonian Institution.]

> "What we know is very little, but what we do not know is immense."—LAPLACE.

[Certain subjects are more developed in this paper than they have been in the lecture itself. This is especially the case with the delta of the Tiniere. Such details, more interesting for the geologist, may be omitted by the general reader, who will find it easy to take in merely the results.]

The process of reasoning, from the known to the unknown, from what is seen to what is not seen, is practiced by every one. When the Arab of the desert descries at a distance an eagle soaring in a peculiar manner, he exclaims, "A lion!" He knows that the eagle is waiting to pounce upon the prey which a lion is about to quit.

In fact, every one is more or less in the habit of forming an opinion by indirect means. Thus, a man's character is judged of by his dress, his language, and even by his handwriting.

It is, in reality, by the same means that a lawyer arrives at his conclusions, and the savant—one ought rather to say the student, for the savant is, after all, but a perpetual student—elaborates his doctrines. He begins by observation, which he combines with experiment, when he can modify the circumstances under which the phenomena observed are produced; he then classifies, coordinates, compares his first results, in order to understand them more fully; and, finally, ascending from effects to causes, he arrives at the great principles, the laws which govern nature. Observation, combined, when feasible, with experiment, comparison, and finally, induction: this process is the method of which the result is science.

One of the most striking examples of the application of this process is furnished by geology, which has reconstructed the history of our planet before the appearance of the human race. But why should we stop at the moment when for the first time an intelligent being appeared on this earth, which had hitherto been solely peopled by animals, endowed with instinct alone? Is not man also part of nature, and does not he, too, belong to the vast plan of creation?

The objection might be raised that for the human periods we have the transmission of facts by written records, which is history proper, and by oral tradition. But, before the invention of writing, where was history? and before the development of language, where was tradition?

The origin of writing is not obscure, showing that the beginning of history does not date very far back. The origin of spoken language is far more ancient, and its study teaches us that it was developed slowly and gradually, starting from a rudimentary beginning, which necessarily corresponded to an equally rudimentary state of the human intelligence. This is sufficient to prove that oral tradition cannot go back to the origin of our species any more than the memory of an individual can revert to the moment of his birth.

Evidently, humanity has passed through an early phase which has left no remembrance of itself. How long did these forgotten times last? when did tradition begin? at what epoch did history, properly so called, take its rise? This is difficult to determine.

For southern Europe, history, ascertained chronologically, goes back several centuries before the Christian era. For that part of Europe situated to the north of the Alps, history begins with the Roman invasion, which is nearly coeval with the Christian era. We have a few historical facts and traditions of a somewhat older date, but they are not of great importance in the researches we are about to undertake, and we may pass them over in silence.

It is these pre-historical and pre-traditional times which we call HIGH ANTIQUITY, and which are to form here the object of our study. And we shall only consider Europe north of the Alps, closing our researches about the time of the Christian era. Our task is thus precisely limited, and this circumstance should not be lost sight of in the sequel.

Since the memory of the long period in question is all but lost, we must seek for other materials wherewith to supply its place. We stand here precisely in the same position as the geologist who reconstructs the history of our planet. We shall, therefore, borrow his method, since our mode of proceeding must necessarily present a strong analogy with his. The materials of the geologists are chiefly the remains of animal and vegetable creations, the fossils buried in the strata which form a great part of our continents. Instead of fossils, we have the remains of human labor and industry. They are to us as a mirror in which is reflected the image of their authors, of their life, and of their entire civilization. For the laborer is known by his work. If the geologist can restore an animal from a single bone, why should we not, with a fragment of a broken pot reconstruct the entire vase, and from the vase rise to its maker? The interval is not so very wide from a mere potsherd to man; for everything is closely linked together in the economy of human life as it is in nature. The primitive inhabitant of our country has long ago disappeared; his mortal remains have returned to dust; his tales of war are forgotten, as well as his lays of love; the very name of his tribe, of his race, is lost; but the work of his hands yet subsists and enables us to revivify our ancestors; to see how they lived and fared; to observe their domestic economy; to follow their commercial traces; to join them in their hunting parties and in their martial forays; to surprise them at some of their religious ceremonies, and to contemplate their funeral rites. Thus we transport ourselves into by-gone ages, just as the geologist has rendered himself the witness of the development of our planet. This is what we mean by the study of high antiquity or of primitive archæology.

It is evident that these researches deal only with material objects, but it is to vivify and compel them to speak, as the fossils of the geologist have been made to give forth a voice. Nature yields her answers when she is properly questioned. But we must not ask of the times when written language was yet unborn to furnish us with proper names; these are completely lost, while they play an important part in ordinary history. Our studies can only embrace the development of civilization, without considering speech. We can, in a certain measure, see our ancestors, but we cannot hear them. We must be content to gaze at them as at so many shadows.

It might be objected that to reconstruct the past by means of the remains of industry we ought to have an abundance of materials such as are but rarely found. Yet fossils were formerly considered quite as scarce, though now museums everywhere abound with them. True it is that time has rarely spared any of those productions of primitive art which rise above the surface of the soil, excepting here and there certain monuments formed of large blocks of stone and certain earthworks. This is especially the case in the countries which we are about to consider, and where the use of masonry, cemented with mortar, dates no further back than the time of the Romans. But, let us remember that numerous generations have succeeded each other, that they have strewn the ground with the remains of their industry, and have themselves descended to the dust, carrying with them into their graves many of the objects upon which they placed the highest value; we shall then understand that the vegetable mould must be rich in documents of the past, like the fossiliferous strata of the geologist, and that these documents only require to be skilfully sought for and properly interpreted. The ground that we tread is the grave of the past, a vast grave, always open, and which is to receive us also, with the remains of our industry and for the benefit of future antiquarians.*

It is also true that the preservation of antiquities is very partial, the fleshy and vegetable substances having generally disappeared, so that it is rare when anything but glass, metal, or pottery has resisted the action of time. But it is the same with the remains of the ancient organic creations; for it is chiefly only the solid parts of plants and animals which the strata of our globe contain as fossils. And yet the geologist has turned them to good account. The task of the antiquary is not more difficult.

In certain cases the preservation of antique remains is more perfect. Thus, when imbedded in peat, or in the mud at the bottom of lakes, vegetable matter, such as seeds, wood, and even remnants of woven stuffs, have been found preserved. When the substance was charred by the action of fire, before falling into the lake, it became unalterable. Thanks to this circumstance, we have discovered in Switzerland bread and even ears of corn several thousand years old.† Far from being scarce, the records of by-gone ages will become more abundant as they are more carefully sought for, and the materials for reconstructing the past of the human race will not be more difficult to obtain than those by means of which the geologist writes the history of our earth.

It might seem, from what has been said, that in forming collections of antiquities, and in studying them rationally, the outlines of the science would have been soon traced, and its fundamental principles, which are always simple, readily arrived at. It is long since the collection of antiquities was commenced, but they were considered, as were at first fossils and other objects of natural history, as mere curiosities, even when they were not turned into talismans and amulets. Again, when their meaning was sought, sterile and interminable controversies were carried on, as is always the case at the dawn of a new science, so apt is human reason to lose its way.

A prejudice which has been and is still a great hindrance to progress is the belief that everything skilfully wrought must be of Roman origin, especially objects in metal, the more ordinary remains being neglected and overlooked.

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* It would be rendering a great service to future science if the date were inscribed wherever it is possible to mark it, particularly on glass and metal, but more especially upon crockery.

† See in the Memoirs of the Society of Antiquaries, at Zürich, for 1854, 1858, 1860, 1861, and 1863, the remarkable reports by Dr. F. Keller, of Zürich, on the ancient pileworks of Switzerland. Every memoir published by the society can be had singly on applying to the bookseller.

Hence the false conclusion that before the Roman invasion the north of Europe was only inhabited by hordes of barbarians. Geology passed through a similar stage when all fossils were considered as vestiges of the deluge. The customary misconceptions prevailed also in the south of Sweden and in Denmark, countries which abound in antiquities, such as flint axes. These were thought by some to be implements used for sacrificial purposes during the time of heathenism; others even believed them to be thunderbolts, an origin which has also been attributed to the fossils called belemnites.

The prevalence of such fancies may give an idea of the state of the question when Mr. Thomsen, director of the archæological museum, at Copenhagen, and Mr. Nilsson, professor of zoology at the University of Lund, in Sweden, began their labors. These illustrious northern antiquaries, too practical to enter into the controversies then in vogue, began to compare the antiquities of their own country with the industrial productions of the more or less savage tribes of Australasia and other regions of the globe. This comparison at once brought to light a remarkable analogy between the flint instruments of the north of Europe and the implements of existing races not yet acquainted with the use of metals. MM. Thomsen and Nilsson observed at the same time that a whole series of characteristic tombs contained, besides the skeletons and some rude pottery, implements of stone only, without any trace of metal. All this suggested, very naturally, that the first inhabitants of the north of Europe had not been acquainted with the use of metals, and bore no little resemblance to the savages of the present day, at least in what concerns the habits of every-day life. Another class of tombs contained cutting implements and arms of metal, axes, knives, swords, spear-heads; not, however, of iron or steel, but of bronze, a mixture of copper and tin. Had iron been known, it would certainly have been used in preference. It follows that bronze was known and employed before iron. Nor can there be a doubt that what iron is now, and has long been, for purposes of industry and the requirements of civilization in general, bronze once was, and stone, chiefly flint, previous to bronze.

Thus was established the plain and practical distinction of the successive ages: First, that of *Stone*, next that of *Bronze*, and lastly that of *Iron*. This classification, which recalls Werner's division of the geological formations into primitive, secondary, and tertiary, was introduced about thirty years ago.* At first applied only in Scandinavia, it spread by degrees to Germany, England, and Switzerland, and is beginning to penetrate, by Piedmont, into Italy,† rendering everywhere essential service.

Attempts are now made to subdivide these three great phases in the development of civilization. Some antiquaries, such as Mr. Worsaae, think they can, from the quality of the objects and the mode of the sepulchral constructions, distinguish a first and a second sub-period in the stone age. The learned explorer of Mecklenburg, Dr. Lisch, at Schwerin, thinks that during the first centuries of the bronze age the casting in metal of pieces hollow inside was unknown, and that such pieces indicate a considerable progress, characterizing the latter times of the bronze age.‡ In Denmark and in Switzerland an

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* The northern savants did not publish their results till several years after having obtained them. Mr. Thomsen printed a paper in *Nordisk Tidsskrift for Old Kyndighed*, 1832, and a very good general treatise, *Ledetraad til Nordisk Old Kyndighed*, *Kjoebenhavn*, 1836, of which there appeared a German edition at Hamburg in 1837, and an English edition, "A Guide to Northern Antiquities, London, 1837." Professor Nilsson published a work on the primitive inhabitants of Scandinavia: *Scandinaviska nordens urinvonare*, *Lund*, 1838, 1843. This latter work is a real masterpiece, worthy of ranking with G. Cuvier's immortal publications, and a second edition in Swedish and in German is about to appear.

† See B. Gastaldi's valuable paper, *Nuovi cennisugli oggetti di alta antichità*, *trovati nelle torbiere e nelle marniere dell' Italia: Torino*, 1862. These researches have been taken up and are continued with great talent by Professor B. Strobel, assisted by L. Pigorini, both at Parma.

‡ The author, who has carefully studied the museum at Schwerin, the capital of Mecklenburg, does not think this subdivision sufficiently well established.

early pre-historical iron age is also beginning to be recognized and to be distinguished from a later iron age, belonging to the historical era. It was necessary to begin by establishing a small number of distinctly characterized periods, as had been done also in geology. But it is becoming evident, in antiquity as in geology, that there have been gradual transitions from one period to the next. Thus, though the presence of cutting implements of bronze generally excludes the simultaneous presence of iron, there are tombs, like those at Hallstatt, in the Austrian alps, which contain the bronze sword, together with the knife or the axe of iron. But in this case an attentive study will teach us that the burials belong to a time of transition from bronze to iron. At Hallstatt the transition has evidently taken place slowly and gradually. In other instances it seems almost to have been brought about violently, perhaps by foreign invasion, or by internal revolution, presenting a certain analogy with the geological convulsions which have so often established a break between immediately overlying strata.

We have seen how the foundations of our science have been laid. Some of its chief principles have been disclosed by the historical sketch, but we must consider them more closely and dwell with greater detail on our method of research.

If we seek to understand the past of our species, we must evidently begin by ascertaining its present state; we must study man, not only in civilized countries, but wherever he has settled. Hence we see that ETHNOLOGY is to be taken for our starting point, and that it contributed largely in guiding the northern antiquaries into the right path has already been already shown. Ethnology is for us what physical geography is for the geologist. We can only understand the former state of our globe by studying its present condition and by following the changes which take place on its surface, as Lyell, the reformer of geology, has so well taught.

Every nation has always had some special mode of manufacturing and of ornamenting its productions, and, moreover, its peculiar habits and customs, impressing a distinctive stamp upon its art and industry. This constitutes what is called STYLE. In Europe, north of the Alps, the style was generally pretty uniform during a given era, but it varied continually from one age to another, just as the fossil species have changed their types from one geological period to the next. The appearance of an object will thus often suffice to determine its age and the relative date of its interment, as we can determine the relative age of a geological stratum by a single fossil, when this is characteristic. In the north of Europe bronze bracelets were worn during the entire bronze age and during the early iron age; but their style is very different, the fashion having changed. Thanks to this circumstance, one is rarely embarrassed in determining the age of a bronze bracelet, or of a mere fragment of such a bracelet.

It is not enough that when making field researches we accumulate antiquities merely for the purpose of forming a collection of them. It is of the greatest interest to observe the ASSOCIATION of the objects, in order to decide which are of the same date, just as it is important to assort together the fossils found in the same stratum. Taken separately, the fossils, like isolated words, would not in themselves be of so much importance, whilst their concourse, like a logical phrase, may throw a vivid light on a whole era of the geological past. In this respect, tombs are of great value, for the series of objects which may be contained in one and the same grave harmonize, and are necessarily of the same date. Nor must we forget that the very mode of burial has varied from one age to another—a circumstance which gives still greater value to the examination of this species of monument. We have already seen how much the study of the tombs contributed to guide the northern antiquaries into the right path.

The question of the special POSITION (*gisement* in French, *laderung* in German) in which objects are found, so important in geology, is not less so when we consider the traces of the human past. The peculiar position of antiquities in the various places where they are met with has often a special signification. Thus, to return to the graves, their interior, carefully examined, will often reveal the funeral customs and may furnish us with notions respecting the religious ideas of the time. Sometimes, and it is found to be generally the most ancient mode, the body was bent up, with the knees joining the chin, as if to occupy the least possible space. Later the dead were usually burnt, which might lead us to suspect the worship of fire. Then again the body has been found stretched out horizontally. When several contemporary skeletons are discovered in the same mound, their relative positions may lead us to infer the practice of human sacrifices. In this case the victims will generally be found lying scattered about irregularly, as if they had been thrown in carelessly, while the centre of the grave has been reserved for the individual in whose honor the funeral rites and sacrifices were instituted. By observing the position of some broken pebbles and of fragments of pottery in the earth covering certain ancient tombs, Dr. Keller, of Zürich, inferred the custom of casting in these objects while raising the mound—a practice which a curious passage from Shakspeare, (Hamlet, act V, scene I,) seems to confirm.* It would appear that the funeral was occasionally combined with a feast on the spot, and that the earthenware which had been used was broken up and scattered over the grave. At other times the entire vases, or such as have been only crushed by the pressure of the earth, seem to have contained food for the departed, with whom were also frequently interred his trinkets, his arms, the emblems of his trade, sometimes his dog, his horse, and even his wife.

The question of SUPERPOSITION is connected with the preceding. It plays here as essential a part as in geology, which it furnishes with the chronological succession of the different strata, since, evidently, an overlying bed must be more recent than the one beneath it. The antiquary meets rarely with series of strata as regularly superimposed as those of the geologist. The case would be more frequent could we examine the deposits which are formed at the bottom of lakes and seas. But, then, the geologist would have taken the advance, and would himself have retraced the history of the human race, leaving but scanty gleanings for the succeeding explorers. The materials of the antiquarian are usually buried in a thin layer of vegetable mould, though even that is sometimes wanting. There are, however, on terra firma, cases of superposition of deposits containing human relics. They are of great value, for they establish more surely than could be done in any other manner the chronological succession of the different ages. In fact, every distinction between ages should invariably rest upon some direct observation of superposition. We have seen how the northern antiquaries arrived at their three ages of stone, bronze, and iron. Their results are satisfactory, but still, having been obtained somewhat indirectly, they are even yet occasionally disputed. Such facts, however, as the following are of a nature to settle the question definitively:

Graves of the early iron age, established upon sepulchral mounds of the bronze age, and, in other cases, interments of the bronze age upon the site of those belonging to the stone age, have been accidentally noticed in Denmark and in the adjoining duchy of Mecklenburg. But the most complete example of such superpositions has been observed, and carefully, too, at Waldhausen, near Lubeck. One of those ancient tombs existed there, in the shape of a mound or barrow, 13 feet high and 161 feet in circumference. It was levelled to the ground to insure a thorough examination, as serious research requires.

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* Memoirs of the Society of Antiquaries at Zurich. Vol. III, part V, 1845.

Just beneath the surface was discovered an ancient grave, to all appearance of pre-historical date. It was occupied by a skeleton, with fragments of coarse pottery and with a piece of iron, rusted away. Lower down, at about midway the depth of the mound, were three tombs of the bronze age. They consisted of small chests or cells of stone-work without mortar, and containing each a cinerary urn filled with fragments of calcined bones, mingled with various objects of bronze, such as neck-collars, hair-pins, and a knife. Finally, at the bottom of the mound was discovered a sepulchral chamber of the stone age, formed of large unhewn boulders, and containing coarse pottery and flint hatchets. Evidently the first inhabitants of the country had constructed, upon the natural soil, a tomb, according to the custom of the times, covering it over with earth. Upon this elevation some interments of the bronze age had taken place, and another covering of earth was added, thus doubling the height of the mound. Finally, in the early iron age, a corpse had been buried by digging a tomb on the summit of the hillock.*

Thus, what at first sight appears to be only one tomb, may furnish antiquities belonging to different periods, and it is of the utmost importance to carry on the researches so as carefully to determine the exact relative position of whatever presents itself, if serious mistakes are to be avoided. MM. Castan and Delacroix, at Besançon, surprised to find a mixture of objects which they thought belonged to different periods, succeeded in distinguishing, in the same mound of only slight elevation, burials of the Roman time, established over Gallic entombments of the early iron age, proving thus an indigenous civilization, based on the use of iron, and previous to the Roman invasion.†

But the incident of mere superposition, notwithstanding its value, can only furnish notions of *relative* chronology, expressed like those of geology, which knows of no absolute dates in numbers of years or of centuries. And yet we could wish to know when each of the three ages of stone, of bronze, and of iron began, and how long each lasted. The best that we can do is to acknowledge our ignorance. The introduction of iron is itself a pre-historical event; even tradition is silent about it; how much, then, must the preceding ages of the bronze and of the stone lie further back, beyond all memory! The problem can only be solved by the aid of geology, by finding out cases of some regular and constant action of the elements, connected with marks of the principal human periods. The following is an example, which will show how dates of ABSOLUTE CHRONOLOGY are to be obtained:

The alpine torrents, when they issue from the ravines or small lateral valleys, which give rise to them, accumulate their alluvium in fan-shaped deposits, or portions of cones of a very regular form. These are real deltas, but with a surface necessarily more inclined than is the case in those of rivers. The inclination of the cone depends on that of the torrent in its previous course, on the volume of the water, and on the quantity of shingle it drifts. This inclination varies with each torrent, and the limits of the variation are, on the one hand, the descent of rapid rivers; on the other, the slope of any accumulation of loose matter formed without the intervention of water, as, for example, in certain landslips. The usual inclination of these torrential deltas in the Alps ranges between 2 and 5 degrees. An inclination of 7 degrees is much less frequent, and the cases where it reaches 15 degrees are rare. If the form and nature of the hydrographical basin of a given torrent and the meteorological circumstances, such as the annual quantity of rain, do not change, it is evident that the torrent cannot alter the form nor the inclination of its cone or delta. The latter will consequently grow by concentric layers,

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* Beitraege zur nordischen Alterthums Kunde, vom Verein für Lubeckische Geschichte. I. Heft, Lubeck, 1844.

† Mémoires de la Société d'émulation du Doubs. Besançon, 1861.

preserving regularly the same inclination. In ordinary times the torrent flows along the central region of its cone; there also it drops its largest boulders in times of inundation, distributing the gravel and less coarse shingle over the sides of the cone, for the relative volume of the drifted stones must diminish with the force of propulsion of the water from the central region of the cone towards its two edges. It is clear that a torrent, left to itself, cannot raise the surface of its cone unevenly and create hollows and prominences. For if the surface were raised on any one spot, the water would immediately flow round and fill up the less elevated parts with drift. The action of water is essentially levelling. The great number of torrential cones, or deltas, which the author has had the opportunity, during the last 15 years, of examining in Switzerland and in the Austrian Alps, have always showed a regular surface. There may be slight irregularities in the action of a torrent from one year to another, but proceeding chiefly from meteorological variations, they become insensible, when the cone is considered in its totality; even at a given spot they will rapidly become levelled and effaced by the continued action of the torrent itself. We must also consider that the alluvium of a torrent is furnished by a slow degradation of its hydrographical basin, which can only yield the drift matter gradually, a circumstance necessarily contributing to regulate the growth of the cone. Thus, when in July, 1848, the torrents in Corinthia (eastern Alps) were swollen and brought down a disastrous quantity of drift, the author heard the country people ascribe the damage, in good part, to the circumstance that the upper region of the water-courses had been more than usually encumbered by loose matter.

The torrent of the TINIERE, where it flows into the lake of Geneva, at Villeneuve, (Switzerland,) forms a cone or delta, such as we have just described. This cone has an inclination of 4 degrees, and its opening, or the angle at the top of the delta, measures about 700 degrees; its radius, taken as a minimum, being 900 Swiss feet, (one Swiss foot equal to 0.3 metres, is divided into 10 inches.)

Modern embankments, in the shape of solid walls, having forced the torrent somewhat towards its right bank, on the northern side of the cone, the alluvium has since been accumulating more on that side, and has raised the surface here, whilst the southernmost part of the cone ceased to increase. Documents preserved in the parish archives of Villeneuve mention these embankments as having been built in the year 1710, and their recent origin is confirmed by the scanty covering of vegetable mould on that part of the cone which was protected by them. Here, where the ground had not been cultivated, there was only from 2 to 3 inches (6 to 9 centimetres) of earth, inclusive of the space occupied by the roots of the grass. The railroad has cut transversely through this cone perpendicularly to its axis, the cutting measuring 1,000 feet in length, and reaching in its central part, where the cone is highest, to 32½ feet above the definitive level of the rails. The section thus obtained (see page 316) may be represented by the segment of a circle, rising to 32½ feet above its chord of 1,000 feet. Happily for science, the works for the railway have been carried on very slowly at this spot; they were begun in 1856, and are not entirely finished now, (March, 1863.) The author followed them attentively from the beginning.

The cone's interior structure, brought to light by this beautiful artificial section, was found to be most regular. In the central region the rounded boulders attained a diameter of 3 feet, as in the actual bed of the torrent. From thence the drifted matter gradually diminished in size along the two halves of the cone towards the two extremities of the cutting. There was an exception for the alluvium formed since the embankments of 1710, for here the drift was naturally coarser than in the underlying part. The waters of a torrent are not apt to produce a marked stratification, of which but slight traces were to be seen, and these beyond the central region in the two sides. But where stratification became apparent, it was perfectly parallel with the present surface of the cone.

All these circumstances go to establish in a highly satisfactory manner the regularity in the formation and growth of the cone. Now, as the hydrographical basin of the Tiniere, surveyed throughout by the author, is regular, and shows no traces of landslips or of other accidents, which might have disturbed the regular working of the torrent, and as the meteorology of the country does not appear to have undergone any alterations of note in modern times, we may admit that the rate of formation and of growth of the cone in question has been proportionate to the volume of its alluvium. The partial clearing of the forests in the hydrographical basin of the Tiniere may have contributed in some slight degree to accelerate the superficial degradation of the latter. But if this effect had been marked, which is doubtful, it would tend to carry higher the dates we shall proceed to deduce, and not to bring them lower down.

In the southern flank of the cone, where it was protected, as we have seen by the embankments of 1710, three beds or layers of ancient mould were discovered, situated at different depths, which had, each in its time, formed the surface of the cone. These three layers were regularly interstratified in the gravel exactly parallel with each other and with the modern surface of the cone, which was itself most regular and inclined by 4 degrees along the line of the steepest dip.

The first of these beds of vegetable mould was found from actual observation to extend, in the southern part of the cone, over a surface of more than 15,000 square feet. It was from 4 to 6 inches thick and was situated at a depth of 4 feet beneath the present surface of the ground; (more exactly at 0.14 metre, measured down to the bottom of the bed.) It belonged to the Roman era, as it was found to contain angular fragments of Roman tiles and a Roman coin in bad condition, but of too good a type to be of the lower empire. The Romans invaded the country after the battle of Bibracte, 58 years before Christ. Allowing them a century to settle in Helvetia and to raise buildings covered with tiles, this Roman bed would date, at the most, 18 centuries back. In the year 563 after Christ, the tremendous landslip of Tauredunum ravaged the neighborhood; by that time the Roman dominion had passed away and had made room, about a century before, for the reign of the Burgundians, who do not appear to have practiced masonry or the manufacture of tiles. The Roman bed must consequently be at least 13 centuries old.

The second bed of ancient mould was followed up, on the southern side of the cone, over a space of about 25,000 square feet. It was about 6 inches thick, and stood 10 feet (more exactly at 2.97 metres, measured down to the bottom of the bed) below the present surface of the ground. It contained a few fragments of pottery, made of clay mixed up with grains of sand, and unvarnished; also a pair of tweezers, (for plucking out the hair,) cast in bronze, and of the characteristic style of the bronze-age.

The third and lowest of these beds of mould was uncovered, on the southern side of the cone, over a space of about 3,500 square feet. It was from 6 to 7 inches thick, and was met with at a depth of 19 feet (or, to be exact, 5.69 metres) below the present surface of the cone. It yielded at one point on the north side of the cone a human skeleton, the skull of which was very round and small, and remarkably thick, showing a strongly-marked Mongolian (turanian or brachycephalic) type, according to the measurements and examination instituted on the spot by T. M. G. Montagu. The same bed yielded at another point, on the southern side of the cone, numerous fragments of very coarse pottery, charcoal, and broken bones of animals, evidently kitchen refuse. The bones have been examined by Professor Rutimeyer, at Bale, author of a remarkable work on the animal remains of the antique pile works or lake dwellings in Switzerland.* Allowing that the bones in question are too few and insufficient to admit of very satisfactory conclusions, the learned professor makes out the ox, goat, sheep, pig, and dog, all domestic, and with characters which seem to point to the end of the stone age, or to the beginning of the bronze age. Weighing all the different circumstances, and avoiding undue precision, we may consider this third bed as belonging to the stone age, although the author, who explored it diligently, has not had the good fortune to discover in it any stone hatchet or other antiquity of that sort. At one spot, on the southern side of the cone, some charcoal was found in a bed of gravel one foot lower than the above mentioned layer of the stone age; consequently, at 20 feet (or, to be exact, at 6.09 metres) below the actual surface of the cone. It is worthy of note, as the art of baking bricks and tiles is generally allowed to have been introduced into the country by the Romans, that below the line of the Roman period the author never discovered the slightest trace of bricks or tiles.

Towards the central region of the cone, where the cutting was deepest, the three beds in question disappeared; naturally enough, as it was here that the torrent's action was most violent, easily washing away any mould which might have formed on the surface. As the torrent, in deviating to the right and left of its central current, lost some of its power and drifted less coarse matter, it would be more apt to overlay with new deposits, without abrading a layer of mould or earth formed since the preceding inundations. Quite in accordance with this there was found in the gravel on the southern side of the cone, at a spot where the mould bed of the bronze age had quite disappeared, but still 10 feet below the present surface, a hatchet-knife of bronze, considerably oxydized, and a well-preserved bronze hatchet which had evidently not been worn by the movement of the water. The specific weight of these two objects must have kept them in place, while the earth which surrounded them was probably swept away by the torrent. Though the three beds of ancient mould disappeared thus in the centre of the cone, they reappeared symmetrically on the other or northern side. But here they stood at a greater depth beneath the present surface, because the torrent, as we have seen, accumulated its alluvium on this side. Yet here, also, the beds were parallel to each other, and the vertical distances which separated them were the same as on the other side of the axis, in the southern part of the cone. There was, in the northern part of the cutting, a depth of 6 feet from the Roman bed to the base of the bronze bed, and 10 feet from this last to the bed of the stone age. It was impossible to mistake these three beds, or to confound them one with another. The stone-age bed was too little interrupted in the central region to allow the observer to deviate from its proper line of prolongation. The bronze-age bed was interrupted to a greater extent, but on both sides of the cone it was equally characterized by being formed of clayey earth of a bluish color, somewhat similar to the blue glacial mud, and bordered towards its upper and lower limit by more sandy zones, colored yellow by hydroxyde of iron. This was remarkable, and evidently indicated some peculiar cause. The stone-age bed sometimes bore a similar aspect, but only occasionally and not so regularly as the bronze bed. The Roman formation on the northern side was only recognized by its height above the bronze-age bed; no fragments of Roman tiles were found here, but it was only laid open over a short space, about 40 feet along the railway line, whilst the bronze-age bed was regularly and distinctly followed on the northern side, over a distance of 200 feet.†

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* *Rutimeyer, Die Fauna der Pfahlbauten der Schweiz. Basel*, 1861.

† The intersection of the bronze-age stratum with the masonry of the bridge, which conveys the torrent over the railway, has been marked on the eastern side, opposite the lake, by a thick line of reddish paint. It is easily discovered in passing in the railway train, being at the height of the windows of the passenger carriages.

Now, considering the measurements and researches conducted on and in the southern side of the cone or delta, making due allowance for the effect of the embankments, but, to remain on the safe side of the question, doubling their age—that is, supposing them to be three centuries old; taking into account the thickness of the vegetable mould on the present surface; observing that the volume of the cone increases in the ratio of the cube of its radius; ascribing to the Roman formation an age of at least 13, and at most 18 centuries; and remembering that the cone must have taken time for growing, in proportion to the volume of its alluvium, we find, by calculation, for the bronze-age bed of mould a date of from 29 to 42 centuries; for the stone-age bed a date of from 47 to 70 centuries; and for the entire cone an age of from 70 to 110 centuries. The author thinks that it would be sufficiently exact, though still within the limit, to deduct but two centuries for the action of the embankments, and to allow for the Roman bed an age of 16 centuries. This would give for the bronze-age bed a date of 38 centuries (2,000 years before Christ,) for the stone-age bed a date of 64 centuries, and for the entire cone, corresponding with the modern geological era, an age of about 100 centuries, which latter number must appear a minimum to the geologist. But, not to reckon too strictly in counting by centuries, let us say that *the bronze-age bed is from three to four thousand and the stone-age bed from five to seven thousand years old.*

It is clear that each of these ancient beds of mould cannot represent the total duration of each corresponding age, but merely a fraction of each of these ages, a period more or less protracted, during which the torrent has worked along the central region of its cone without overflowing its sides, on which vegetation could then take root. The surface of the cone must usually have been formed of bare shingle, upon which only a few shrubs could thrive. This explains why no traces of human habitation were noted in the gravel interstratified between the three beds of ancient mould. The clayey nature of the latter seems to indicate that they probably owed their origin to inundations of an exceptional character, forming loamy rather than gravelly deposits, thus favoring the development of vegetation and attracting man to the spot. It might possibly be objected that, these three beds having been deposited by the torrent, the ancient remains which they contain might also have been swept down by the water from some other locality, in which case the age of these formations would remain undetermined. But the ancient remains were well preserved and evidently not worn by translation; the fragments of pottery and of bricks were angular, as were also the small pieces of charcoal scattered through each of the beds, all three of which contained, moreover, entire shells of various species of snails. The objection raised falls, therefore, to the ground.

Let us here notice that the minimum date of 29 centuries for the bronze-age bed agrees very well with the purely archæological deductions, which carry back the introduction of iron into our countries to at least one thousand years before the Christian era.* This correspondence is the more complete, as the style of the tweezers found in the bronze-age bed indicates the end rather than the beginning of that age. If that minimum of 29 centuries for the date of the bronze bed be correct, the minima dates of 47 centuries for the stone-age bed, and of 74 centuries for the age of the entire cone, must also be correct,

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* See the chapter on the chronological question in the *Etudes géologico-archéologiques en Danemark et en Suisse par A. Morlot. Bulletin de la Société vaudoise des sciences naturelles, Tome VI, No. 46—Lausanne*, 1860; reproduced in English in the "Smithsonian Report for 1860: Washington, 1861." Greek coins of the oldest type are met with on the shores of the Baltic, as far as the island of Oesel. Throughout Germany, as far as the Danish archipelago, are found certain antique objects, indicating commercial intercourse between the north and the south of Europe long before the Christian era. This implies for that period the knowledge of iron in the north of Europe.

while the maxima obtained may in reality be underrated. Thus the maximum of 110 centuries for the age of the entire cone is evidently rather below than above the mark. Still it appears from the latter number that the modern geological period to which the cone or delta of the Tiniere corresponds has not been very long, and that, soon after its beginning, man inhabited Europe. This is confirmed by the study of the peat-bogs in Denmark and in Switzerland. The rude flint hatchets found in England and in France in drift gravel, associated with the bones of the elephant (*elephas primigenius*) and of other extinct species, would seem to carry back the appearance of man in Europe to an epoch even preceding what is usually considered as the modern geological period.[*]

Let us further notice, that if the stone-age bed really belongs to the beginning of the bronze age, it follows that the latter has lasted from two to three thousand years, since the tweezers found in the bronze-age bed, at a depth of ten feet, point, as we have seen, belonged to the end of the age. Hitherto we had been left without the faintest notion as to the real length of the bronze age; it was only evident, from the quantity and the quality of its remains, that it must have been of long duration.

We have thus attempted to substantiate for *high antiquity* the first data in a system of absolute chronology, expressed by thousands of years. The opportunity has been singularly favorable, it is true, but it has the defect of being the first and only case of its kind. Let us hope that others will not be long in presenting themselves, and that they will be turned to good account; for as long as a fact remains isolated, the inferences drawn from it cannot be verified by comparison, and the mind cannot rest fully satisfied.[†]

But what avail these researches into the past, when the present more than suffices to engross our attention? The question is legitimate, and it is right that we should close with some reference to the END AND PRACTICAL USE of our study.

When the philosophers of ancient Greece exercised their ingenuity in investigating the properties of the conic sections, they little thought they were laying the foundation of the modern methods by which are calculated the astronomical tables that guide the mariner's course. No one asks at present what is the use of mathematics.

Less than a century ago geologists would have been rather puzzled to demonstrate the practical utility of their labors. Now, it is easy to furnish the most satisfactory exemplifications of geology to industry.

Every step in the acquisition of real knowledge, the least secret of nature duly unravelled, has its intrinsic value, and will sooner or later contribute towards the well-being of our race. But science requires time for clearing its ground, for sowing its seeds, and for bringing in its harvests. Archæology is even younger than its sister geology, and we must not be surprised if it has not as yet matured its fruits. The following, however, may be accepted as a word of apology in its behalf:

Nature forms one harmonious whole, of which the compound elements have

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[*] T. Prestwich, on the occurrence of flint implements, &c.: Philosophical Transactions, Part II, 1860.

[†] This first attempt has already met with a singular corroboration through researches of Mr. Gillieron at Neuveville, on the lake of Bienne, Switzerland. This shrewd and careful observer makes out for a pile-work or lake dwelling of the stone age at Pont-de-Thielle, in his neighborhood, an antiquity of 67½ centuries. See *Actes de la société jurasienne d'émulation. Année*, 1860. A calculation of the age of some pile-works in the peat at Les Uttins, near Yverdon, Switzerland, has been proved to be a failure. See the paper by Mr. Tayet in the *Bulletin de la société vaudoise des sciences naturelles*. 16 *Avril*, 1862. These chronological researches in Switzerland are dwelt upon by Sir Charles Lyell in his new work, "The geological evidences of the antiquity of man. London, 1863."

the most intimate reciprocal relations. Consequently, a knowledge of the present throws light upon the past, and reciprocally the past renders the present more clear. We know the observations upon the changes which are now taking place on the surface of the globe are necessary to enable us to comprehend the geological results of past times to explain the present condition of the continents. The naturalist who would have a satisfactory idea of an organized being, even after dissecting it, must study its development from the first germ, and this germ itself cannot be fully comprehended without a knowledge of the entire being. And as regards man, could he account for his present condition without reviving the recollections of his youth, or could he understand his infancy had he not ripened into manhood?

Thus, if the knowledge of the present state of our race is necessary for reconstructing its past ages, the study of antiquity is also indispensable for the proper comprehension of the present, and for arriving at a real understanding of the social relations which constitute the life of nations. It will therefore be an immense gain when the progress of scientific research into the development of our race shall substitute positive notions, rich in practical application, for those vain and empty political discussions which, orginating in ignorance, end in error.

Finally, if the astronomer has succeeded in foretelling the movements of the celestial bodies, because he has detected the laws which govern them, may we not hope, with Condorcet,* that when the present shall be well understood as a necessary result of the past, we may be enabled in some degree to penetrate the mysterious future? This would be one of the most glorious, and certainly also one of the most fertile, triumphs of human intelligence.

Let us, then, study the past, in order that we may understand the present, and perhaps catch a glimpse of the future.

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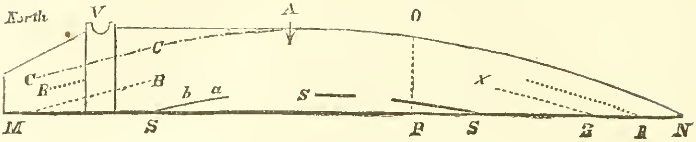
* *Condorcet. Esquisse d'un Tableau Historique du Progrés de L'esprit Humain: Paris*, 1798: *page* 332.

SECTION OF THE CONE OR DELTA OF THE TINIERE.



R. *Roman stratum,* at a depth of 4 feet, on the southern side.

B. *Bronze-age stratum,* 10 feet deep, from 3 to 4 thousand years old. +, spot where a hatchet-knife and a hatchet, both of bronze, were found.

S. *Stone-age stratum,* 19 feet deep, from 5 to 7 thousand years old. In it have been found: at *a,* a bit of pottery; at *b,* a human skeleton with a very small, round, and thick skull of the Mongolian or turanian type, (brachycephalic;) at *d,* numerous fragments of very rude pottery, much charcoal, and a number of broken bones of quadrupeds.

A. *Central axis of the cone,* which cone is cut transversely by the railway. It was here that the torrent flowed in ordinary times, before it had been driven more northwardly by dams.

C C. *Surface of the cone,* when the dams were raised. This line is, to a certain degree, ideal; all the others are such as have been ascertained by direct observation.

M N. *The railway line.*

V. *Bridge,* serving also as an aqueduct, to carry the torrent over and across the railway.

O P N. Region to which refer exclusively the measurements that have figured in the chronological computation. These measurements, often repeated, were susceptible of being taken here with considerable accuracy; they may be considered as within half an inch of the truth.

The section has been interrupted in M, because there it becomes indistinct. Its southern end was perfect in every respect.

Some persons will perhaps infer from the regularity of the cone of the Tiniere, that its rate of growth was irregular. The same persons would doubtless, had the structure of the cone been irregular, have come to the conclusion that the rate of growth had been regular! Others will probably practice the by no means unusual method of supposing imaginary circumstances, by means of which to invalidate the direct inductions from positive facts. This would be *verba, non res,* instead of *res, non verba.* Thus Professor A. Wagner, at Munich, of course without examining the spot, found that the 4 feet of gravel, which cover regularly the Roman stratum, might as well have been deposited in 10 or 15 minutes, instead of as many centuries.* It would have been just as ingenious to say that a human being may grow up to manhood in 30 minutes instead of 30 years!

At all events, the author will be delighted to abandon his results as soon as he is shown something better.

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* *Sitzungsberichte der Math: Phys: Classe der Akademie in München.* 8 *Juni,* 1861, *Heft. II.*

PROGRAMME OF THE COURSE OF LECTURES BY M. MORLOT AT THE ACADEMY OF LAUSANNE.

Introductory Lecture.

What is meant by the study of high antiquity.—History of the science.—MMrs. Thomson and Nilsson.—Their three ages: of the stone, of the bronze, of the iron.—The method to be followed: Ethnography; the style; association; superposition.—Chronology; the delta of the Tinière.—End and practical use of the science.

Lecture II.

The stone age in the North.—The peat bogs in Denmark show three periods of the forest: the Scotch fir, the oak, the beech.—Antiquities of the peat bogs. Kjœkkenmœdding (kitchen refuse); Plants; animals; the traces of human industry in the Kjœkkenmœdding.—How flint was turned to use.

Lecture III.

The stone age in Switzerland.—Pile-works or lake dwellings: their discovery; their situation; their structure.—Instruments: arms; pottery.—Plants: flax and flaxen stuffs; corn and bread.—Wild and domestic animals.

Lecture IV.

The bronze age.—The art of mining and smelting: copper and tin.—The copper age in North America.—Bronze worked in the north of Europe.—Chemical examination of the antique bronze.—Gold known and worked.—The art of casting metal.—Ornaments on the cast bronze, geometrical style.—Instruments; arms; trinkets.—In Switzerland, pile-works of the bronze age.

Lecture V.

The early iron age.—Meteoric iron: the primitive mode of working iron.—Transition period from bronze to iron: Hallstatt.—Silver, glass, and enamel; glazed pottery.—Coins.—The alphabet.—*The North.*—*Switzerland*: The Tiesenan-find; the Helvetians; arms; chariots; roads; pile-works; coins and inscriptions; ornaments.—The geometrical style, combined with rude animal and human figures.—Monuments in Switzerland.

Lecture VI.

General review.—History of civilization; different branches of that study.—The origin of man.—Flint instruments in diluvial gravel, with extinct species.—The art of producing fire; its consequences.—*The stone age*: comparison with the present savages; analogies and differences; tombs and sacred stones; human race of the period.—*The bronze age*: a new civilization; arts and commerce; tombs; religion; races; the primitive inhabitants dispossessed.—*The early iron age*: tombs; human sacrifices; religion; human races; domestic animals; coining; writing.—The dawn of history; the beginning of science.

Moral of the Lectures.—The study of antiquity reveals the slow but steady progress of man, as the study of geology reveals the gradual development of our planet.

NORTH AMERICAN ARCHÆOLOGY.

PUBLISHED BY THE SMITHSONIAN INSTITUTION.

[From the "Natural History Review," London.

BY JOHN LUBBOCK, F.R.S., L. S., AND G. S.

1. Ancient Monuments of the Mississippi Valley, comprising the Results of extensive Original Surveys and Explorations. By E. G. Squier, A.M., and E. H. Davis, M.D.
2. Aboriginal Monuments of the State of New York, comprising the Results of Original Surveys and Explorations; with an illustrative Appendix. By E. G. Squier, A.M.
3. The Antiquities of Wisconsin, as surveyed and described by I. A. Lapham.
4. The Archæology of the United States; or Sketches, Historical and Bibliographical, of the Progress of Information and Opinion respecting Vestiges of Antiquity in the United States. By Samuel F. Haven.

The four works which we have placed at the head of this article form a part of the long series of scientific researches which have been published under the auspices of the Smithsonian Institution. There are several other memoirs which we ought, perhaps, to have added to our list, and especially one by Mr. Caleb Atwater, who, according to Messrs. Squier and Davis, "deserves the credit of being the pioneer in this department." His researches form the first volume of the Archæologia Americana, which was published in 1819, and contains plans and descriptions of many ancient works.

The memoir by Messrs. Squier and Davis, occupying more than three hundred pages, is chiefly descriptive of ancient fortifications, enclosures, temples, and mounds, and of the different implements, ornaments, &c., which have been obtained from them. It is embellished with forty-eight plates, and no less than two hundred and seven woodcuts.

In his second work, Mr. Squier confines himself to the antiquities of the State of New York. Within these limits, however, he describes many ancient monuments of various kinds, and he feels "warranted in estimating the number which originally existed in the State at from two hundred to two hundred and fifty." He comes to the conclusion, "little anticipated," he says, "when I started upon my trip of exploration, that the earthworks of western New York were erected by the Iroquois, or their western neighbors, and do not possess an antiquity going very far back of the discovery." *(sic.)*

The systematic exploration of the ancient remains in Wisconsin, of which the memoir by Mr. Lapham is the result, was undertaken by him on behalf of the American Antiquarian Society, from whose funds the necessary expenses were provided. The cost of the publishing, however, which, from the great number of engravings, (fifty-five plates, besides sixty-one wood engravings,) was considerable, was defrayed by the Smithsonian Institution, and the work is included in the seventh volume of "Contributions." As our account of the "Animal Mounds" will be almost entirely derived from the data furnished by Mr. Lapham, we will for the moment say no more on the subject.

Mr. Haven's work is well described in the title, and forms an interesting introduction to the study of North American Archæology. He gives us comparatively few observations or opinions of his own; but after a careful examination of what others have written, he comes to the conclusion that the ancient earthworks of the United States "differ less in kind than in degree from other remains concerning which history has not been entirely silent. They are more numerous, more concentrated, and in some particulars on a larger scale of labor, than the works which approach them on their several borders, and with whose various characters they are blended. Their numbers may be the result of frequent changes of residence by a comparatively limited population, in accordance with a superstitious trait of the Indian nature, leading to the abandonment of places where any great calamity has been suffered; but they appear rather to indicate a country thickly inhabited for a period long enough to admit of the progressive enlargement and extension of its movements."

The last work on our list is of a very different nature. It is more general and more ambitious. At the same time, it scarcely fulfils the promise of its title; for though some portions are sufficiently general, by far the larger part is purely North American. It will form the subject of a separate notice in this Review.

The antiquities themselves fall into two great divisions: Implements (including ornaments) and Earthworks. The Earthworks have been divided by the American Archæologists into seven classes: 1. Defensive enclosures; 2. Sacred and Miscellaneous enclosures; 3. Sepulchral mounds; 4. Sacrificial mounds; 5. Temple mounds; 6. "Animal" mounds; and 7. Miscellaneous mounds. These classes we shall treat separately, and we can then better consider the "mound-builders" themselves.

IMPLEMENTS.

The simple weapons of bone and stone which are found in America closely resemble those which occur in other countries. The flakes, hatchets, axes, arrow-heads, and bone implements are, for instance, very similar to those which occur in the Swiss lakes, if only we make allowance for the differences of material. In addition to the simple forms, which may almost be said to be ubiquitous, there are some, however, which are more complicated. In many cases they are perforated, as, for instance, those figured by Messrs. Squier and Davis (l. c., p. 218.) These perforated axes are generally considered in Europe to belong to the metallic age, as also was probably the case in the New World.

At the time of the discovery of America iron was absolutely unknown to the natives, with the exception, perhaps, of a tribe near the mouth of the La Plata, who had arrows tipped with this metal, which they are supposed to have obtained from masses of native iron. The powerful nations of Central America were, however, in the age of bronze, while the North Americans were in a condition of which we find in Europe but scanty traces—namely, in the age of copper. Silver is the only other metal which has been found in the ancient tumuli, and that but in very small quantities. It occurs sparingly in a native form with the copper of Lake Superior, whence, in all probability, it was derived. It does not appear to have been ever smelted. From the large quantity of galena which is found in the mounds, Messrs. Squier and Davis are disposed to think that lead must have been used to a certain extent by the North American tribes; the metal itself, however, has not, I believe, yet been found.

Copper, on the other hand, occurs frequently in the tumuli, both wrought and unwrought. The axes have a striking resemblance to the simple axes of Europe, which contain the minimum quantity of tin; and some of the Mexican paintings give us interesting evidence as to the manner in which they were handled and used. These, however, were of bronze, and had therefore been fused; but the Indian axes, which are of pure copper, appear in all cases to have been worked in a cold state, which is the more remarkable, because, as Messrs. Squier and Davis have well observed, "the fires upon the altar were sufficiently intense to melt down the copper implements and ornaments deposited upon them. The hint thus afforded does not seem to have been seized upon." *

This is less surprising than it at first appears, if we remember that round Lake Superior, and in some other still more northern localities, copper is found native in large quantities, and the Indians had therefore nothing to do but to break off pieces and hammer them into the required shape. Hearne's celebrated journey to the mouth of the Coppermine river was undertaken in order to examine the locality whence the natives of that district obtained the metal. In this case it occurred in lumps actually on the surface, and the Indians seemed to have picked up what they could, without attempting anything that could be called mining. Around Lake Superior, however, the case is very different. A short account of the ancient copper-mines is given by Messrs. Squier and Davis in the work already so often cited, by Mr. Squier in "The Aboriginal Monuments of the State of New York," and by Mr. Lapham,† while the same subject is treated at considerable length by Prof. Wilson. The works appear to have been first discovered in 1847, by the agent of the Minnesota Mining Company.

"Following up the indications of a continuous depression in the soil, he came at length to a cavern where he found several porcupines had fixed their quarters for hybernation; but detecting evidences of artificial excavation, he proceeded to clear out the accumulated soil, and not only exposed to view a vein of copper, but found in the rubbish numerous stone mauls and hammers of the ancient workmen. Subsequent observations brought to light ancient excavations of great extent, frequently from twenty-five to thirty feet deep, and scattered over an area of several miles. The rubbish taken from these is piled up in mounds alongside; while the trenches have been gradually refilled with the soil and decaying vegetable matter gathered through the long centuries since their desertion; and over all, the giants of the forest have grown, and withered, and fallen to decay. Mr. Knapp, the agent of the Minnesota Mining Company, counted 395 annular rings in a hemlock tree, which grew on one of the mounds of earth thrown out of an ancient mine. Mr. Foster also notes the great size and age of a pine stump, which must have grown, flourished, and died since the works were deserted; and Mr. C. Whittlesey not only refers to living trees now flourishing in the gathered soil of the abandoned trenches, upwards of three hundred years old, but he adds, 'On the same spot there are the decayed trunks of a preceding generation or generations of trees, that have arrived at maturity, and fallen down from old age.' According to the same writer, in a communication made to the American Association, at the Montreal meeting in 1857, these ancient works extend over a track from 100 to 150 miles in length, along the southern shore of the lake."

In another excavation was found a detached mass of native copper, weighing upwards of six tons. It rested in an artificial cradle of black oak, partly preserved by immersion in water. Various implements and tools of the same metal were found with it. The commonest of these are the stone mauls or hammers, of which from one place ten cart-loads were obtained. With these were "stone axes of large size, made of greenstone, and shaped to receive the withe-handles." "Some large round greenstone masses, that had apparently been used for sledges, were also found. They had round holes bored in them to a depth of several inches, which seemed to have been designed for wooden plugs, to which withe-handles might be attached, so that several men could swing them with sufficient force to break the rock and the projecting masses of copper. Some of them were broken, and some of the projecting ends of rock exhibited marks of having been battered in the manner here suggested." ‡

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* One "cast" copper axe is, however, recorded as having been found in the State of New York, but there is no evidence to show by whom it was made.

† Loc. cit., p. 74.

‡ Prof. W. W. Mather, in a letter to Mr. Squier, l. c., p. 184.

Wooden implements are so perishable that we could not expect many of them to have been found. Two or three wooden bowls, a trough, and some shovels with long handles, are all that appear to be recorded.

It has often been stated that the Indians possessed some method, at present unknown, by which they were enabled to harden the copper. This, however, from examinations instituted by Prof. Wilson, seems to be in error. Some copper implements, which he submitted to Prof. Crofts, were found to be no harder than the native copper from Lake Superior. "The structure of the metal was also highly laminated, as if the instrument had been brought to its present shape by hammering out a solid mass of copper."

POTTERY.

Before the introduction of metallic vessels, the art of the potter was much more important even than it is at present. Accordingly, the sites of all ancient habitations are marked by numerous fragments of pottery lying about; this is as true of the ancient Indian settlements as of the Celtic towns of England, or the Lake villages of Switzerland. These fragments, however, would generally be those of rude household vessels, and it is principally from the tumuli that we obtain those better-made urns and cups from which the state of the art may fairly be inferred. Yet I know of no British sepulchral urn, belonging to the stone age, which has upon it a curved line. It is unnecessary to add that representations of animals or plants are entirely wanting. They are also absent from all articles belonging to the bronze age in Switzerland, and I might almost say in Western Europe generally, while ornaments of curved and spiral lines are eminently characteristic of this period. The ornamental ideas of the stone age, on the other hand, are confined, so far as we know, to compositions of straighter lines, and the idea of a curve does not seem to have occurred to them. The most elegant ornaments on their vases are impressions of the finger-nail, or of a cord wound round the soft clay.

Very different was the condition of American art. Dr. Wilson has well pointed out that, as regards Europe, "in no single case is any attempt made to imitate leaf or flower, bird, beast, or any simple natural object; and when, in the bronze work of the later iron period, imitative forms at length appear, they are chiefly the snake and dragon shapes and patterns, borrowed seemingly by Celtic and Teutonic wanderers, with the wild fancies of their mythology, from the far eastern cradle land of their birth." This rule, however general, is not quite without exceptions; witness the bronze knife, fig. 166, in the catalogue of the Copenhagen museum. This interesting specimen has for a handle the figure of a man, which, however, is but a poor specimen of art. Moreover, some doubt may possibly be entertained about the age of this knife; the tip is broken off, but the blade, as far as it goes, is quite straight in the back, a form which, though general in the iron age, is seldom, if ever, found in knives of the bronze age, in which the back part is always more or less curved.*

But I must not suffer myself to be led into a digression on ancient art, especially as M. Morlot has been specially devoting himself to this study, and, in his forthcoming work on the Antiquities of Mecklenbourg, will, I hope, throw much light on the subject.

"Among the North American mound-builders the art of pottery attained to a considerable degree of perfection" Some vases, indeed, are said to rival, "in elegance of model, delicacy, and finish," the best Peruvian specimens. The material used is a fine clay; in the more delicate specimens, pure; in the coarser ones, mixed with pounded quartz. The art of glazing and the use of the pot-

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* I except, of course, the small razor-knives, which (Copenhagen Catalogue, Nos. 171 to 175) have a totally different form. These, moreover, from the character of their ornamentation, belong probably to the close of the bronze age, if not to that of iron.

ter's wheel appear not to have been known, though that "simple approximation to a potter's wheel may have existed," which consists of "a stick of wood grasped in the hand by the middle, and turned round inside a wall of clay formed by the other hand or by another workman."*

Among the most characteristic specimens of ancient American pottery are the pipes. Some of these are simple bowls, smaller indeed, but otherwise not unlike a common every-day pipe, from which they differ, however, in having generally no stem, the mouth having apparently been applied direct to the bowl. Others are highly ornamented, and many are spirited representations of monsters or of animals, such as the beaver, otter, wild cat, elk, bear, wolf, panther, raccoon, opossum, squirrel, manatee, eagle, hawk, heron, owl, buzzard, raven, swallow, parroquet, duck, grouse, and many others. The most interesting of these, perhaps, is the manatee or lamantin, of which seven representations have been found in the mounds of Ohio. These are no mere rude sculptures, about which there might easily be a mistake, but "the truncated head, thick semicircular snout, peculiar nostrils, tumid, furrowed upper lip, singular feet or fins, and remarkable moustaches, are all distinctly marked, and render the recognition of the animal complete." † This curious animal is not at present found further north than the shores of Florida, a thousand miles away.

ORNAMENTS.

The ornaments which have been found in the mounds consist of beads, shells, necklaces, pendants, plates of mica, bracelets, gorgets, &c. The number of beads is sometimes quite surprising. Thus the celebrated Grave Creek mound contained between three and four thousand shell-beads, besides about two hundred and fifty ornaments of mica, several bracelets of copper, and various articles carved in stone. The beads are generally made of shell, but are sometimes cut out of bone or teeth; in form they are generally round or oblong; sometimes the shell of the Unio is cut and strung so as to "exhibit the convex surface and pearly nacre of the shell." The necklaces are often made of beads or shells, but sometimes of teeth. The ornaments of mica are thin plates of various forms, each of which has a small hole. The bracelets are of copper, and generally encircle the arms of the skeletons, besides being frequent on the "altars." They are simple rings, "hammered out with more or less skill, and so bent that the ends approach, or lap over, each other." The so-called "gorgets" are thin plates of copper, always with two holes, and probably, therefore, worn as badges of authority.

EARTHWORKS.

Defensive enclosures.—The works belonging to the first class "usually occupy strong natural positions," and as a fair specimen of them we may take the Bourneville enclosure in Ross county, Ohio. "This work," say Messrs. Squier and Davis (l. c., p. 11.) "occupies the summit of a lofty detached hill, twelve miles westward from the city of Chillicothe, near the village of Bourneville. "The hill is not far from four hundred feet in perpendicular height; and is remarkable, even among the steep hills of the west, for the general abruptness of its sides, which at some points are absolutely inaccessible." * * * * "The defences consist of a wall of stone, which is carried round the hill a little below the brow; but at some places it rises, so as to cut off the narrow spurs, and extends across the neck that connects the hill with the range beyond." It must not, however, be understood that anything like a true wall now exists; the present appearance is rather what might have been "expected from the falling outwards of a wall of stones, placed, as this was, upon the declivity of

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* Squier and Davis, l. c., p. 195.

† Squier and Davis, l. c., p. 252.

a hill." Where it is most distinct it is from fifteen to twenty feet wide by three or four in height. The area thus enclosed is about one hundred and forty acres, and the wall is two miles and a quarter in length. The stones themselves vary much in size, and Messrs. Squier and Davis suggest that the wall may originally have been about eight feet high, with an equal base. At present, trees of the largest size are growing upon it. On a similar work, known as "Fort Hill," Highland county, Ohio, Messrs. Squier and Davis found a splendid chestnut tree, which they suppose to have been six hundred years old. "If," they say, "to this we add the probable period intervening from the time of the building of this work to its abandonment, and the subsequent period up to its invasion by the forest, we are led irresistibly to the conclusion that it has an antiquity of at least one thousand years. But when we notice, all around us, the crumbling trunks of trees, half hidden in the accumulating soil, we are induced to fix on an antiquity still more remote."

The enclosure known as Clark's work, in Ross county, Ohio, is one of the largest and most interesting. It consists of a parallelogram, two thousand eight hundred feet by eighteen hundred, and enclosing about one hundred and eleven acres. To the right of this, the principal work is a *perfect square*, containing an area of about sixteen acres. Each side is eight hundred and fifty feet in length, and in the middle of each is a gateway thirty feet wide, and covered by a small mound. Within the area of the great work are several smaller mounds and enclosures; and it is estimated that not less than three millions of cubic feet of earth were used in this great undertaking.

It has also been observed that water is almost invariably found within or close to these enclosures.

Sacred and miscellaneous enclosures.—If the purpose for which the works belonging to the first class were erected is very evident, the same cannot be said for those which we have now to mention. That they were not intended for defence is inferred by Messrs. Squier and Davis from their small size, from the ditch being inside the embankment, and from their position, which is often completely commanded by neighboring heights.

Dr. Wilson also (vol. i, p. 324) follows Sir R. C. Hoare in considering the position of the ditch as being a distinguishing mark between military and religious works. But Catlin expressly tells us that in the Mandan village which he describes, the ditch was on the inner side of the embankment, and the warriors were thus sheltered while they shot their arrows through the stockade. We see, therefore, that, in America at least, this is no reliable guide.

While, however, the defensive earthworks occupy hill-tops, and other situations most easy to defend, the so-called sacred enclosures are generally found on "the broad and level river bottoms, seldom occurring upon the table-lands, or where the surface of the ground is undulating or broken." They are usually square or circular in form, a circle being often combined with one or two squares. "Occasionally we find them isolated, but more frequently in groups. The greater number of the circles are of small size, with a nearly uniform diameter of two hundred and fifty or three hundred feet, and invariably have the ditch interior to the wall." Some of the circles, however, are much larger, enclosing fifty acres or more. The squares or other rectangular works never have a ditch, and the earth of which they are composed appears to have been taken up evenly from the surface, or from large pits in the neighborhood. They vary much in size; five or six of them, however, are "exact squares, each side measuring one thousand and eighty feet—a coincidence which could not possibly be accidental, and which must possess some significance." The circles also, in spite of their great size, are perfectly round, so that the American archæologists consider themselves justified in concluding that the mound-builders must have had some standard of measurement, and some means of determining angles.

The most remarkable group is that near Newark, in the Scioto valley, which covers an area of *four square miles!* A plan of these gigantic works is given by Messrs. Squier and Davis, and another, from a later survey, by Mr. Wilson. They consist of an octagon, with an area of fifty, a square occupying twenty acres, two large circles occupying, respectively, thirty and twenty acres. From the octagon an avenue formed by parallel walls extends southwards for two miles and a half; there are two other avenues which are rather more than a mile in length, one of them connecting the octagon with the square.

Besides these, there are various other embankments and small circles, the greater number about eighty feet in diameter, but some few much larger. The walls of these small circles, as well as those of the avenues and of the irregular portions of the works generally, are very slight, and for the most part about four feet in height. The other embankments are much more considerable; the walls of the large circle are even now twelve feet high with a base of fifty feet, and an interior ditch seven feet deep and thirty-five in width. At the gateway, however, they are still more imposing; the walls being sixteen feet high, and the ditch thirteen feet deep. The whole area is covered with "gigantic trees of a primitive forest;" and, say Messrs. Squier and Davis, "in entering the ancient avenue for the first time, the visitor does not fail to experience a sensation of awe, such as he might feel in passing the portals of an Egyptian temple, or in gazing upon the silent ruins of Petra of the desert."

The city of Circleville takes its name from one of these embankments, which, however, is no more remarkable than many others. It consists of a square and a circle, touching one another; the sides of the square being about nine hundred feet in length, and the circle a little more than a thousand feet in diameter. The square had eight doorways, one at each angle, and one in the middle of each side, every doorway being covered by a mound. The circle was peculiar in having a double embankment. This work, alas! has been entirely destroyed; and many others have also disappeared, or being gradually obliterated by the plough. Under these circumstances, we read with pleasure that "the directors of the Ohio Land Company, when they took possession of the country at the mouth of the Muskingum river in 1788, adopted immediate measures for the preservation of these monuments. To their credit be it said, one of their earliest official acts was the passage of a resolution, which is entered upon the journal of their proceedings, reserving the two truncated pyramids and the great mound, with a few acres attached to each, as public squares." Such enlightened conduct deserves the thanks of archæologists, and we sincerely hope that the company has prospered.

Both as being the only example of an enclosure yet observed in Wisconsin, and also as having in many respects a great resemblance to a fortified town, the ruins of Aztalan are well worthy of attention. They are situated on the west branch of Rock river, and were discovered in 1836 by N. F. Hyer, esq., who surveyed them hastily, and published a brief description, with a figure, in the "Milwaukie Advertiser." In "Silliman's American Journal," No XLIV, is a paper on the subject by Mr. Taylor, from which was derived the plan and the short account given by Messrs. Squier and Davis.* The most complete description is contained in Mr. Lapham's "Antiquities of Wisconsin."† The name "Aztalan" was given to this place by Mr. Hyer, because the Aztecs had a tradition that they originally came from a country to the north, which they called Aztalan. It is said to be derived from two Mexican words, Atl, water, and An, near. "The main feature of these works is an enclosure of earth (not brick, as has been erroneously stated) extending around three sides of an irregular parallelogram;" the river "forming the fourth side on the east. The space thus enclosed is seventeen acres and two-thirds. The corners are not

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* L. c., p. 131.

† P. 41.

rectangular, and the embankment or ridge is not straight." "The ridge forming the enclosure is 631 feet long at the north end, 1,419 feet long on the west side, and 700 feet on the south side; making a total length of wall of 2,750 feet. The ridge or wall is about 22 feet wide, and from one foot to five in height. The wall of earth is enlarged on the outside, at nearly regular distances, by mounds of the same material. They are called buttresses, or bastions; but it is quite clear that they were never intended for either" the one or the other. They vary from sixty-one to ninety-five feet apart, the mean distance being eighty-two feet. Near the southwest angle are two outworks, constructed in the same way as the main embankment.

In many places the earth forming the walls appears to have been burnt. "Irregular masses of hard reddish clay, full of cavities, bear distinct impressions of straw, or rather wild hay, with which they had been mixed before burning." "This is the only foundation for calling these 'brick walls.' The 'bricks' were never made into any regular form, and it is even doubtful whether the burning did not take place in the wall after it was built." Some of the mounds, or buttresses, though forming part of an enclosure, were also used for sepulchral purposes, as was proved by their containing skeletons in a sitting posture, with fragments of pottery. The highest point inside the enclosure is at the southwest corner, and is "occupied by a square truncated mound, which * * * presents the appearance of a pyramid, rising by successive steps like the gigantic structures of Mexico." "At the northwest angle of the enclosure is another rectangular, truncated, pyramidal elevation, of sixty-five feet level area at the top, with remains of its graded way, or sloping ascent, at the southwest corner, leading also towards a ridge that extends in the direction of the river."

Within the enclosure are some ridges about two feet high, and connected with them are several rings, or circles, which are supposed to be the remains of mud houses. "Nearly the whole interior of the enclosure appears to have been either excavated or thrown up into mounds and ridges; the pits and irregular excavations being quite numerous over much of the space not occupied by mounds." In these excavations and ridges, also, we should be inclined to see the ruins of houses. Some years ago a skeleton was found in one of the mounds wrapped apparently in cloth of open texture, "like the coarsest linen fabric;" but the threads were so entirely rotten, as to make it quite uncertain of what material they were made.

The last Indian occupants of this interesting locality had no tradition as to the history or the purpose of these earthworks.

Among the northern tribes of existing Indians there do not appear to be any earthworks corresponding to these so-called sacred enclosures. "No sooner, however, do we pass to the southward, and arrive among the Creeks, Natchez, and affiliated Floridian tribes, than we discover traces of structures which, if they do not entirely correspond with the regular earthworks of the west, nevertheless seem to be somewhat analogous to them."* These tribes, indeed, appear to have been more civilized than those to the north, since they were agricultural in their habits, lived in considerable towns, and had a systematized religion, so that, in fact, they must have occupied a position, as well economically as geographically, intermediate between the powerful monarchies of Central America and the hunting tribes of the north. The "structures" to which Mr. Squier alludes are described by him both in his "Second Memoir," and also in the "Ancient Monuments of the Mississippi valley," (p. 120.) The "Chunk Yards," now or lately in use among the Creeks, and which have only recently been abandoned among the Cherokees, are rectangular areas, generally occupying the centre of the town, closed at the sides, but with an opening at each end. They are sometimes from six to nine hundred feet in length, being largest in the

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* Squier, l. c., p. 136.

older towns. The area is levelled and slightly sunk, being surrounded by a low bank formed of the earth thus obtained. In the centre is a low mound, on which stands the Chunk Pole, to the top of which is some object which serves as a mark to shoot at. Near each corner at one end is a small pole about twelve feet high; these are called the "slave-posts," because in the "good old times" captives condemned to the torture were fastened to them. The name "Chunk Yard" seems to be derived from an Indian game called "Chunke," which was played in them.

At one end of and just outside this area stands generally a circular eminence, with a flat top, upon which is elevated the great Council House.

At the other end is a flat-topped, square eminence, about as high as the circular one just mentioned; "upon this stands the public square."

These and other accounts given by early travellers among the Indians certainly throw much light on the circular and square enclosures; but some of those, classed by Messrs. Squier and Davis under this head, seem to us to be the slight fortifications which surrounded villages, and were undoubtedly crowned by stockades. We have already seen that the position of the ditch is in reality no argument against this view; nor does the position of the works seem conclusive, if we suppose that the works were intended less to stand a regular siege than to guard against a sudden attack.

Sepulchral mounds.—The *sepulchral* mounds are very numerous. "To say that they are innumerable, in the ordinary sense of the term, would be no exaggeration. They may literally be numbered by thousands and tens of thousands." They vary from six to eight feet in height; generally stand outside the enclosures; are often isolated, but often also in groups; they are usually round, but sometimes elliptical or pear-shaped. They cover generally a single skeleton, which, however, is often burnt. Occasionally there is a stone cist, but urn burial also prevailed to considerable extent, especially in southern States. The contracted position of the corpse seems to be as usual as in the more ancient burials of Europe. Implements both of stone and metal occur frequently; but while personal ornaments, such as bracelets, perforated plates of copper, beads of bone, shell, or metal, and similar objects, are very common, weapons are but rarely found; a fact which, in the opinion of Dr. Wilson, "indicates a totally different condition of society and mode of thought" from that of the present Indian. Plates of mica are very generally present, and in some cases the skeleton is entirely covered by them.

What now is the "idea" implied in these often gigantic tumuli, and in the disposition of the corpse? The reason suggested by M. Troyon for the contracted position of the body has already been mentioned in this journal. Dr. Wilson appears to regard the tumulus as a simple development of that little heap of earth "displaced by interment, which still to thousand, suffices as the most touching memorial of the dead." Probable as these suggestions may appear, we confess that if we were to express an opinion we should lean rather to the opinion of the illustrious Swedish antiquary, Prof. Nillson, and imagine that the grave was but an adaptation, a copy, or a development of a dwelling-place. Unable to imagine a future altogether different from the present, or a world quite unlike our own, primitive nations seem always to have buried with their dead those things which in life they valued most; with ladies their ornaments; with chiefs their weapons, and sometimes also their wives. They burned the house with its owner; the grave was literally the dwelling of the dead. According to Prof. Nillson, when a great man died he was placed in his favorite seat, food and drink were arranged before him, his weapons were placed at hand, and his house was closed, sometimes forever, sometimes to be opened once more, when his wife or his children had joined him in the land of spirits. The ancient tumuli in northern Europe, which never contain metal, consist usually of a passage leading into a central vault, in which the dead "sit." At Godhavn, in the year 1830, a grave of this kind was opened, and numerous skeletons were found sitting on low seats around the walls, each with their weapons and ornaments. The description given by Captain Graah of the Eskimo "winter-house," and Scoresby's account of those belonging to the Greenlanders, agree closely with these graves, even to the fact that the passage points generally to the south or east, but never to the north. In a few cases tumuli have been examined which contained weapons, implements, ornaments, pottery, &c., but no human bones; in short, every indication of life, but no trace of death. Ernan also tells us that the graves of Tartars resemble their dwellings, a statement which Nillson apparently considers to be true of all primitive nations. In the Sulu islands it is the custom to desert any house in which a great man has died,* and Captain Cook mentions his having seen at Moon certain houses raised on mounds, in which he was told "the dead had been buried."

Certain small tumuli found in America have already been regarded as the remains of mud villages. Mr. Dille † has examined and described some small tumuli observed by him in Missouri. He dug into several, but never succeeded in finding anything except coals and a few pieces of rude pottery, whence he concluded that they were the remains of mud houses.‡ The Mandans, Minatarees, and some other tribes, also built their huts of earth, resting on a framework of wood.

On the other hand, there are some tumuli to which it would seem that this explanation is quite inapplicable, and which are full of human remains. This was long supposed to be the case with the great Grave Creek Mound, which, indeed, was positively described by Atwater§ to be full of human remains. This has turned out to be an error, but the statement is not the less true as regards other mounds. In conjunction with them may be mentioned the "bone pits," many of which are described by Mr. Squier.‖ "One of these pits, discovered some years ago in the town of Cambria, Niagara county, was estimated to contain the bones of several thousand individuals. Another which I visited in the town of Clarence, Erie county, contained not less than four hundred skeletons." A tumulus described by Mr. Jefferson in his "Notes on Virginia" was estimated to contain the skeletons of a thousand individuals, but in this case the number was perhaps exaggerated.

The description given by various old writers of the solemn "Festival of the Dead" satisfactorily explains these large collections of bones. It seems that every eight or ten years the Indians met at some place previously chosen; that they dug up their dead, collected the bones together, and laid them in one common burial place, depositing with them fine skins and other valuable articles.

Sacrificial mounds.—"The name of sacrificial mounds," says Dr. Wilson, "has been conferred on a class of ancient monuments, altogether peculiar to the New World, and highly illustrative of the rites and customs of the ancient races of the mounds. This remarkable class of mounds has been very carefully explored, and their most noticeable characteristics are, their almost invariable occurrence within enclosures; their regular construction in uniform layers of gravel, earth, and sand, disposed alternately in strata conformable to the shape of the mound; and their covering a symmetrical altar of burnt clay or stone, on which are deposited numerous relics, in all instances exhibiting traces, more or less abundant, of their having been exposed to the action of fire." The so-called "altar" is a basin, or table, of burnt clay, carefully formed into a symmetrical form, but varying much both in shape and size. Some are round, some elliptical, and others, squares or parallelograms, while in size they vary from two feet to

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* St. John's Life in the Forests of the Far East, Vol. ii, p. 217.

† Smithsonian Contributions, Vol. i, p. 136.

‡ Archæologia Americana, Vol. i, p. 223.

§ See also Lapham, l. c., p. 80.

‖ L. c., p. 25, 56, 57, 68, 71, 73, 106, 107. Squier and Davis, l. c., p. 118, &c.

fifty feet by twelve or fifteen. The usual dimensions, however, are from five to eight feet. They are almost always found within sacred enclosures; of the whole number examined by Messrs. Squier and Davis, there were only four which were exterior to the walls of enclosures, and these were but a few rods distant from them.

The "altar" is always on a level with the natural soil, and bears traces of long-continued heat; in one instance, where it appears to have been formed of sand, instead of clay, the sand for the depth of two inches is discoloured as if fatty matter of some sort had been burned on it. In this case a second deposit of sand had been placed on the first, and upon this stones a little larger than a hen's egg were arranged so as to form a pavement, which strongly reminds us of the ancient hearths in the Danish Kjkökenmöddings.

In a few instances traces of timber were found above the altar. Thus in one of the twenty-six tumuli forming the "Mound City," on the Scioto river, were found a number of pieces of timber four or five feet long, and six or eight inches thick. "These pieces had been of nearly uniform length; and this circumstance, joined to the position in which they occurred in respect to each other and to the altar, would almost justify the inference that they had supported some funeral or sacrificial pile."* The contents of these mounds vary very much. The one just mentioned contained a quantity of pottery and many implements of stone and copper, all of which had been subjected to a strong heat. The pottery may have formed a dozen vessels of moderate size. The copper articles consisted of two chisels, and about twenty thin strips. About fifty or a hundred stone arrow-heads, some flakes, and two carved pipes, completed the list of articles found in this interesting tumulus. In another mound nearly two hundred pipes were buried. Generally speaking, the deposit is homogeneous. "That is to say, instead of finding a large variety of relics, ornaments, weapons, and other articles, such as go to make up the possessions of a barbarian dignitary, we find upon one altar *pipes* only, upon another a single mass of galena, while the next one has a quantity of pottery, or a collection of spear-heads, or else is destitute of remains, except perhaps a thin layer of carbonaceous material. Such could not possibly be the case upon the above hypothesis, for the spear, the arrows, the pipe, and the other implements, and personal ornaments of the dead, would then be found in connexion with each other."†

This conclusion does not seem to us altogether satisfactory; and although these altar-containing mounds differ in so many respects from the above-described tumuli, we still feel disposed to regard them as sepulchral rather than sacrificial. Not having, however, had the advantage of examining them for ourselves, we throw this out as a suggestion, rather than express it as an opinion. We confess that we feel much difficulty in understanding why "altars" should be covered up in this manner; we can call to mind no analogous case. On the other hand, if Prof. Nillson's suggestion in relation to the ancient tumuli be correct, the long-continued fire will offer no difficulty; while the wooden constructions and the burnt bones will all be explicable on the hypothesis that we have before us a sepulchre, rather than a temple.

Nor does the "homogeneousness" of the deposits found in these mounds appear so decisive to us as to Messrs. Squier and Davis. Take, for instance, the cases in which pipes are found. The execution of these is so good that "pipe-carving" was no doubt a profession; the division of labor must have already begun. Exactly the same feeling which would induce them to bury weapons with the dead hunter, in order that he might supply himself with food in Hades as on earth; that feeling which among some ancient nations suggested the placing of money in the grave, would account not only for the presence of these pipes, but also for their number. The hunter could use but few weapons, and

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* Squier and Davis, l. c., p. 151.

† Ditto, p. 160.

must depend for success mainly on his strength and skill; whereas the pipe-seller, if he could use a pipe at all in the grave, might render his whole stock in trade available.

If, therefore, "the accumulated carbonaceous matter, like that formed by the ashes of leaves or grass," which suggests to Prof. Wilson "the graceful offerings of the first fruits of the earth, so consonant to the milder forms of ancient sacrifice instituted in recognition of the Lord of the Harvest," seems to us only the framework of a house, or the material of a funeral pyre; on the other hand, we avoid the conclusion to which he is driven, that on "the altars of the mould-builders, human sacrifices were made; and that within their sacred enclosures were practised rites not less hideous than those which characterized the worship which the ferocious Aztecs are affirmed to have regarded as most acceptable to their sanguinary gods."

Temple mounds.—The class of mounds, called by Messrs. Squier and Davis "Temple mounds," "are pyramidal structures, truncated, and generally having graded avenues to their tops. In some instances they are terraced, or have successive stages. But whatever their form, whether round, oval, octangular, square, or oblong, they have invariably flat or level tops, of greater or less area." These mounds much resemble the Teocallis of Mexico, and had probably a similar origin. They are rare in the north, though examples occur even as far as Lake Superior, but become more and more numerous as we pass down the Mississippi, and especially on approaching the Gulf, where they constitute the most numerous and important portion of the ancient remains. Some of the largest, however, are situated in the north. One of the most remarkable is at Cahokia, in Illinois. This gigantic mound is stated to be *seven hundred feet* long, *five hundred* feet wide at the base, and *ninety* feet in height. Its solid contents have been roughly estimated at twenty millions of cubic feet.

Probably, however, these mounds were not used as temples only, but also as sites for dwellings, especially for those of the chiefs. We are told that among the Natchez Indians "the temples and the dwellings of the chiefs were raised upon mounds, and for every new chief a new mound and dwelling were constructed." Again: Garcilego de la Vega, in his History of Florida, quoted by Mr. Haven,* says, "The town and house of the cacique of Osachile are similar to those of all other caciques in Florida, and, therefore, it seems best to give one description that will apply generally to all the capitals, and all the houses of the chiefs in Florida. I say, then, that the Indians endeavor to place their towns upon elevated places; but because such situations are rare in Florida, or that they find a difficulty in procuring suitable materials for building, they raise eminences in this manner. They choose a place to which they bring a quantity of earth, which they elevate into a kind of platform two or three pikes in height, (from eighteen to twenty-five feet,) of which the flat top is capable of holding ten or twelve, fifteen or twenty houses, to lodge the cacique, his family, and suite."

Animal mounds.—Not the least remarkable of the American antiquities are the *animal mounds,* which are principally, though not exclusively, found in Wisconsin. In this district "thousands of examples occur of gigantic basso-relievos of men, beasts, birds, and reptiles, all wrought with persevering labor on the surface of the soil," while enclosures and works of defence are almost entirely wanting, the "ancient city of Aztalan" being, as is supposed, the only example of the former class.

The "Animal Mounds" were first observed by Mr. Lapham in 1836, and described in the newspapers of the day, but the first account of them in any scientific journal was that by Mr. R. C. Taylor, in the American Journal of Science and Art, for April, 1838. In 1843 a longer memoir, by Mr. S. Taylor,

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*L. c., p. 57.

appeared in the same journal. Professor J. Locke gave some account of them in a "Report on the Mineral Lands of the United States," presented to Congress in 1840. Messrs Squier and Davis devoted to the same subject a part of their work on the "Ancient Monuments of the Mississippi Valley;" and finally, the seventh volume of the Smithsonian Contributions contains the work, by Mr. Lapham, which we have placed at the head of this article. Dr. Wilson does not appear to have made any original observations on this branch of the subject, but in a chapter on "Symbolic Mounds" he has given an interesting summary derived from these sources.

Mr. Lapham gives a map showing the distribution of these curious earthworks. They appear to be most numerous in the southern counties of Wisconsin, and extend from the Mississippi to Lake Michigan, following generally the courses of the river, and being especially numerous along the great Indian trail or war-path from Lake Michigan, near Milwaukie, to the Mississippi, above Prairie du Chien. This, however, does not prove any connexion between the present Indians and the mounds, as the same line has been adopted as the route of the United States military road.

The mounds themselves not only represent animals, such as men, buffaloes, elks, bears, otters, wolves, raccoons, birds, serpents, lizards, turtles, and frogs, but also some inanimate objects; if at least the American archæologists are right in regarding some of them as crosses, tobacco-pipes, &c.

Many of the representations are spirited and correct, but others, probably through the action of time, are less definite; one, for instance, near the village of Muscoda, may be either "a bird, a bow and arrow, or the human figure." Their height varies from one to four feet, sometimes, however, rising to six feet; and as a "regular elevation of six inches can be readily traced upon the level prairies" of the west, their outlines are generally distinctly defined where they occupy favorable positions. It seems probable that many of the details have disappeared under the action of rain and vegetation. At present a "man" consists generally of a head and body, two long arms and two short legs, no other details being visible. The "birds" differ from the "men" principally in the absence of legs. The so-called "lizards," which are among the most common forms, have a head, two legs, and a long tail; the side view being represented, as is, indeed, the case with most of the quadrupeds.

One remarkable group in Dale county, close to the Great Indian trail, consists of a man with extended arms, seven more or less elongated mounds, one tumulus and six quadrupeds. The length of the human figure is one hundred and twenty-five feet, and it is one hundred and forty feet from the extremity of one arm to that of the other. The quadrupeds vary from ninety to a hundred and twenty-six feet in length.

At Waukesha are a number of mounds, tumuli, and animals, including several "lizards," a very fine "bird," and a magnificent "turtle." "This, when first observed, was a very fine specimen of the art of mound-building, with its graceful curves, the feet projecting back and forward, and the tail, with its gradual slope, so acutely pointed, that it was impossible to ascertain precisely where it terminated. The body was fifty-six feet in length, and the tail two hundred and fifty; the height six feet." This group of mounds is now, alas! covered with buildings. "A dwelling-house stands on the body of the turtle, and a Catholic church is built upon the tail."

"But," says Mr. Lapham, "the most remarkable collection of lizards and turtles yet discovered is on the school section, about a mile and a half southeast from the village of Pewaukee. This consists of seven turtles, two lizards, four oblong mounds, and one of the remarkable excavations before alluded to. One of the turtle mounds, partially obliterated by the road, has a length of four hundred and fifty feet; being nearly double the usual dimensions. Three of them are remarkable for their curved tails, a feature here first observed."

In several places a very curious variation occurs. The animals, with the usual form and size, are represented not in relief, but in intaglio; not by a mound, but by an excavation.

The few "animal mounds" which have been observed out of Wisconsin differ in many respects from the ordinary type. Near Granville, in Ohio, on a high spur of land, is an earthwork known in the neighborhood as the "Alligator." It has a head and body, four sprawling legs, and a curled tail. The total length is two hundred and fifty feet; the breadth of the body forty feet; and the length of the legs thirty-six feet. "The head, shoulders, and rump, are more elevated than the other parts of the body, an attempt having evidently been made to preserve the proportions of the object copied." The average height is four feet, at the shoulders six. Still more remarkable, however, is the great serpent in Adams county, Ohio. It is situated on a high spur of land, which rises a hundred and fifty feet above Brush creek. "Conforming to the curve of the hill, and occupying its very summit, is the serpent, its head resting near the point, and its body winding back for seven hundred feet, in graceful undulations, terminating in a triple coil at the tail. The entire length, if extended, would be not less than one thousand feet. The accompanying plan, laid down from accurate survey, can alone give an adequate conception of the outline of the work, which is clearly and boldly defined, the embankment being upwards of five feet in height by thirty feet base at the centre of the body, but diminishing somewhat toward the head and tail. The neck of the serpent is stretched out, and slightly curved, and its mouth is opened wide, as if in the act of swallowing or ejecting an oval figure, which rests partially within the distended jaws. This oval is formed by an embankment of earth, without any perceptible opening, four feet in height, and is perfectly regular in outline, its transverse and conjugate diameters being one hundred and sixty and eighty feet respectively."

When, why, or by whom these remarkable works were erected, as yet we know not. The present Indians, though they look upon them with reverence, can throw no light upon their origin. Nor do the contents of the mounds themselves assist us in this inquiry. Several of them have been opened, and "in the process of grading the streets of Milwaukie" "many of the mounds were entirely removed," but the only result has been to show that they are not sepulchral, and that, excepting by accident, they contain no implement or ornament.

Under these circumstances speculation would be useless; we can but wait and hope that time and perseverance may solve the problem, and explain the nature of these remarkable and mysterious monuments.

INSCRIPTIONS.

There is one class of objects which I have not yet mentioned, and which yet ought not to be left entirely unnoticed.

The most remarkable of these is the celebrated Dighton Rock, on the east bank of the Taunton river. Its history, and the various conclusions which have been derived from it, are very amusingly given by Dr. Wilson.* In 1783 the Rev. Ezra Stiles, D.D., president of Yale College, when preaching before the governor of the State of Connecticut, appealed to this rock, inscribed, as he believed, with Phœnician characters, for a proof that the Indians were descended from Canaan, and were, therefore, accursed. Count de Gebelin regarded it as a Carthaginian inscription. In the eighth volume of the "Archæologia" Colonel Vallency endeavors to prove that it is Siberian; while certain Danish antiquaries regarded it as Runic, and thought that they could read the name "Thorfinn," "with an exact, though by no means so manifest, enumeration of the associates who, according to the Saga, accompanied Karlsefne's expedition to Vinland, in A. D. 1007." Finally, Mr. Schoolcraft submitted a copy of it to Chingwauk

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* Vol. ii, p. 172.

an intelligent Indian chief, who "interpreted it as the record of an Indian triumph over some rival native tribe," but without, we believe, offering any opinion as to its antiquity.

In the "Grave Creek Mound" was found a small oval disk of white sandstone, on which were engraved twenty-two letters. Mr. Schoolcraft, who has especially studied this relic, finally concludes, after corresponding with many American and European archæologists, according to Dr. Wilson, that of these twenty letters, four corresponded with ancient Greek,* four with the Etruscan, five with the old Northern Runes, six with the ancient Gaelic, seven with the old Erse, ten with the Phœnician, fourteen with the Anglo-Saxon, and sixteen with the Celtiberic; besides which possibly equivalents may be found in the old Hebrew. "It thus appears that this ingenious little stone is even more accommodating than the Dighton Rock in adapting itself to all conceivable theories of ante-columbian colonization." A stone of such doubtful character could prove little under any circumstances; but it must also be mentioned that "Dr. James W. Clemens communicated to Dr. Morton all the details of the exploration of the Grave Creek Mound; * * * without any reference to the discovery of the inscribed stone. Nor was it till the excavated vault had been fitted up by its proprietor for exhibition, to all who cared to pay for the privilege of admission, that the marvellous inscription opportunely came to light to add to the attractions of the show."

One or two other equally doubtful cases are upon record; but upon the whole we may safely assert that there is no reason to suppose that the nations of America had developed for themselves anything corresponding to an alphabet. The picture-writing of the Aztecs and the Quipa of the Peruvians was replaced among the North American Indians by the "wampum." This curious substitute for writing consisted of variously-colored beads generally worked upon leather. One very interesting example is the belt of wampum "delivered by the Lenni Lenape Sachems to the founder of Pennsylvania, at the great treaty, under the elm-tree at Shachamox in 1682." It is still preserved in the collection of the Historical Society at Philadelphia, and consists of "eighteen strings of wampum, formed of white and violet beads worked upon leather thongs," the whole forming a belt twenty-eight inches long and two and a half broad. "On this five patterns are worked in violet beads on a white ground, and in the centre Penn is represented taking the hand of the Indian Sachem." The large number of beads found in the tumuli were, perhaps, in a similar manner intended to commemorate the actions and virtues of the dead.

THE MOUND-BUILDERS.

Just as the wigwam of the recent Mandan consisted of an outer layer of earth supported on a wooden framework, so also, in the ancient sepulchral tumuli, the body was protected only by beams and planks, so that when these latter decayed, the earth sank in and crushed the skeleton within. Partly from this cause, and partly from the habit of burying in ancient tumuli, which makes it sometimes difficult to distinguish the primary from secondary interments, it happens that from so many thousand tumuli we have only three skulls which indisputably belong to the ancient race. These are decidedly brachycephalic; but it is evident that we must not attempt to build much upon so slight a basis.

No proof of a knowledge of letters, no trace of a burnt brick have yet been discovered, and, so far as we may judge from their arms, ornaments, and pottery, the mound-builders closely resembled some at least of the recent Indian tribes; and the earthworks resemble in form, if they differ in magnitude from those still, or until lately, in use. Yet this very magnitude is sufficient to show that, at some early period, the great river valleys of the United States must have been

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* Vol. ii, p. 180.

very much more densely populated than they were when first discovered by Europeans. The immense number of small earthworks, and the mounds, "which may be counted by thousands and tens of thousands," might indeed be supposed to indicate either a long time or a great population; but in other cases we have no such alternative. The Newark constructions; the mound near Florence, in Alabama, which is forty-five feet in height by four hundred and forty feet in circumference at the base, with a level area at the summit of one hundred and fifty feet in circumference; the still greater mound on the Etowah river, also in Alabama, which has a height of more than seventy-five feet, with a circumference of twelve hundred feet at the base, and one hundred and forty at the summit; the embankments at the mouth of the Scioto river, which are estimated to be twenty miles in length; the great mound at Selsertown, Mississippi, which covers six acres of ground; and the truncated pyramid at Cahokia, to which we have already alluded; these works, and many others which might have been quoted, indicate, we think, a population large and stationary, for which hunting cannot have supplied enough food, and which must, therefore, have relied in a great measure upon agriculture for its support. "There is not," says Messrs. Squier and Davis, "and there was not in the sixteenth century, a single tribe of Indians (north of the semi-civilized nations) between the Atlantic and the Pacific, which had means of subsistence sufficient to enable them to apply, for such purposes, the unproductive labor necessary for the work; nor was there any in such a social state as to compel the labor of the people to be thus applied." We know also that many, if not most of the Indian tribes, still cultivated the ground to a certain extent, and there is some evidence that even within historic times this was more the case than at present. Thus De Nonville estimates the amount of Indian corn destroyed by him in four Seneca villages at 1,200,000 quarters.

Mr. Lapham* has brought forward some ingenious arguments for thinking that the forests of Wisconsin were at no very distant period much less general than at present. In the first place, the largest trees are probably not more than five hundred years old; and large tracts are now covered with "young trees, where there are no traces of antecedent growth."

Again, every year many trees are blown down, and frequent storms pass through the forest, throwing down nearly everything before them. Mr. Lapham gives a map of these windfalls in one district; they are very conspicuous, firstly, because the trees, having a certain quantity of earth entangled among their roots, continue to vegetate for several years; and, secondly, because even when the trees themselves have died and rotted away, the earth so torn up forms little mounds, which are often mistaken by the inexperienced for Indian graves. "From the paucity of these little 'tree-mounds,' we infer that no very great antiquity can be assigned to the dense forests of Wisconsin, for during a long period of time, with no material change of climate, we would expect to find great numbers of these little monuments of ancient storms scattered everywhere over the ground."

But there is other more direct evidence of ancient agriculture. In many places the ground is covered with small mammillary elevations, which are known as Indian corn-hills. "They are without order of arrangement, being scattered over the ground with the greatest irregularity. That these hillocks were formed in the manner indicated by their name is inferred from the present custom of the Indians. The corn is planted in the same spot each successive year, and the soil is gradually brought up to the size of a little hill by the annual additions." † But Mr. Lapham has also found traces of an earlier and more systematic cultivation. These consist "of low, parallel ridges, as if corn had been planted in drills. They average four feet in width, twenty-five of them having been counted in the space of a hundred feet; and the depth of the walk between them

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* L. c., p. 90.

† Lapham, l. c., p. 19.

is about six inches. These appearances, which are here denominated 'ancient garden-beds,' indicate an earlier and more perfect system of cultivation than that which now prevails; for the present Indians do not appear to possess the ideas of taste and order necessary to enable them to arrange objects in consecutive rows. Traces of this kind of cultivation, though not very abundant, are found in several other parts of the State." (Wisconsin.)

Date.—In the ancient monuments of the Mississippi valley it is stated that no earthwork has ever been found on the first or lowest terrace of any of the great rivers, and that "this observation is confirmed by all who have given attention to the subject." If true, this would, indeed, have indicated a great antiquity, but in his subsequent work Mr. Squier informs us that "they occur indiscriminately upon the first and upon the superior terraces, as also upon the islands of the lakes and rivers." Messrs. Squier and Davis* are of opinion that the decayed state of the skeletons found in the mounds may enable us to form "some approximate estimate of their remote antiquity," especially when we consider that the earth around them "is wonderfully compact and dry, and that the conditions for their preservation are exceedingly favorable." "In the barrows of the Ancient Britons," they add, "entire well-preserved skeletons are found, although possessing an undoubted antiquity of at least eighteen hundred years." Dr. Wilson† also attributes much importance to this argument, which, in his opinion, "furnishes a stronger evidence of their great antiquity than any of the proofs that have been derived either from the age of a subsequent forest growth, or the changes wrought on the river terraces where they most abound." This argument, if it proves anything, certainly requires a much longer time than eighteen hundred years, and carries us back, therefore, far beyond any antiquity indicated by the forests. These, nevertheless, have also a tale to tell. Thus Captain Peck‡ observed near the Ontonagon river, and at a depth of twenty-five feet, some stone mauls and other implements in contact with a vein of copper. Above these was the fallen trunk of a large cedar, and "over all grew a hemlock tree, the roots of which spread entirely above the fallen tree" * * * and indicated, in his estimation, a growth of not less than three centuries, to which must then be added the age of the cedar, which indicates a still "longer succession of centuries, subsequent to that protracted period during which the deserted trench was slowly filled up with accumulations of many winters."

The late President Harrison, in an address to the Historical Society of Ohio, made some very philosophical remarks on this subject, which are quoted by Messrs. Squier and Davis.§ "The process," he says, "by which nature restores the forest to its original state, after being once cleared, is extremely slow. The rich lands of the west are, indeed, soon covered again, but the character of the growth is entirely different, and continues so for a long period. In several places upon the Ohio, and upon the farm which I occupy, clearings were made in the first settlement of the country, and subsequently abandoned and suffered to grow up. Some of these new forests are now sure of fifty years' growth, but they have made so little progress towards attaining the appearance of the immediately contiguous forest as to induce any man of reflection to determine that at least ten times fifty years must elapse before their complete assimilation can be effected. We find in the ancient works all that variety of trees which give such unrivalled beauty to our forests in natural proportions. The first growth on the same kind of land, once cleared and then abandoned to nature, on the contrary, is nearly homogeneous, often stinted to one or two, at most three kinds of timber. If the ground has been cultivated, the yellow locust will thickly spring up; if not cultivated, the black and white walnut will be the prevailing growth. * * * * * Of what immense age, then, must be the works so often referred

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* L. c., p. 168.

† L. c., vol. i, p. 359.

‡ Willson, c., vol. i, p. 256.

§ L. c., p. 306.

to, covered, as they are, by at least the second growth, after the primitive forest state was regained?"

We get another indication of antiquity in the "garden-beds," which we have already described. This system of cultivation has long been replaced by the simple and irregular "corn-hills;" and yet, according to Mr. Lapham*, the garden-beds are much more recent than the mounds, across which they extend in the same manner as over the adjoining grounds. If, therefore, these mounds belong to the same era as those which are covered with wood, we get thus indications of three periods: the first, that of the mounds themselves; the second, that of the garden-beds; and the third, that of the forest.

But American agriculture was not imported from abroad. It resulted from, and in return rendered possible, the gradual development of American semi-civilization. This is proved by the fact that the grains of the Old World were entirely absent, and that American agriculture was founded on the maize, an American plant. Thus, therefore, we appear to have indications of four long periods:

1. That in which, from an original barbarism, the American tribes developed a knowledge of agriculture and a power of combination.

2. That in which the mounds were erected and other great works undertaken.

3. The age of the "garden-beds," which occupy some at least of the mounds. Hence it is evident that this cultivation was not until after the mounds had lost their sacred character in the eyes of the occupants of the soil; for it can hardly be supposed that works executed with so much care would be thus desecrated by their builders.

And 4. The period in which man relapsed into barbarism, and the spots which had been first forest, then (perhaps) sacred monuments, and, thirdly, cultivated ground, relapsed into forest once more.

But even if we attribute to these changes all the importance which has ever been claimed for them, they will not require an antiquity of more than three thousand years. We do not, of course, deny that the period may have been very much greater or very much less, but, in our opinion at least, it need not be greater. At the same time there are other observations which, if they shall eventually prove to be correct, would indicate a very much greater antiquity.

One of these is an account "given of a mastodon found in Gasconade county, Missouri, which had apparently been stoned to death by the Indians, and then partially consumed by fire. The pieces of rock, weighing from two to twenty-five pounds each, which must have been brought from a distance of four or five hundred yards, 'were,' says the narrator, 'evidently thrown with the intention of hitting some object.' Intermixed with burned wood and burned bones were broken spears, axes, knives, &c., of stone." This statement, which, if true, is of the highest importance, is given by Mr. Haven† without a word of caution, and is repeated by Dr. Wilson.‡ Both these gentlemen refer to the American Journal of Sciences and Art, (first series, vol. xxxvi, p. 199,) as if they were quoting from an article communicated to that respectable journal. Now, the fact is that the only authority for the statement is an anonymous correspondent of the Philadelphia Presbyterian. The editor of the American Journal, while reprinting the communication, inserted a notice requesting the author to make himself known, and to give some more particulars. I cannot, however, ascertain that, in answer to this appeal, any one came forward to take upon himself the responsibility of so important an observation.

Nor is this all. The original communication to the Philadelphia Presbyterian *never alludes to the mastodon at all*, but refers the skeleton to the mammoth; and the mastodon was first suggested by the editor of the American Journal. Under these circumstances it certainly seems to us that some better evidence

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* L. c., p. 19.

† L. c., p. 142.

‡ L. c. v. i., p. 112.

will be required before we can be expected to believe that any mastodon was ever stoned to death by North American Indians.

There are, indeed, upon record other facts of a similar tendency. We have, however, already exceeded our limits, and we will therefore defer the consideration of them to some future opportunity.

If, however, the facts above recorded justify the conclusion that parts at least of North America once supported a numerous and agricultural population, then we cannot but ask, What fatal cause has destroyed this earlier civilization? Why are these fortifications forsaken—these cities in ruins? How were the populous nations which once inhabited the rich American valleys reduced to the poor tribes of savages which the Europeans found there? History suggests by luxury or war. And the archæologist, if he perceive little evidence of the first, finds abundant proof of the second.

HISTORICAL SKETCH

OF THE

ACADEMY OF SCIENCES OF PARIS.

BY M. FLOURENS,

PERPETUAL SECRETARY, ETC. TRANSLATED FOR THE SMITHSONIAN INSTITUTION, BY C. A. ALEXANDER.

The Academy of Sciences of Paris was established in 1666, at a time when Italy had already seen the rise and fall of its Academy of the *Lyncei*, (founded at Rome in 1603, by the Prince Cesi, and extinct after his death,) and could still boast its Academy *del Cimento*, founded at Florence in 1651. Germany, too, had its Academy *Naturæ Curiosorum*, founded in 1652, and England its Royal Society, definitely established in 1660, but existing for some time previous. In the order of legal date, therefore, our own Academy is but the fifth, yet had it existed in a free or private form before it received a regular organization *by order of the King*. As a few men of letters, meeting in 1629 at the private residence of Courart, "without noise or pomp," as Pelisson tells us, "and solely with a view to the pleasures of intellectual association and a rational life," laid the foundation of the *French Academy*, (formally organized by Cardinal Richelieu, in 1635,) so the Academy of Sciences commenced in the assembling of a small company of savants at the houses, first of Monmor, and afterwards of Thevenot and Bourdelot. Here experiments and new discoveries were examined, and hither, as these meetings soon became celebrated, learned foreigners resorted; here the Italian Boccone presented his *observations* on the coral and shells of Sicily, and the Danish Stenon, a man of genius and an anatomist and geologist of great penetration, read his ingenious *discourse* on the anatomy of the brain.

"It is perhaps these assemblages of Paris," says Fontenelle, "which have given rise to many of the academies in the rest of Europe. It is, at any rate, certain," he adds, "that the English gentlemen who laid the first foundations of the Royal Society of London had travelled in France and been received in the houses of MM. Monmor and Thevenot."

I cite these words of Fontenelle without attaching to them, as may well be believed, too much importance. Dating from the middle of the seventeenth century, a new taste in philosophy had spread itself almost everywhere, and had as generally given rise to Academies.* As soon as the learned world began to grow weary of scholastics, that *philosophy of words* which had so long hindered it from perceiving the *philosophy of things*, (we owe these designations to Fontenelle,) as soon as it became tired of studying nature only in the ancients, and chose to study nature herself, Academies sprung into existence.

Academies are the offspring of the intellectual spirit of modern times. This modern spirit dates from Bacon, Galileo, Descartes; it is propagated through

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*Among those of the above mentioned and the succeeding century the academy of Berlin dates from the year 1700; those of St. Petersburg, Copenhagen, Edinburgh, Madrid, &c., belong to the commencement of the eighteenth century.

Leibnitz and Newton; it makes itself popular in Fontenelle, d'Alembert, Voltaire. The history I am writing is that of the spirit of the sciences from Bacon and Descartes to our own times.

Bacon offers us, in his *New Atlantis*, a perfect image of our academies. In that work there is an institute of Solomon. It is an academy like those of our day. We might think that in the latter we saw the Atlantis of Bacon carried into effect; "the dream of a savant realized," as Fontenelle says in his *Eloge* of Marsigli. In the institute of Solomon the members are distributed into sections, and each section corresponds to a science. Three members occupy themselves with mechanics, three with physics, three with natural history, &c.; some travel in foreign countries to bring thence machines, instruments, models, experiments, and observations of every sort; there are some whose sole employment is to try new experiments, &c. "The end of our foundation," says one of the members, "is the knowledge of causes and secret motions of things, and the enlarging of the bounds of human empire to the effecting of all things possible."

Fontenelle paints in his own manner—that is, with expressions of which each has its point and its import—the new spirit which endowed us with academies. "We have abandoned," he says, "a sterile system of physics, which has stood for centuries always at the same point. The reign of words and of terms is past; we seek now for things. Principles are established which we understand. We follow them, and hence we advance. Authority has ceased to have more weight than reason. What was received without contradiction, because it had long been received, is now examined and often rejected; and as the plan has been adopted of consulting, in reference to natural objects, nature herself rather than the ancients, nature more readily lends herself to discovery, and often, when solicited by new expedients of interrogation, accords to us the knowledge of some one of her secrets." Thus we see the empire of *terms* is past; *things* are preferred; *authority* is less consulted than *reason; nature* more than the *ancients;* in a word, we make *experiments.*

The ancients did not make experiments, or, at least, they made too few. They made them in no sustained, consecutive, unintermittent manner. Had they done so, they would have soon felt the need of academies. As Fontenelle justly says, "the revival of true philosophy rendered academies so necessary that they were at once established. That mass of materials which the new sciences—science become experimental—demand, there are no means either of collecting or preparing, except through the instrumentality of associations, and associations protected by the government. Neither the information, nor the care, nor the life, nor the faculties of an individual would suffice for it. There needs too great a number of experiments, too many of different kinds, too many repetitions of those of the same kind; it is requisite to vary them in too many modes, and to pursue them for too great a length of time in the same spirit."

Wherever we see the genius of experiment developed, we witness the rise of an academy. The Royal Society of London commences with the experiments of Boyle; the Academy *del Cimento* is the work of the disciples of Galileo; the Academy of Sciences of Paris was at first Cartesian, and the systems of Descartes may have been adverse to experiment, but his noble method, stronger than his systems, perpetually leads us back to it. Descartes asked of men but two things—leisure and the means of making experiments. "If there were in the world," he says, "some one whom we knew with certainty to be capable of making the grandest and most useful discoveries, and his fellow-men should exert themselves by every means to aid him in attaining the objects of his research, I see not how they could do aught else for him but defray the expense of the experiments which he would have occasion to make, and prevent his leisure from being interrupted by the intrusions of any one."—*(Discours de la methode.)* Elsewhere he observes, "the schools seem to me chiefly to have erred in this; that they have occupied themselves more in the speculative search for terms to be employed in treating of things than in the search for the truth of the things themselves, by means of good experiments; hence, they are poor in the latter and rich in the former."

"Up to this time," says the Cartesian Fontenelle, "the Academy of Sciences only considers nature in small portions. No general system, for fear of falling into the inconvenience of precipitate systems, in which the impatience of the human mind is too prone to take refuge, and which, once established, oppose the reception of new truths. To-day we assure ourselves of a fact; to-morrow of another which has no relation to it. True, we do not shun the hazarding of conjectures respecting causes; but these are only conjectures."

Claude Perrault, a man of genius in more than one line, and perhaps a more practical savant than Fontenelle, in the preface to the excellent *memoirs* which, in company with Duverney, he has given us *on the anatomy of animals*, speaks the same language with Fontenelle respecting the rising spirit of the Academy. "What most entitles the memoirs of the Academy to consideration is the irreproachable testimony of their assured and recognized verity. For they are not the work of an individual who may readily allow himself to be biased by his opinion; who does not easily perceive anything but what confirms the first thoughts he has entertained, and for which he has all the blindness and the complacency with which one regards his own views. These inconveniences cannot be incident to the *memoirs* in question, for they contain no facts which have not been verified by a whole company, composed of persons who have eyes for seeing these kinds of objects otherwise than the greater part of mankind see them, just as they have hands for handling them with more dexterity and success; who see well what is, and could with difficulty be brought to see what is not; who study not so much to discover novelties as to examine thoroughly what is alleged to have been discovered, and to whom even the assurance of having been deceived carries scarcely less satisfaction than a curious and important discovery. So much, in their minds, does the love of certainty prevail over everything else."[*]

The spirit, then, of the Academy of Sciences of Paris has been always the spirit of experiment, of direct study, of precise observation, the *love of certainty*. At first Cartesian, it afterwards became Newtonian; but whether with Descartes or Newton, or since Newton and Descartes, it has been always devoted to experiment. To write its history is to write the history of the experimental method.

I return to the first establishment of the Academy. I say the *first*, for there have, in fact, been two—that of 1666, and that of 1699. "The Royal Academy of Sciences," says Fontenelle, "had, by its labors and its discoveries, so well answered the intentions of the King, that, many years after its establishment, his Majesty was pleased to honor it with a new degree of attention, and to confer upon it a second organization, still more noble, and, so to speak, still more energetic than the first."

It is a circumstance worthy of note, though it has been but little remarked, that the idea was at first entertained of creating in 1666, not a simple academy of

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[*] *Histoire de l'Academie des Sciences*, (*Memoires pour servir a l'Histoire naturelle des animaux*, preface, p. VII.) Another testimony to the *spirit of the Academy*, comprising a judicious estimate of the *spirit of Descartes*, (that *spirit* which manifested its experimental tendency in spite of systems,) is that of Mairan, in his *Eloge* of Pourfour du Petit. "It was to the Academy that he recurred, not in quest of Cartesianism, but of the spirit of Descartes, the love of experiments, and all the ardor which that philosopher evinced in availing himself of their help; the spirit, in a word, of doubt and of discussion which characterizes his immortal method no less than it does the Academy; or rather it was there that he saw Descartes preferred by some, Newton by others, and still oftener Descartes associated with Newton, with Leibnitz, with Aristotle himself, and with all the great minds whose meditations and labors have enriched the human intellect with new acquisitions."

sciences, but a grand and comprehensive or universal academy. "M. Colbert," says Fontenelle, "at first conceived the project of an academy composed of all who were highly skilled in every department of letters. Historians, grammarians, mathematicians, philosophers, poets, orators, were equally to enter into this great body, in which all, even the most opposite talents, were to be united and reconciled. The *Bibliotheque du Roi* was destined to be their common place of meeting. The professors of history were to assemble there on Mondays and Thursdays; the votaries of the belles-lettres, Tuesdays and Fridays; the mathematicians and physicists, Wednesdays and Saturdays. Thus no day of the week was to remain unemployed; and that there might be something in common which should connect these different companies, there was to be the first Thursday of every month a general assembly of all, in which the secretaries were to report the judgments and decisions of their respective assemblies, and every one might ask a solution of his difficulties; for on what subject would not these *estates general* of literature have been ready to answer? If, however, the difficulties proved too considerable to be at once resolved, they were to be committed to writing and answered in the same manner, and all the decisions were to have been considered as issuing from the entire academy."

This project was not carried into execution; the plan of distinct academies was adhered to; doubtless because it was perceived that, even for academies, the first law of labor is *division.* G. Cuvier calls the modern era of the sciences, in other words their grand era, the era of the *division of labor.* Our present Institute has resolved the problem which Colbert proposed to himself—all the academies united by a common bond of emulation and fame, and each, as regards its special labors, independent and free.

To the idea of uniting everything, succeeded that of too thorough a separation. It was deliberated whether geometricians and physicists should form distinct societies, or be combined in one. Fortunately, they were combined in one. The spirit of geometry is the ever present, though often secret, guide of all our exact sciences.

The rules of the Academy date from the remodelling in 1699. "Till then," says Fontenelle, "the love of the sciences constituted almost alone its whole law." In 1699 positive and written laws were prescribed, all dictated by a sound discretion.

The whole number of academicians was seventy—ten *honoraries,* twenty *pensionaries,* twenty *associates,* and twenty *eleves.* The class of honoraries was not distributed into sections. That of pensionaries was composed of three geometers, three astronomers, three mechanicians, three anatomists, three chemists, three botanists, a secretary and a treasurer. Of the twenty associates, twelve were French; two geometers, two astronomers, two mechanicians, two anatomists, two chemists, and two botanists. The others were foreigners, and had no designated sections. It was in this list of the eight earliest foreign associates of the Academy that might have been seen, nearly at the same time, the names of Leibnitz, Newton, the two Bernouillis, Ruysch, and the Czar Peter. Of the *eleves,* each cultivated the science of the pensionary who had chosen him, for each of these last selected his own eleve; but, in 1716, the title of *eleve* was suppressed;* "a title," says Fontenelle, "which they have had the delicacy to abolish, though no one disdained it." In speaking of the anatomist Tauvey, whom he himself had chosen as eleve, Fontenelle gracefully says: "I believe that I could make no better present to the company than of M. Tauvey; and though my nomination was not honorable enough for

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* In the place of the twenty *eleves.* twelve *adjoints* were created, who had a deliberative voice in matters of science, as had also the *associates.* This class of twelve adjoints was composed, like that of the associates, of two geometers and the same number of astronomers, mechanicians, anatomists, chemists, and botanists.

him, yet his desire of entering the illustrious body was such as to prevent his being fastidious respecting the manner of his entrance." Regular ecclesiastics, or those attached to any order of religion, could be neither pensionaries, associates, nor eleves; but, fortunately, they might be honoraries, and so the Academy included Malebranche.

Till the reorganization in 1699, the Academy had occupied for its meetings a small chamber in the Bibliotheque du Roi; in the year just mentioned the King assigned it at the Louvre a *spacious and magnificent* apartment*, and it was here that its sessions were held for a century. Of these there were two a week (Wednesday and Saturday,) and each continued at least two hours, from three to five o'clock. Further, everything had been provided for the dignified conduct of these sessions. "The Academy," says the rule, "shall observe great care that on occasion of a difference of opinion among any of the academicians, they shall employ no term of contempt or asperity towards one another, either in their discourse or their writings; and even when combating the sentiments of the learned, whoever they may be, the Academy shall exhort its members to speak with forbearance." So far was attention to this point carried, that savants of different denominations were placed by the side of one another—a geometer beside an anatomist, a botanist beside an astronomer; "for, as they did not speak the same language," says Fontenelle, "private conversations were less to be apprehended."

It was particularly desired that the discussions of the Academy should not resemble the disputes of the schools. These words of Fontenelle have much meaning: "Nothing can more contribute to the advancement of the sciences than emulation among savants, but an emulation confined within certain limits. It was, therefore, decided to give to the academic conferences a form quite different from the public exercises of philosophy, in which the great point is not to elucidate truth, but to avoid being reduced to silence. Here it was intended that all should be simple, quiet, without ostentation of ingenuity or knowledge, that no one should think himself obliged to be in the right, and that there should always be room for receding without discredit; above all, that no system should bear sway in the Academy to the exclusion of others, and that every door should at all times be left open to truth."

I shall add here but one particular, and that because it relates to Louis XIV. "The year 1681," says Fontenelle, "was a proud one for the Academy, through the honor which it received of a visit in person from the King." On this occasion the King, accompanied by the Dauphin, by Monsieur, his only brother, by the Prince of Condé, and a part of the court, visited the library, the laboratory, where some experiments were made before him, the hall of the meetings, where Colbert presented him the printed works of the academicians, &c. On retiring, his Majesty was pleased to say, "that it was not necessary for him to exhort the Academy to labor, since of themselves they evinced quite a sufficient spirit of application." Louis XIV had a native instinct for glory; he relished it in all its forms; he protected the arts; he loved letters, and gave an unwearied attention to the sciences; the list in which he caused the celebrated writers of his time to be inscribed, with a view to recompense them, received, likewise, the names of the illustrious savants, not only of France, but of Europe.

II.

HISTORY OF THE ACADEMY, BY FONTENELLE.

When Fontenelle was nominated, in 1697, perpetual secretary of the Academy of Sciences, he had been a member of the *French Academy* for six years, and four years afterwards he became a member of the Academy of Inscriptions

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* Expressions of Fontenelle. *Histoire de l'Academie des Sciences*, 1689.

and Belles-Lettres. He had, moreover, published all his principal works, among which his *Plurality of Worlds* (1686) had formed his true title to the place of secretary of the Academy of Sciences, as the *History of Oracles* (1687) opened to him the doors of the Academy of Inscriptions. His genius was therefore formed; his ideas had taken shape; he was master of his philosophy, his style, his distinctive manner; and this was first clearly seen in the two prefaces prefixed, the one to his *History*, of 1666, the other to that of 1699—works in which the new spirit of the sciences shines with so much lustre, and the finest, no doubt, that he has written.

"Fontenelle," says Cuvier, "by the clear and lucid manner in which he exhibited the labors of the Academy, contributed to diffuse a taste for the sciences more, perhaps, than any one of his time who cultivated them." That is true; but it is not enough. Fontenelle did not restrict himself to diffusing a taste for the sciences. No one more ably seconded Descartes, the destroyer of the scholastic philosophy; no one, after the great men who founded it, Descartes, Bacon, Galileo, Leibnitz, Newton, better comprehended the modern philosophy. He was one of the first who discerned the metaphysics of the sciences, and the first who made them speak the common language. His influence has been greater than is generally thought. The same thing has occurred in his case as in Buffon's: the writer has thrown into the shade the savant and the philosopher.

§ 1. *Of the scholastic philosophy.*

The scholastic philosophy sprang from what would cause it to revive to-morrow if there were no academies: from the persuasion that all was known. from adhesion to the words of the master, to the authority of the book; from resting in terms without proceeding to things. Fontenelle well defined this as the *philosophy of words*, and modern philosophy not less justly as the *philosophy of things*.

He says, in speaking of the treatise of Duhamel, entitled *Philosophia Vetus et Nova*, &c.: "This work appeared in 1678, and is as judicious and happy an assemblage as could well be found of the old and new ideas, of the philosophy of words and of that of things, of the schools and of the academy." He everywhere mocks at *substantial forms and occult qualities:* "words, he says," "which have no other merit but that of having long passed for things." In all this, it is true, he follows Descartes. The latter had said of the ancient philosophy, "It contains only words, and I seek only for things." Of *substantial forms and occult qualities* he pronounced, "that they were nothing more than chimeras, more difficult to be understood than all that was pretended to be explained by their means; that they had been invented only to make it easy to give a reason for everything, if it may be said, indeed, that a reason is given for things when we explain what is obscure by something which is still more so."

It is the daring genius of Descartes which animates the acute intellect of Fontenelle; an intellect not only acute, but singularly judicious, and which, when it is necessary, knows how to check Descartes himself. "The errors of Descartes," he says, "are such that quite often they impart light to other philosophers, whether it be that when he is deceived he is not far estray from the truth and the mistake is easy to rectify, or that he sometimes gives original views and furnishes ingenious ideas, even when he is most astray." He further says, and still with characteristic felicity, "It is by following the principles of Descartes that we place ourselves in a position to abandon his opinions." And again: "It behooves us always to admire Descartes, and sometimes to follow him."

There was one point, however, and an important one, in which he could never abandon Descartes. I mean his prejudices against a vacuum and against attraction. "Attraction and a vacuum," he says in his *Eloge* of Newton, "banished from physics by Descartes, and apparently banished forever, return under the leadership of Newton, armed with a new force of which they were thought incapable, and only perhaps a little disguised." He confounds, in this *Eloge*, the *attraction* of Newton with the *occult qualities* of the scholastics—a demonstrated *fact* with imaginary *forces*—and, without doubt, he is deceived: but we can afford to pardon the ingenious writer and profound thinker who had exerted so much talent in defending and extolling Descartes, if he remained Cartesian somewhat longer than others. We must render justice to Fontenelle for a half century of struggle against the ancients, and forgive him for having been himself something of an ancient.

§ 2. *Of the modern philosophy.*

Fontenelle everywhere opposes the modern philosophy to the scholastic. He calls it, as we have seen, the philosophy of things; he further calls it (and here we have the right word) the *experimental philosophy*.*

The modern philosophy is, in fact, philosophy sprung from the direct observation of things—from the study of facts—from experiment. And herein this most decided partisan of Descartes becomes the most judicious admirer of the great Galileo. "A rare genius," he says of him, "and one whose name will always be seen at the head of some of the most important discoveries on which modern philosophy is founded." Since Galileo, experiment is the guide, and. as Fontenelle has well said, the *sovereign mistress* of all our physical sciences. "We are at present thoroughly persuaded," he says, "that physics must not be treated except by experiments."—*Hist. of the Academy of Sciences*, 1724

He delights to point out at once the accurate attention and the auspicious sagacity which these experiments require. "The art of making experiments," he says, "carried to a certain stage, is by no means common. The least fact which offers itself to our eyes is complicated with so many other facts which compose or modify it, that it is impossible, without extreme address, to separate all that enters into it. or even, without extreme sagacity, to suspect all that *may* enter into it. It is necessary to decompose the fact which is before us into others which have also their own composition; and sometimes, if the route has not been well chosen, we become engaged in labyrinths from which there is no extrication. To us it appears that primitive and elementary facts have been hidden by nature with even as much care as causes; and when we arrive at a sight of them, it is a spectacle altogether new and wholly unforeseen."—*Eloge of Newton.*

No one before Fontenelle had so distinctly defined the great art of experiment.† That whole art, in effect, has but one end, that of giving us simple facts—simple facts which, compared together according to their nature, give us laws; and on this last point—which is the most elevated point of the experimental method—we may again profitably listen to Fontenelle: "The time will perhaps come when we shall unite in a regular body these scattered members, *(isolated facts;)* and if they are such as we could wish, they will come together, in some sort, of themselves. Various separate truths, at least,

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* "What the *experimental* philosophy is in relation to the scholastic." * * * (*Eloge of Duhamel.*) One utility of this work, (the *Opticks* of Newton,) as important, perhaps, as that which we derive from the great number of new facts of which it is full, is, that it furnishes an excellent model of the art with which the *experimental philosophy* is to be conducted.

† He made an approach to this lucid definition when he said: "The laws of the impact of bodies are very simple, but in almost all the effects which they produce to our eyes they are so enveloped and so smothered under the multitude of different circumstances that it is difficult to disentangle them, and to see them in their natural simplicity. The secret is, to separate first the greatest number of circumstances possible, and to consider only the cases into which there enter the fewest of those circumstances."—*Hist. of the Academy of Sciences*, 1706.

if in sufficiently large number, strike the mind in so vivid a manner with their relations and mutual dependence, that it would seem, that after having been detached by a species of violence from one another, they naturally seek to reunite themselves again."—*Preface of* 1699.

§ 3. *Of the metaphysics of the sciences.*

Each science has its metaphysics, or, as we more commonly say nowadays, its philosophy.

Descartes praises, in his cotemporary Desargues, certain new views on the metaphysics of geometry. "The mode," he says, "in which he commences his reasoning is so much the more commendable as it is more general, and seems to be derived from what I am in the habit of calling the metaphysics of geometry." "The geometrical spirit," says Fontenelle, "is not so identified with geometry that it cannot be transferred to other subjects of knowledge."* And it was in this connexion that he gave us his striking allusion to Descartes: "Sometimes a great man gives the tone to his whole age, and he to whom we might most justly ascribe the glory of having established a new art of reasoning was an excellent geometer."

What he admires in the sciences, and would especially challenge admiration for, is not so much discovery as the art of discovering: "Perhaps," he says, "the excellence of the geometric methods which from day to day are invented or improved, will bring us at last to see the import of geometry—that is to say, of the art of making discoveries in geometry, which is everything;" it is less the material truth than the abstract truth. "Although lines and numbers," he says, "should conduct absolutely to nothing, they would still teach us how to operate upon truths;" it is less the fact than the idea. He seeks everywhere "that genius of metaphysics which," as he says, "hides itself, and can only be perceived by eyes sufficiently penetrating." †

Above physical science he sees an intellectual science: in physical science the cases are particular, the experiments bounded; it is intellectual science which gives them a general force, and, to borrow one of his striking expressions, a *universal spirit.* ‡

§ 4. *Of common language applied to the sciences.*

"When the Academy of Sciences," says Fontenelle, addressing the *Academie francaise*, "assumed a new form at the hands of one of your most illustrious colleagues,§ he conceived the design of diffusing, as far as was in his power, the taste of the abstract and elevated sciences which formed his sole occupation. These employed, for the most part, as in ancient Egypt, only a species of sacred language understood by none but priests and a few of the initiated. Their new lawgiver desired that they should speak, as far as possible, the common language,

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* Elsewhere he says, "geometry, and what is still better worth, the geometric spirit."—*Eloge of Guglielmini.* "The art of discovery in mathematics is more valuable than the greater part of what is discovered."—*Eloge of Liebnitz.*

† *Discours a l'Academie francaise*, 1741. He says of Leibnitz: "He was a metaphysician, and it was next to impossible that he should not be so; he had too universal a spirit. I mean universal not only because he essayed everything, but still more because in everything he seized upon the highest and most general principles, which is the character of metaphysics."—*Eloge de Leibnitz.*

‡ He employs this expression in speaking of the necessary union of geometry and physics: "It is requisite that the subtle speculations of the one should become embodied, so to say, by connecting themselves with the experiments of the others; and that experiments naturally limited to particular cases should assume, by means of speculation, a universal spirit, and be changed into principles."—*Preface of* 1666.

§ The Abbé Bignon, member of the *Academie francaise* and honorary of the Academy of Sciences.

and he did me the honor of adopting me as their interpreter in this place." But this merit—and it is a great one—of having taught science to speak the common language, is the most generally known of Fontenelle's merits, and I restrict myself here to a mere mention of it.

Fontenelle, as has been seen, was nominated secretary of the Academy of Sciences in 1697; in 1699 the Academy was remodelled, receiving, among other ordinances, the following: "The secretary shall be exact in collecting in substance all that shall have been proposed, agitated, examined and resolved in this company, entering it on his register with a reference to each day of assembling, and he shall also insert therein the treatises which shall have been read; and at close of December of each year he shall give to the public an abstract of his registers, or an analytical history of the most remarkable acts and proceedings of the Academy."

Fontenelle addressed himself at once to the work, and in 1702 appeared the first volume of his great history. He excuses himself in the first lines for its retarded appearance: "According to the ordinance imposed by the King on the Academy at the beginning of the year 1699, this history ought to have appeared at the end of that year. But as the entire Academy was remodelled by that ordinance, it required some time to communicate to the whole a first movement, which it will henceforward be easy to maintain."—*Preface of* 1699.

This, in effect, was the case. From 1702 each year yielded its volume, containing, in part, the *Memoirs* of academicians, and, in part, the *History of the Academy*, by Fontenelle. The latter is composed of two portions—the general history of the Academy, of its labors, of its ideas, of the sciences with which it was occupied; and the history, the *Eloge* of individual academicians.

We will first consider the general history. In this Fontenelle combined an *abridgment* of everything remarkable which had been said or done in the Academy during the year, with an *analysis* of the printed memoirs, the whole discussed and illustrated, and composed, moreover, in a style of such admirable clearness as to recall at once the line of Voltaire:

> "The ignorant understood, the learned admired him."

"The design," says Fontenelle, "has been that the history should, on all subjects, whether common to it and the memoirs, or peculiar to the former, be adapted to the capacity of those who have but a moderate tincture of mathematics and physics." He goes on to say, "it has been considered that, with a view as well to profound savants as to those who are not such, it would be best to present, under two different forms, the materials which compose this collection; that the labors of the Academy would thence become better known, and the taste of the sciences be more widely diffused." He adds, in fine, "Care has been taken to intersperse in the history illustrations suited to facilitate the reading of the memoirs, and some of these will undoubtedly be more intelligible to the greater part of readers, if associated with that portion of the history which corresponds to them." Still another phrase is worthy of remark, as showing us that Fontenelle well understood the kind of service which he rendered to his colleagues. He observes, in his *Eloge* of the geometer Parent, who was reproached for the obscurity of his writings: "I cannot help recording it to his honor, that, in a letter written to his warmest friend two days before his death, he thanks me for having, as he said, made him intelligible. This was frankly conceding the fault which was imputed to him, and carrying very far his gratitude for a slight service which I owed him."

In reference to the savants whose history he has written, Fontenelle has two merits: that of having cleared up what is obscure, of having generalized what is technical in the writings of each; and that of having always employed what each has bequeathed us that is most important and most durable as the vehicle of his eulogy. He praises by means of facts which define the character. The following portraiture of the savants who illustrated by their genius the first half of the seventeenth century is an instance: "In Italy, Galileo, mathematician to the grand duke, at the commencement of that century first observed the spots on the sun. He discovered the satellites of Jupiter, the phases of Venus, the small stars which compose the milky way, and, what is still more considerable, the instrument of which he availed himself to discover them. Torricelli, his disciple and successor, devised the famous experiment of the vacuum, which has led the way to an infinitude of entirely new phenomena. Cavallerius detected the ingenious and subtle geometry of indivisibles which is now extended so far, and which, at every moment, embraces the infinite. In France, Descartes opened to geometers new paths which had not been before known, and disclosed to physicists a multitude of views which either suffice of themselves or prepare the way for others. In England, Baron Napier distinguished himself by the invention of logarithms; and Harvey by the discovery, or, at least, the incontestable proof of the circulation of the blood. The honor which accrued to the whole English nation from this new system of Harvey seems to have turned the attention of the English to anatomy. Several of them adopted particular parts of the body as the subject of their researches—Wharton, the glands; Glisson, the liver; Willis, the brain and the nerves; Lower, the heart and its movements. About the same time, the reservoir of the chyle and the thoracic duct were discovered by Pecquet, a Frenchman, and the lymphatic vessels by Thomas Bartholin, a Dane, to say nothing of the salivary ducts which Stenon, another Dane, taught us to know still more exactly than Wharton had done, nor of all which Marcel Malpighi, an Italian and first physician to Pope Innocent XII, observed in the epiploon, in the heart and in the brain, anatomical discoveries which, however important, will yet yield him less honor than his happy conception of extending anatomy even to plants. In fine, all the sciences and all the arts whose progress had been arrested for centuries, acquired, in this, new forces, and commenced, so to speak, a new career."—*Preface of* 1666.

Fontenelle portrays himself in his *Eloge* of Duhamel, that first secretary of the Academy of Sciences whom he has caused us to forget: "There was required for this association a secretary who understood and could competently speak all the different languages of these savants; who might be their common interpreter with the public; who could not only throw light on so many intricate and abstract topics, but give them a certain turn and even grace which authors sometimes neglect, and which yet the greater part of readers desire; one, in fine, who, by his character, should be exempt from partiality, and qualified to render a disinterested account of the academic contests. The choice of M. Colbert for this functionary fell upon M. Duhamel." It is in this same *Eloge* that we meet with this ingenious remark: "That which ought not to be embellished beyond a certain determinate point is precisely what it costs most pains to embellish;" and nothing could better characterize the writer's own felicitous manner and discriminating art.

I have already cited more than once the two prefaces which precede the *histories* of the years 1666 and 1699. The latter, on its first appearance, excited attention, not only in France, but in Europe. Not since the *discourse* of Descartes on method had there been heard such language applied to such objects. The admiration was universal. The preface of 1666 had a different fate. In the first place, it did not appear until several years later; and then, when it did appear, it attracted scarcely any notice. Trublet tells us that in his time it was almost unknown. "Many persons," he says, "have the history of the Academy of Sciences since 1699, and buy the new volumes as they issue from the press. Very few have had the curiosity to go back to 1666, or even know that M. de Fontenelle had labored on the first memoirs and composed the history of the first years of the Academy." Garat, in his *Eloge* of Fontenelle, crowned by the *Academie francaise*, says of the preface of 1699: "This preface, which comprises but a few pages, has yet made good its claim to be ranked among the distinguished works of the century. It is the most vigorous and comprehensive survey of human knowledge from Bacon to the preface of the Encyclopedia." This is well and justly said; but of the preface of 1666 there is not a word. Wherefore this silence? Had Garat not seen it? This I cannot believe; and yet the preface in question is quite as fine as that of 1699. Perhaps it is even more so, for there prevails in it a graver strain of eloquence, and, for that reason, a more excellent one.

Fontenelle, not having been nominated as secretary until 1697, might very well have acquiesced in the terms of the ordinance, which only exacted of him the history of the Academy after the year 1699; but the monument which he raised to the sciences would have thus been incomplete, and he undertook the entire history from 1666 to 1699. Duhamel had already written in Latin the history of these thirty-two first years, (*Regiæ Scientiarum Academiæ Historia*, 1698.) Fontenelle, through a considerate delicacy, did not publish the French history of these years until after the death of Duhamel, and long after. The latter died in 1706; the history appeared in 1733.

Duhamel had relinquished the functions of secretary in 1697. The lustre of the sciences, every day augmenting, demanded for them a more brilliant interpreter, and Fontenelle succeeded him. Duhamel, a most learned, laborious, and unpretending man, recalls, by his tone and by his Latin, the ancient period of the sciences. Fontenelle, by his original genius, his vivacity of thought, his language, and above all by his French style, represents their new period. To see the two eras, so different, yet so little distant, we need but compare the *Latin history* and *French history* of the Academy of Sciences, Duhamel and Fontenelle.

We must still recur to Fontenelle to learn how to speak of others and of himself. "At the commencement of 1697," he says, "M. Duhamel resigned the pen, having represented to M. de Pontchartrain, chancellor of France, that he had become so infirm as to stand in need of a successor. It would be to my own interest to conceal here the name of him who had the temerity to take the place of such a man; but the gratitude which I owe him for the goodness with which he accepted me, and for the care which he took to form me, does not permit me to do so."—*Eloge de Duhamel.*

In 1737 Fontenelle, at the age of eighty years, and having been secretary for forty, felt, in his turn, the necessity of retiring. He wrote, therefore, to Cardinal de Fleury, asking anew the *superannuation (veterance)* which he had already applied for seven years before. "It is just seven years," he said, "since I obtained from your eminence permission to abdicate the only dignity which I hold in this world, that of secretary of the Academy of Sciences. I then yielded to the instances of several of the members and remained, though compliment, no doubt, had its part in their remonstrances. Seven more years greatly strengthen the reasons which I then had. It is very far from being the case that every one is exempt from the danger of self-delusion. Whatever difference there may be between France and the Academy, I renew my earnest prayer, and am, with very profound respect, &c." The cardinal, who had his own reasons for not admitting that one is old at eighty years, (he was himself seventy-seven,) returned but an evasive reply. Fontenelle was therefore obliged to write to him a third time, three years after, in 1740; and this time the cardinal yielded, not, however, without reservations. "You are," he replied to him, "only an idle fellow and a libertine; but still it is necessary to have some indulgence for characters of that sort."

Fontenelle was nominated at the beginning of 1697; and, as he retired at the close of 1740, was, consequently, secretary for nearly forty-four years.

III.

ELOGES OF THE ACADEMICIANS BY FONTENELLE.

The *Eloges* of Fontenelle commence in 1699,* with the reorganization of the Academy, and he had already produced twelve in the year 1708. At that time a small volume appeared, entitled, "History of the Reorganization of the Academy of Sciences in 1699, and Historical Eulogies of the Academicians who have died since that time, with a preliminary discourse on the utility of Mathematics and of Physics."

In this work, forming the first collection of Fontenelle's *Eloges*, are embraced twelve memoirs, being those of Bourdelin, Tauvey, Tuillier, Viviani, the Marquis de L'Hôpital, Jacques Bernouilli, Amontons, Duhamel, Regis, Marshal Vauban, the Abbé Gallois, and Dodart. The preliminary *discourse* is the admirable preface of 1699, of which I have repeatedly spoken. The history is a curious though brief recital of the facts attending the recent inauguration of the Academy. An advertisement, which precedes the whole, announces that "the collection would be followed by no other until there was a sufficient number of *Eloges* to form a second volume equal to the first." This condition was fulfilled in 1717, when another volume appeared, followed by a third in 1722, and subsequently by another series of the *Eloges*. Of these memoirs, Fontenelle produced sixty-nine, and pronounced them all before the Academy within the space of forty-two years: 1699—1740.

The second volume is introduced by this simple and interesting preface: "There appeared, some years ago, a volume composed of the History of the Reorganization of the Academy of Sciences and *Eloges* of Academicians since dead. The present volume contains only the subsequent *Eloges*. They have all been composed to be read in the meetings of the Academy, and some expressions will be found in them which have a relation to that circumstance. The title of *Eloges* can hardly be considered just; that of *Lives* would have been more so; for properly they are but *lives*, such as would have been written with the design simply to render justice. I guarantee their truth to the public. A very large number of the facts which I relate have fallen under my own observation; others I have derived from the writings of those of whom I speak; others from the writings even of those who have assailed them, or from memoirs furnished by persons whose information was most exact. I have not felt at liberty, still less have I purposed, to draw portraits at pleasure of those whose memory is so fresh. If, in the mean time, it should be thought that they have not been sufficiently praised, I shall neither be surprised nor annoyed."

I confess myself well pleased to see that Fontenelle was not satisfied with the title of *Eloges*. The word *life* is the true and natural word; that of *Elogs* is but the conventional expression of a given literary epoch. Elsewhere he remarks: "These *Eloges* are simply historical—that is to say, true." And true they are to their full extent; hence it is that each is stamped with its own character, its own tone, with an originality springing from that of the personage who is the subject, and hence the Eloge of Méry, or of Couplet, is so different from that of Newton or of Malebranche.

The *Eloges* of Fontenelle for the first time, in France, brought savants into public notice, and the sciences into fashion. If he ably seconded Descartes, the founder of a new philosophy, he not less ably seconded Colbert, as much an innovator in politics as Descartes in philosophy. But who remembers now

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* "As the history of the Academy should be, as much as possible, that of the academicians, we shall not fail, when one of them dies, to render him a species of funeral honors in a separate article, in which we shall collect the most considerable particulars of his life. M. Bourdelin, having died in the year whose history we now write, will be the first towards whom the Academy will acquit itself of this duty."—*Histoire, &c.*, 1699.

what Colbert performed for savants and for science? What Richelieu had been for the *Academie francaise*, that was Colbert for the Academy of Sciences. We have noticed his grand idea of a general and universal academy, an institute such as we now possess. I find in every page of the *Eloges* of Fontenelle, traces of that assiduous, active, comprehensive solicitude which Colbert manifested for the sciences; a solicitude which, in a statesman, was then so great a novelty.

"M. Colbert," says Fontenelle, "favored letters, induced thereto, not only by his natural inclination, but a wise policy. He knew that the sciences and arts even alone suffice to render a reign glorious; that they extend the language of a nation more, perhaps, than conquests; that they confer the empire of intellect and of industry equally flattering and useful; that they attract a multitude of strangers, who enrich the country by their curiosity, adopt its habits, and attach themselves to its interests. For many centuries, the University of Paris has not contributed in a less degree to the grandeur of the capital than the residence of its kings. To M. Colbert we owe the lustre to which letters have attained, the rise of this Academy, of that of inscriptions, of the Academies of painting, sculpture and architecture, the new favors obtained from the king by the French Academy, the impression of a great number of excellent books at the expense of the royal press, a vast augmentation of the *Bibliotheque du Roi*, or rather of the public treasury of the learned, an infinitude of such works as great authors and skilful artisans only accord to the caresses of ministers and princes, a taste for the beautiful and refined everywhere diffused and continually gathering strength."—*Eloge of the Abbe Gallois.*

Here we have Colbert painted, after the manner of Fontenelle, by facts. The following are some of these facts, chosen among many others, for Fontenelle forgets none of them. His particular eulogies of different savants seem the general and continued eulogy of this great minister.

"If any new book of reputation or discovery of moment came to light, Colbert was soon apprized of it, and the recompense was not long deferred. The liberalities of the king were extended even to foreign merit, and sometimes sought out in the very depths of the north a savant surprised to find that he was known."

Homberg visited Paris when he was young, and, as sometimes happens with young men who visit Paris, for some time evaded the injunctions of his father to return. "At last," says Fontenelle, "the father grew impatient and his commands more pressing. Homberg prepared to obey, and was about entering the carriage, when M. Colbert sent to require his presence on the part of the king. This minister, believing that persons of eminent merit are a benefit to the state, made such advantageous offers to induce him to stay, that Homberg asked for some little time to form a decision, and finally determined to remain."

About 1682, a young geometer, then quite unknown, had resolved in a felicitous manner a problem which had been recently propounded. "Forthwith," says Fontenelle, "M. Colbert, who had spies to discover hidden or rising merit, disinterred M. Rolle in the extreme obscurity in which he lived, and bestowed on him a gratuity which became afterwards a settled pension."

Charles II, king of England, had sent to Louis XIV two repeating watches, the first which had been seen in France. These watches, which could only be opened by a secret artifice, got out of order, and it was necessary to repair them. But, how to open them? After some vain efforts, the horologist of the king ("with a courage," says Fontenelle, "not unworthy of remembrance") told M. Colbert that he knew a young Carmelite who was capable of effecting it. The watches were, therefore, given to this young Carmelite, who promptly opened them, and, moreover, repaired them, without knowing that they were the king's. "Some time after," says Fontenelle, "an order arrives from the minister, directing M. Sebastien to come to him at seven o'clock in the morning of a certain day; no explanation of the reason of this order; a silence which might well cause some alarm. Father Sébastien fails not to arrive at the hour; presents himself, confused and apprehensive; the minister praises him on account of the watches, tells him for whom he had worked, exhorts him to cultivate his great talent for mechanics, and in order still more to encourage him, and speaking still more to the point in his capacity of minister, confers on him a pension of six hundred livres; the first year being paid, according to the custom of those times, in advance." "Father Sébastien," adds Fontenelle, "was then but nineteen years of age; and with what a desire of meriting approval must he have been inspired! Princes and ministers who do not obtain suitable agents for every purpose, either do not know that there is need of men, or have not the art of finding them."

I have said that Fontenelle forgets nothing which was done by Colbert. "It was in 1665," he tells us, "that for the first time appeared the *Journal des Savans,* the idea of which was so novel and happy, and which still subsists with greater vigor than ever, accompanied by a numerous progeny, disseminated throughout Europe, under the different names of *Nouvelles de la republique des lettres; Histoire des ouvrages des savants; Bibliotheque universelle; Bibliotheque choisie; Acta eruditorum; Transactions philosophiques; Memoires pour l'histoire des sciences et des beaux-arts,* &c. M. de Sallo, ecclesiastic councillor to the Parliament, had conceived the design of it, and had associated with himself the Abbé Gallois, who, from the vast variety of his erudition, seemed born for this labor, and, what is more, and by no means common with those who know everything, who knew French, and wrote it well."

I find, from Fontenelle, that the *Journal* took at first a tone somewhat too bold; that it censured too freely most of the works which appeared; that the republic of letters, thinking its liberty menaced, revolted, and the publication was suspended at the end of three months. It reappeared in 1666, under the sole direction of the Abbé Gallois, "and immediately," says Fontenelle, "M. Colbert, struck with the beauty and utility of the *Journal,* took a taste for the work." From that moment the fortunes of the *Journal* were secured; a happy event, not only for letters and science in general, but for the Academy in particular." "The Abbé Gallois," says Fontenelle, "enriched his *Journal* with the principal discoveries of the Academy, which were then made known to the public only through this medium."

"In 1683, M. Colbert was lost to letters." Fontenelle says but these few words; but what do not these few words convey after all that precedes!

By the side of Colbert, who renovated by means of the sciences the face of the most civilized empire of the world, I place the memorial of the Czar Peter, who carried them into countries the most barbarous.

The czar came to Paris in 1717; he came with the curiosity of genius; he visited everything and penetrated everywhere, and especially did he observe the Academy of Sciences. "No sooner," says Fontenelle, "had he returned into his own dominions, than he wrote to the Abbé Bignon, through M. Erskine, a Scotchman, who was his first physician, that he desired to become a member of this company; and when acknowledgments were made him with all the respect and gratitude that were his due, he wrote with his own hand a letter to the Academy, which we will not venture to call a letter of thanks, though coming from a sovereign who had long been accustomed to regard himself as a man." "It was thenceforth obligatory," continues Fontenelle, "to send him, each year, the volume due to him in his quality of academician, which was always graciously acknowledged as an attention on the part of his colleagues."—*Eloge of the Czar Pierre I.*

In the letter, which Fontenelle does not venture to call one of thanks, the Czar said to the Academy: "The choice which you have made of us personally to be a member of your illustrious society could not but be highly agreeable to us. Hence we have not been disposed to defer our acknowledgment of the joy and gratitude with which we accept the place you offer us therein, having nothing at heart more than to make every effort in our power to promote, in our estates, the advancement of the sciences and fine arts, so as thereby to render ourselves more worthy of being a member of your society." And he adds: "As there had never yet been any very exact chart of the Caspian sea, we despatched competent persons thither to construct one, with all possible care, upon the spot, and we send it to the Academy in the confidence that it will be kindly received as a memorial of ourself."

There is an art common to all the *Eloges* of Fontenelle, and a special art in the portrait which he traces of each academician. Thus he graphically presents to us the physician and botanist, Morin: "Retiring to rest at seven o'clock and rising at six, throughout the year, he then gave three hours to his devotions. He now repaired to the post of his duties, the Hotel Dieu, going thither at five or six o'clock in summer, and between six and seven in winter. Mass he generally heard at Notre Dame. After his return home he read the Holy Scriptures, dined at eleven, and, when the weather was fair, spent till two in the royal garden, where he gratified his earliest and strongest passion by the examination of new plants. After this, if there were no poor whom he was called upon to visit, he shut himself up and spent the rest of the day in reading books of physic or erudition, chiefly the former, as his profession required. [This likewise was the time at which he received visits, if any were paid him. He often used this expression: 'Those that come to see me do me honor; those that stay away do me a favor.' It is easy to conceive that a man of his temper was not crowded with salutations; there was only now and then an Antony that would pay Paul a visit."] *

He thus paints to us the great astronomer Cassini, "Whose spirit was even, tranquil, exempt from those vain disquietudes and senseless agitations which are the most afflicting and the most incurable of all maladies. A vast fund of religion, and, what is still more, the practice of religion greatly promoted this perpetual calm. The heavens, which declare the glory of their Creator, had never more discoursed of it, nor ever carried more conviction to any one than to him."

He paints to us La Hire: "All his days, from beginning to end, were occupied by study, and his nights often interrupted by astronomical observations. No diversion for him but that of a change of labor; no other bodily exercise but going from the observatory to the Academy of Sciences, to that of architecture, to the royal college, of which also he was a professor. Few persons can comprehend the happiness of a recluse, who is such by a choice which is every day renewed."

After indulging himself in the praise of his savants, Fontenelle seems to feel new pleasure in transferring his praises to the sciences themselves. He says, in the memoir of Lemery: "We are almost weary of noticing this merit in those of whom we have to speak. It is a praise which very generally appertains to that particular and not numerous class of persons whom converse with the sciences separates from that with men." In reference to Varignon he says: "His character was as simple as his superiority of intellect would imply it to be. I have already given this same praise to so many members of this Academy, that it might be thought the merit of it belongs rather to our sciences than to our savants."

Portraying others, he portrays himself. He well says of the *Theodicæa* of Leibnitz: "This work of itself would sufficiently represent its author;" and as

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* The words between brackets are an addition to the rather meagre extract of the text, and are taken from a translation of Fontenelle's Eloge of Morin, by Dr. Johnson, first printed in the Gentleman's Magazine for 1741.—Tr.

much might be said of his own *Eloges* in regard to himself. We there see the character of his intellect: "Luminous, comprehensive, and capable of adding something of its own to all acquired knowledge."—*Eloge of Saurin.* We see also his disposition and turn of thought. He says of Reyneau: "He held himself aloof from business, still more from intrigue, and prized highly the advantages, so little appreciated, of being nothing;" of Tschirnaus: "True philosophy had penetrated to his very heart, and established there that exquisite tranquillity which is the greatest and the least coveted of all possessions;" of Varignon: "I have never seen any one who had more conscientiousness—I mean to say, who applied himself more scrupulously to satisfy the inward sentiment of duty, and contented himself less with having satisfied appearances;" and in the *Eloge* of Homberg: "Whoever has leisure for thought sees nothing better that he can do than to be virtuous."

The mind of Fontenelle had all the boldness which a superior judgment permits, or rather supposes. Otherwise, would he have chosen Descartes for a master? "In all inquiries," says he, "the first systems are too limited, too narrow, too timid, and it would seem that truth itself can only be the reward of a certain audacity of reason." But he stipulates that this audacity should be sagacious and discreet. "It is necessary," he says, "to dare in all things, but the difficulty is to dare discreetly; it is to reconcile a contradiction."—*Eloge of Chazelles.*

No one had more clearly or closely observed the powers of the human intellect than the continuer of Descartes and the historian of Leibnitz and of Newton; but he had observed those powers without being dazzled, and was aware of their limits. "A first veil," he says, "which covered the Isis of the Egyptians, has been for some time removed; a second, if you please, has been so in our own day; a third will not be removed, if that third be the last."—*Eloge of Ruysch.*

Colbert, having founded the Academy of Sciences in 1666, continued to be its immediate protector while he lived. On his death, which occurred in 1683, the Academy was transferred to Louvois, appointed superintendent of constructions, arts and manufactures in the place of Colbert; and on the death of Louvois in 1691, it passed to Pontchartrain, at first Secretary of State, and afterwards Chancellor of France. Pontchartrain gave the Academy in charge to the Abbé Bignon, his nephew, and "by doing so," says Fontenelle, "rendered to the sciences one of the greatest favors they have ever received from a minister." The abbé, who had long presided over the body, and thoroughly understood its constitution, contributed much, in fact, by his views and his influence, to the great reorganization of 1699. When the Duke of Orleans became Regent, he reserved to himself the government of the Academy. "He treated our sciences," says Fontenelle, "as a private domain, of which he was jealous." This prince, it is well known, possessed much taste and even talent for the sciences; he had become a chemist with Homberg, and evinced that speculative curiosity which belongs to genius, but unfortunately as ill regulated as all his other qualities.*

The Regent having assumed the direction of the Academy, the personage who represented it, whether secretary or president, Fontenelle or the Abbé Bignon, was naturally called upon to assist him in his labors. Fontenelle, with his usual delicacy, wished to defer this honor to the Abbé Bignon. "Nothing could be more courteous," replied the latter, "than your proposal that I should have the honor of rendering to his Highness, the Regent, an account of the affairs of the Academy of Sciences; but the matter would be infinitely better

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*"He was curious in all sorts of arts and sciences. He has often told me that he had sought, by all the means at his command, to obtain a sight of the devil and other extraordinary objects, and to know the future."—*Memoires de Saint Simon*, V, p. 121.

in your hands. The most important point is, that the Regent has declared that he reserved our sciences to himself alone. You and I will not quarrel about the reports he may require. But, however honorable this distinction may be to our Academy, or flattering to ourselves, I still have a fear that it may expose our poor savants to envy and the ill offices which might follow. I fear, moreover, that in the multiplicity of more important affairs with which his royal Highness is overwhelmed, it will not be possible for him to enter into all our details, whose numbers frighten even yourself, and which will hereafter undoubtedly increase. The example of our dear *Academie francaise* alarms me. From the day that the King condescended to take the title of its Protector, and it had the honor, consequently, of being immediately responsible to none but his Majesty, you know to what extent the spirit of state affairs invaded it, and how many evils, or at least inutilities, followed in their train. The Academy would soon be reduced to nothing, if it fell into a condition anything like this. Think of these things, I pray you."

These particulars are curious; happily, however, the Abbé was needlessly alarmed. The constitution of the Academy was an excellent one, and there are two things therein which strike me as possessing singular wisdom: one, that it had entire liberty in the domain of the sciences; the other, that it was absolutely limited to that domain: no function, either of administration, or even of instruction.

The Academy is no university. The barrier which separates them should be eternal. Universities teach, the Academy discovers and improves; this the very terms of its device inculcate: *Invenit et perficit.* Nor has it any more an administrative function. The Academy seeks, as it ought to seek, in everything, ideal excellence; administration aims only at practicable excellence. Solon gave to the Athenians, not the best possible laws, but such only as they could bear.

The numerous editions of the *Eloges* which have appeared since the death of their author are more or less infected with verbal errors. In one we read: "Two or three great geniuses suffice to advance theories very far in a short time, but practice demands greater slowness, because it depends on too many hands, most of which are *plus habiles.*" Read *peu habiles, i. e.* little qualified. Again: "Father Malebranche had taken little pains to cultivate the faculty of imagination; on the contrary, he was very prone to decry it; but he had that faculty in a very high and vivid degree, though it labored for an ingrate in spite of himself, and *directed (ordonnait)* reason while it hid itself from her." Read *embellished (ornait)* reason. In an edition of Fontenelle printed at the close of the last century, the word Monsieur, or its substitute the capital M., has been suppressed before the name of every personage; so that Fontenelle, the most scrupulous observer of all proprieties, is made to call M. de Pontchartrain simply *Pontchartrain,* or the *Chancellor;* the Minister M. de Maurepas, *Maurepas,* &c., &c. He had said: M. Tournefort, M. Leibnitz, M. Newton, &c.; the editor makes him say: *Tournefort, Leibnitz, Newton, Bossuet, Colbert, Louvois,* &c. In the *Eloge* of Sauveur we find: "One thing determined Sauveur to follow the sage counsel of Condom;" Condom being the great Bossuet, then dead but a few years. This sort of anachronism changes the whole physiognomy of the book.

Bosnage thus portrays Fontenelle: "Some pretend that mathematics distort and impoverish the mind; M. de Fontenelle might with reason serve to refute this disparaging idea in regard to mathematicians; he carries not into the world that absent and dreamy air imputed to geometers; he speaks not as a savant who knows nothing beyond the terms of his art. The system of the world, which had been, for any one else, the groundwork of a dogmatical dissertation, not to be understood without the help of a dictionary, becomes, in his hands, an agreeable pleasantry, and the reader who dreamed only of being amused, finds himself in some sort an astronomer without having thought of being so."

Voltaire writes to Fontenelle: "You know how to render things attractive which many other philosophers scarcely render intelligible; and nature owed to France and to Europe a man like you to correct the savants, and give the ignorant a taste for the sciences."* No one more than Fontenelle possessed that "subtle and dexterous art" which he admired in Leibnitz, "the art not only of arriving at truth, but of arriving at it by the shortest paths;" of occupying always those elevated points of view which command wide horizons, and of separating or disentangling ideas, which was constantly with him an object of the greatest solicitude.

The life of Fontenelle is so generally known that we shall recall but few particulars of it. Born at Rouen, he there composed most of his earlier works, and afterwards established himself in Paris. He was a nephew of the great Corneille, who gave the *Cid* to France the year before that in which Descartes presented to it the *Discourse upon Method.* Much has been written about Fontenelle, and the tone adopted has not seldom been sufficiently censorious. Grimm, for instance, strongly reproaches him for his famous expression, "If I had my hand full of truths, I would take good care how I opened it." But Grimm need not have been troubled; Fontenelle, in spite of the phrase, opened his hand often enough. Voltaire sarcastically calls him the *discreet Fontenelle.* Was it necessary that he should be as indiscreet as Voltaire? The following sayings of his seem better to paint his character: "I have never permitted myself to cast the slightest ridicule upon the least of the virtues;" and his reply to the Regent, who pressed him to accept the perpetual Presidency of the Academy, "Ah, Sire, do not deprive me of the pleasure of living with my equals."

Fontenelle was born the 11th of February, 1657, and died 9th January, 1757, having thus lived almost exactly a century. His birth and death connect two remarkable epochs, the death of Descartes and the meridian fame of Voltaire.

APPENDIX TO THE FOREGOING SKETCH.

"Fontenelle," says Arago, "had so brilliantly fulfilled the functions of secretary of the Academy of Sciences, that at his death no one was willing to succeed him. After much solicitation, Mairan consented to exercise those functions, provisionally, in order to give the learned body time to make a choice which it should not afterwards have occasion to regret. It was felt at last that the only means of avoiding all injurious comparison would be to give to the nephew of Corneille a successor content not to imitate him, and who should disarm criticism by the modesty of his pretensions. Under these circumstances Grand-Jean de Fouchy became, in 1743, the official organ of the old Academy.

"Fouchy had occupied this place more than thirty years, when Condorcet entered the learned company. The age and infirmities of the perpetual secretary made him desire an assistant, and he cast his eyes on this the youngest of his colleagues. As this measure seemed equivalent to the creation of a survivorship, it proved distasteful to that portion of the Academy which usually allied itself with the views of Buffon, while another portion, acting under the leadership of d'Alembert, with equal ardor supported the nomination." The opposing candidate was Bailly, the astronomer, certainly a noble and worthy com-

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* Voltaire terms Fontenelle: "The first of men in the new art of diffusing light and grace over the abstract sciences;" and he adds: "That he stands first among all the savants who have not been gifted with the faculty of invention." (*Siecle de Louis* XIV.) Fontenelle, it is true, made no discovery in the sciences, but he discovered the style for diffusing them. That *new art* of which Voltaire speaks is his *invention.*

petitor; but the acrimony excited on this occasion was such as it is to be hoped may seldom be allowed to enter the halls consecrated to science and human improvement.

> Edita doctrinâ sapientum, templa serena.

For some weeks, says Arago, the Academy presented the aspect of two hostile camps, and the asperity of the contest may be conceived when we learn that to the illustrious name of d'Alembert was applied the stigma of having proved "equally faithless to friendship, to honor, and the first principles of honesty." The charge rested on the fact that d'Alembert, looking to an early vacancy of the office, had first held out encouragements to Bailly to qualify himself for its discharge, and six years afterwards had offered similar inducements to Condorcet. The only injury to the two young aspirants would seem to have been to engage them to devote a portion of their time to the composition and publication of probationary *Eloges*, which brought their respective merits before the public eye and crowned them with the applause of the most judicious critics. "It is seldom," continues Arago, "that abstract principles impassion men to such a degree as on this occasion, while to the outward world the question clearly put seemed only to be, shall the successor of Fontenelle be called Bailly or Condorcet?" The choice of the Academy fell upon Condorcet.

It is to the period here reviewed, we must presume, that the words of the compiler of the article "Royal Academy of Sciences" in the Encyclopedia Britannica were designed to apply: "Through the intrigues or intervention of the court for the admission of unworthy members or the exclusion of the meritorious, the Academy had gradually sunk in public estimation until admission not only ceased to be an honor, but even became a subject of contempt and derision. Hence the following humorous and well-known epitaph:

> 'Ci-gît Piron, qui ne fut rien,
> Pas même academien.'"

Here there is a manifest confusion of objects. Such names as those of La Lande, Reaumur, Maraldi, De la Caille, Daubenton, d'Alembert, Condamine, Adanson, Daniel Bernouilli, Borda, Fontaine, Haller, Lavoisier, and others, thickly strewn through the Memoirs of the Academy of Sciences of that time, satisfactorily refute any presumption that it had ceased to merit or maintain its hold upon public respect or its title to the lasting admiration and gratitude of mankind. The writer of the *article* might have quoted other sarcasms directed to the same object with the epitaph. Thus, when Voltaire was asked in England respecting the Memoirs of the Academy, he replied: "It writes no memoirs, but it has published sixty or eighty volumes of compliments." And the above-named Piron had said that a *discours de reception* at the Academy ought never to exceed three words; "that the recipiendary should say, 'Many thanks, sirs;' and the director should reply, 'Sir, there is no occasion for any.'" But all these taunts were aimed at a different academy from the above; at what Voltaire termed the "academie de paroles," as he styled the other the "academie de choses." Yet the petulance of satire can scarcely lead us to believe that even the Academie Francaise, which still numbered among its members a d'Alembert, a Buffon, a Voltaire, had sunk so low as to render admission into its ranks "a subject of contempt and derision." True enough that the vices of its regimen, as exhibited by Arago, had done much to reduce it to a state of servility and imbecility. "Until 1758, the subjects for prizes proposed by this Academy exclusively related to questions of devotion and morality. The eloquence of the competitors was thus called upon to exercise itself successively on the science of salvation, on the merit and dignity of martyrdom, on purity of mind and body, on the dangers which lurk amidst seeming security, &c. The *Ave Maria* even was paraphrased. Each discourse was to be terminated by a short prayer. Accepted papers were never allowed to reach the public until they had been submitted to the rigorous censorship of four doctors of theology; nor could the approbation of the high dignitaries of the church which the distinguished assembly always counted among its members secure any dispensation from this humiliating formality." *(Arago; Eloge de Bailly.)*

The interference of an external influence foreign to the spirit and embarrassing to the ends of the institution certainly existed also in the case of the Academy of Sciences; but here it seems to have been limited to courtly intrigues for the admission or exclusion of particular persons. Arago shows us in his Autobiography that he piqued himself in no small degree on having sometimes successfully disconcerted the unauthorized procedures of a minister, and even on one occasion thrown discountenance on the advocacy of the king.

The official term of Condorcet as perpetual secretary conducts us to the period when, in common with other learned bodies of the kingdom, the Academies of Paris fell before the levelling force of the Revolution. Their suppression took place in 1793. They may be considered to have been revived by an ordinance of 1795, in an associated form, under the name of the National Institute, in which a distinction into classes superseded the ancient name of Academies. Of these classes there were three: 1. The class of the physical and mathematical sciences; 2, that of the moral and political sciences; 3, that of literature and the fine arts. This latter class represented as well the *Academie francaise* as the former academy of inscriptions and that of painting. The name of National Institute has survived all modifications of form and changes of dynasty.

M. Flourens thinks it worthy of note that on the day (1 pluviôse an IV) when the National Institute held its first public sitting, Cuvier read before it his memoir on the *species of fossil elephants* compared with *living species*. "In this memoir," he observes, "the illustrious savant announces, for the first time, his views respecting lost animals. Thus on the very day when the Institute opened the first of its public sessions, there was opened at the same time the career of the grandest discoveries made by natural history in our age; a singular coincidence, and one which the history of science should preserve." It is more to the present purpose to remark, that after a few temporary appointments, Cuvier was chosen perpetual Secretary in the first class for the section of physical sciences, and that M. Flourens survives to this day as his only and immediate successor in that section, thus presenting through this long space of time a permanence of official tenure which, taken in connexion with the instances of Fontenelle and Fouchy, must argue that in philosophical labors there is at least nothing unfavorable to relative longevity. The order of succession as perpetual Secretaries in the other section—that, namely, of the mathematical sciences—is Delambre, Joseph Fourier, Arago, Elie de Beaumont, (1854.)

In 1803, Napoleon, who was then First Consul, prescribed some modifications in the form and probably in the spirit of the Institute. The class of moral and political sciences was suppressed, and of the remainder a fourfold division was formed: 1. The class of physical and mathematical sciences; 2, that of French language and literature; 3, that of Ancient history and literature; 4, that of the Fine Arts. Having thus eliminated the suspicious element of ideology, Napoleon continued, as he had always done, to watch with judicious and lively interest over the prosperity of the Institute, of which he was himself a member, often selecting the objects of his trust and favor from its ranks, and exacting from it reports upon the progress and prospects of science, which he received in person with a distinction and eclat not usually bestowed upon such subjects in the courts of princes. This was quite conformable with the spirit which had led him in the campaigns of Egypt to sign himself in his orders of the day as "*member of the Institute*, commanding-in-chief the army of the East."* *(Arago; Eloge de Fourier.)*

Upon the Restoration a royal ordinance of March 21, 1816, again conferred on the classes of the Institute their ancient title of Academies. The first class once more became the Academy of Sciences; the second, the *Academie francaise;* the third, the Academy of Inscriptions and Belles-Lettres; the fourth, the Academy of Fine Arts. Finally, in 1832, Louis Philippe reinstated the proscribed class under the title of the Academy of Moral and Political Sciences, and to the five sections, of which this was at first composed, the present Emperor has added a sixth, devoted to politics, administration, and finance. The Institute, it is stated, now consists of 223 members, 31 associates, 228 correspondents, 7 secretaries, and 35 free academicians.

The commemoration of deceased members by a notice apart, formed, as has been seen, an early, and has remained a constant part of the academic observances. Of these notices it is said, by one of those most distinguished for their composition, that, equally with the memoirs of the Academy, "they were designed to have truth for their basis and their object." And although, as Cuvier remarks, it be difficult to observe the cold impartiality of history when the hand is resting, as it were, on the funeral urn of an instructor or friend, yet the general candor with which they are written, the equity with which merit is assigned even if censure is softened or evaded, cannot in most instances fail to secure the confidence of the reader. There is but little, perhaps, in English literature which resembles them, for it has been often complained that the biographical notices of literary and scientific men are here but too frequently either limited to dry catalogues of writings and discoveries, or spread through such wastes of commentary and circumstance that the traits of individuality lose their distinctness and vivacity. "It seems," says an English reviewer, "to have been an established tradition in our literature that the 'life' of a man of letters must necessarily be a dull book." However this may be, the writers of the *Eloges*, from Fontenelle to Arago, have certainly contrived for the most part to fill the comparatively narrow outline to which they are limited with such sharp and well-defined features of personality as to maintain an ever-varying interest in the subjects themselves as well as in their labors. There is here no gallery of scientific masks, but a succession of distinct and animated, if reduced, portraitures. The scientific value of the *Eloges*, on the other hand, seems to be assured as well by the ability of the distinguished savants to whom we owe them, as the character of the body to which they were addressed; and as the series extends from the middle of the seventeenth century to the present day, they are well calculated to show the successive steps and "devious paths" by which experimental philosophy has advanced to the "bright eminence" from which it now challenges the confidence and admiration of the world.—TRANSLATOR.

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* "I can understand and appreciate the grounds of your refusal," said Napoleon one day to a member of the Institute who had just declined a proffered office. "You wish to devote yourself entirely to your pursuits. Well, I myself, had not fortune called me to preside over the destinies of a great people, think you that I would have haunted the bureaus and saloons in quest of official favor from any quarter as minister or ambassador? No; I would have thrown myself into the study of the exact sciences. I would have advanced in the path of the Galileos and the Newtons. And since I have constantly succeeded in all great enterprises, I should have highly distinguished myself by my scientific labors. I should have left the memory of noble discoveries. No other glory could have tempted my ambition." (*Arago; Eloge de Thomas Young.*)

MEMOIR OF LEOPOLD VON BUCH.

By M. FLOURENS,

PERPETUAL SECRETARY OF THE FRENCH ACADEMY OF SCIENCES.

TRANSLATED FOR THE SMITHSONIAN INSTITUTION BY C. A. ALEXANDER.

"Dating from the first years of the age of Louis XIV," says Voltaire, "a general revolution has been effected in our arts, our genius, our manners, which must forever serve to mark the true glory of our country. This revolution," he adds, "did not stop in France; it extended to England, carried taste into Germany, science into Russia, and reanimated the languishing spirit of Italy." The period of which Voltaire speaks was in truth distinguished by the rise of an honorable and strenuous emulation among all the nations of Europe, and by an alliance of intellects which, deriving new force from mutual support, no longer feared to submit to investigation those great and fundamental questions whose solution might have seemed forever hidden from us.

In Germany, one of those who most contributed to inspire science with courage for arduous enterprises was Leibnitz. While this rare genius was meditating the project of endowing his country with a great literary and scientific association, a colony of French savants, driven into exile by the revocation of the edict of Nantes, came to seek shelter in his neighborhood. Profiting by this valuable aid, the Academy of Berlin was established. But the course of its prosperity was short. The reign of William I, the rigorous tactician, who thought of nothing but war, who measured the merit of his subjects by their stature, and defined savants *frivolous inutilities,* supervened. The learned assembly found itself from that moment discountenanced, and was only restored to its position under the influence of the great Frederick. This last monarch practiced no disguise as to his admiration for France, of which he loved alike the literature, the philosophy, the language, and above all the men of letters, whom he would fain have lured away to Berlin. In default of Voltaire or d'Alembert, he took from us Maupertius, and made him president of his Academy.

Frederick impressed on all the mental activities of his country the ardor which governed himself. Enlightened by his example, the oldest and most noble families perceived that to dedicate their sons to the higher objects of intellectual toil, was at once to reflect honor on themselves and to acquire for the nation inexhaustible resources of utility and fame. At Stolpe, in the Uckermark, in the tranquillity of a residence inherited from many generations, one of these families, which could already point to names illustrious in diplomacy and letters, numbered, among an attractive group of brothers and sisters, a young enthusiast, active and intelligent, but wayward and contemplative, who, neglecting the usual sports and pleasures of his age, devoted his childish admiration to the objects presented by the beautiful scenery in which he was nurtured.

After a preliminary course of instruction, the young Leopold Von Buch, born April 26, 1774, quitted the banks of the Oder in order to enter, at scarcely sixteen years of age, upon new and more severe studies. The school of mines, a first step to geology, was that in which his aptitude and energy received their earliest development.

Few sciences are at once so recent and so old as geology. In every age men have sought to know how the globe they inhabit was formed, and the problem has always proved highly embarrassing. Hence certain ancient philosophers were led to solve the difficulty by the very convenient supposition that the world is eternal. Fortunately, a writer much older than these philosophers, and, without himself suspecting it, much more learned, has transmitted to us a singularly faithful indication of the manner in which things had their beginning, and of the stages by which they have arrived at the state in which we now see them. The record of Moses had become, at the end of the XVIIth century, the theme which exercised all intellects. Stenon, Burnet, Woodward, Whiston,* applied themselves to the study of the deluge described in Genesis, and thought that all the changes of the globe might be explained by the effects of that deluge alone. Liebnitz† was the first to comprehend that previous to the action of the waters, a still more energetic action, that of fire, must have been exerted; for all has been melted, all has been liquefied. "And what other agent," he cries, "what other agent but fire could have been capable of dissolving those mighty bones of the globe, those naked rocks and imperishable boulders: *magna telluris ossa, nudæque illæ rupes atque immortales silices!*"

To Leibnitz succeeded Buffon. In his *Theory* of the earth, Buffon, as yet, saw nothing but the action of water; in his system on the *Formation of the planets,* he sees nothing but the action of fire; in his *Epochs of nature,* his best considered and most perfect work,‡ he skilfully subordinates the action of water to that of fire, assigns to each of these agents its part, to every event its place, to every fact its age; but this admirable book came too late. From the appearance of the two earlier productions of Buffon, his cotemporaries had been divided; some had taken sides for his *theory,* some for his system. The first imagined everything to have been formed by water, the last by fire; these were called *Vulcanians,* those *Neptunians.* In England the Vulcanians acknowledged as their chiefs Hutton and Playfair,§ in France Desmarets and Dolomieu.‖ The school of Freyberg, where Germany flocked around Werner,¶ became the centre of *neptunism.* It was here that the young Von Buch arrived in 1791.

Confided to the care of Werner, he was his favorite disciple, and an inmate of his house. In long and paternal colloquies, the master, who united with the

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* Stenon: *Nicolai Stenonis de solido intra solidum naturaliter contento dissertationis Prodromus. Florentiæ,* 1669.

Burnet: *Telluris theoria sacra, etc. Londini,* 1681.

Woodward: *An essay towards the natural history of the earth, etc.* London, 1695.

Whiston: *A new theory of the earth.* London, 1708.

† Leibnitz: *Protogæa, sive de prima facie telluris, etc.* (*Actes de Leipsick,*) 1683

‡ Further developments on this point may be seen in the author's *Histoire des travaux et des idées de Buffon.*

§ Hutton, (James,) born 1726, died 1797: Theory of the earth, with proofs and illustrations, in four parts. Edinburg, 1795. This work is a reproduction of two former Essays or Memoirs, published, the first in 1785, the second in 1788.

Playfair, John: *Illustrations of the Huttonian theory of the earth.* Edinburg, 1802.

‖ Desmarets, (Nicolas,) born 1725, died 1815, was the first who, in France, conceived the system of *vulcanism.*

Dolomieu, (Deodat-Guy-Sylvain-Tancrede de Gratet de,) born 1750, died 1801, was the geologist who, before Von Buch, most advanced the theory of volcanoes and that of the action of fire on the globe.

¶ Abraham-Gottlob Werner, born 1750, died 1817, influenced more than any other man of his time the progress of geology.

genius of method the charm of eloquence and the seductiveness of good nature, found himself happy in an opportunity of communicating to a quick and penetrating intellect the treasures of knowledge which had been accumulated by long years of meditation and observation, and which a disinclination for writing,[*] only to be accounted for by his happy facility of speech, left him no other means of imparting.

About the same period with Von Buch, there arrived at the school of Freyberg several young men with whom he naturally entered into relations of friendship. These attachments, so easily contracted in youth, so often dissolved amid the conflicts of life, were with him as enduring as life itself. No similarity of aims ever disturbed the uniformity of his regard for Charles Friesleben,[†] and throughout his whole career his love and admiration for Alexander Von Humboldt, who to a less candid nature might have seemed a dangerous rival, were as unrestricted as they were disinterested.

At eighteen years of age he made a first trial of his strength by publishing a *descriptive mineralogy*,[‡] from the motto of which we learn the boldness of his aspirations: "What is new," he says, "extends, what is great exalts, the circle of our observation." Soliciting, two years after, employment in the service of mines, he addressed to the Minister Heinitz a second essay, equally evincing the early penetration of his intellect: "What I have sought to prove," he says, "is the possibility of finding constant laws according to which the formation of crystals takes place." A *royal scholarship*, with a commission for directing the working of the mines, was speedily conferred on him, and imposed engagements whose restraints he submitted to for three years. But, independent in spirit and in fortune, with a rich future before him, knowing as yet no explanation of the great phenomena of the globe but those which the school of Freyberg admitted, and too clear-sighted to content himself with these, he threw aside the shackles of the artificial world with the badge of the engineer and resumed his liberty. This fortunate breach of discipline, the first awakening of genius, was silently connived at by government, to the subsequent advantage of both parties.

Of the disciples of Werner[§] it has been said, "that they dispersed themselves through all countries, from pole to pole, in order to interrogate nature in the name of their master." Von Buch was pre-eminently one of those indefatigable interrogators of nature. He set out in 1797, directing his course towards the Alps, wandered for some time in the mountainous districts of Styria, passed a winter at Salzbourg,[‖] and then turned his steps towards Italy. He wished to visit the places where violent commotions have ruptured the crust of the earth and opened it, according to his own expression, to the eyes of observers. It was here, however, that his confidence in the infallibility of his school was destined speedily to be shaken.

From Perugino, the young Neptunian already writes: "Here the different species of rocks seem to have been overwhelmed by chaos itself. I find the beds of porphyry above the secondary limestone, and the micaceous schists above the porphyry. Does not all this threaten with ruin the fine systems which determine the epoch of formations?" In a series of letters to his friend

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[*] The writings left us by Werner are few and very short: *Treatise on the characters of minerals*, (1774,) a work in which the spirit of Linnæus is predominant; *Classification and description of mountains*, (1787;) *New theory of the formation of mineral and metallic veins*, (1791,) a work of a high order, in which the genius of observation and method is everywhere visible.

[†] Johan Karl Friesleben, (died 1846,) captain of mines at Freyberg, and known by his geological writings *on the gypsum of the Val-canaria*, *Formations of Thuringia*, *&c.*

[‡] *Materiaux pour une description mineralogique de la contrée de Carlsbad.* Freyberg, 1792.

[§] See the excellent work of d'Aubuisson de Voisins; *Traité de geognosie*, etc., 1819.

[‖] A sojourn shared by his friend Humboldt, and memorable for the experiments of the latter on meteorology and eudiometry.

de Moll, we see that Italy appeared to his youthful and enthusiastic imagination a promised land; and that though science is always in his thoughts, nothing escapes notice, and all sorts of observations gratify him. If the Albanian hills constrain him to modify the ideas which he had brought with him respecting the insignificance of volcanic effects, yet, in the midst of constantly recurring alarms for the system of his master, he pleases himself with the description of the beauties unfolded before his eyes: "Nature," he exclaims, "seems here inexhaustible in the creation of delights which spring up at every step. Whoever has not seen the sun set in the sea while his rays gild the cupolas of the eternal city, whoever has not watched on Lake Nemi the alternating play of the light, can form no conception of the charm of those regions." A tone such as this reveals the man for whom, during a long career, study and the seductions of travel are to be inseparably linked, and who in research is intent only on that which is exalted and aggrandized by its union with the emotions of the soul.

Arriving at Rome, he there observes the doubtful traces of extinct volcanoes, and his disquietude increases. "I am lost," he says, "in the contradictions which seem to have been here accumulated. One knows not what to believe, nor even if it is permitted to trust one's own eyes."

To him Vesuvius had always held out the promise of a revelation. At length, after several delays, he saw it, on the 19th of February, 1799. "I arrived," he tells us, "by way of the fair plains of the Campania; a fog which covered the horizon suddenly vanished, and before me rose sublime the double peak of Vesuvius crowned with eternal flame. There it is! was the involuntary cry which an expectation so keen and so often disappointed drew from me; while the cloud in lifting itself seemed to aspire to unite the vast mountain with the heavens."

On approaching Naples, the young German, brought into contact with a vivacious and impassioned population, felt a natural surprise at the singular contrast which the brisk petulance of the inhabitants of these climates forms with the phlegmatic earnestness of his native Germany: "Here," he says, "where language seems scarcely the competent organ of expression, where gesture seems the true language, how does everything recall the idea of that mysterious fire which we know only by its effects, and which strikes us in so unexpected a manner."

Vesuvius, whose mysteries he so earnestly longed to penetrate, baffled him on this occasion with delusive hopes. He brought away little but a presentiment of the vast labors which lay before him: "I have seen the crater and descended it," he writes, "but I have realized nothing but a religious horror which certainly gives me no insight into the connexion of causes and effects." Following the currents of lava, he retraces that which filled Naples with dismay in 1767, as well as the fiery torrent which some years later swept away the town of Torre del Greco and spread far into the sea; and animated by recitals still impressed with terror, he paints the effects of this fearful unloosening of subterranean forces with a poetic energy which recalls the celebrated letter of the younger Pliny. From this first expedition our young savant was taught to comprehend that the study of strata tranquilly deposited by the waters is not, as was thought at Freyberg, the whole of geology, that nature reveals herself in crises, and it is only at such epochs that we can hope to detect secrets which were otherwise impenetrable.

Von Buch left Italy, where fire in activity spreads its ravages, only to pass into France, where Auvergne offered him the most suitable of theatres for the study of extinct volcanoes.

Buffon had seen in volcanoes nothing but a congeries of sulphurs and pyrites situated quite near the summit of mountains.* The sagacious and patient de Saussure had too long meditated and suffered among the snows of Mont Blanc to concede much influence to mountains of fire. Werner, averse to what might disturb the regular order of nature, which he had elaborated, and interrupt the tranquil flow of his instructions, accepted volcanoes but as local and limited accidents. It was thus that matters stood, and would perhaps have long stood, had not two travellers, who happened to be detained on the road to Moulins, been struck at observing the great difficulty experienced by a mason at work near them, in breaking the stones with which he was constructing a fountain: their hardness, color, and porous structure recalled to one of the observers the lavas of Vesuvius. "Whence do you bring these stones?" he asked. "From Volvic, near Riom." "*Volvic! Vulcani, vicus;* there must have been a volcano there," said our celebrated naturalist, Guettard,† to his friend Malsherbes; "let us take the road to Auvergne." This they did; it was in 1751. Guettard discovered a whole chain of extinct volcanoes, and revealed to his fellow-citizens that they trod a soil once on fire; the lavas, the cinders, the scoriæ, the mountains, with their craters, all lent confirmation to the fact. The unexpected announcement was received, we are told, with astonishment, and even with alarm.

Twelve years later, the practical and sagacious Desmarets, in the course of one of those excursions in which he traversed the whole of France on foot, made a visit to the Puy de Dome, and clearly distinguished the pillars of black stone, whose figure and position struck him with their resemblance to what he had read respecting basalts and giant causeways. In their regularity, these columns bore the indications of a melted product; ‡ and further investigation left no doubt on the mind of Desmarets that they had been cast by the action of fire.

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* "Perpendicular fissures, some larger, some smaller, have doubtless been formed in vast numbers in the body of the mountain. The rain, of course, penetrated into all these fissures, and taking up or dissolving whatever substances were capable of being thus acted on, have formed pyrites, sulphurs, and other combustible materials; and when, in a long succession of ages, these had accumulated in immense quantity, fermentation and conflagration have taken place, producing the explosions and other effects of volcanoes. Perhaps, also, there were masses of these mineral substances already existing before the rains could reach them, but whenever openings or crannies, by which the water and air could penetrate, have been formed, ignition has taken place in the inflammable matter and a volcano has been the result."—*Buffon*, 1, p. 287. "The fire of the volcano comes rather from the summit than from the lower depths of the mountain."—*Id.*, 1, p. 285. (The author cites here and elsewhere his own edition of Buffon, just published in 12 vols. 8vo.)

† Guettard, (Jean Etienne,) born 1715, died 1786: *Memoire sur quelques montagnes de la France qui ont eté des Volcans.* Mem. de l' Academie des Sciences, 1752.

‡ That the columns were almost always found at the termination of long courses of lava which had themselves issued from craters still discernible, carried conviction to the mind of Desmarets. "In 1763," he says, "I traversed a part of Auvergne, where traces of volcanoes are to be seen, and particularly from Volvic to the Monts Dor. On the route from Clermont to the Puy de Dome, I perceived prisms of a black and compact stone, like that which covered a great part of the surface, the prisms resting on a bed of scoriæ. It was evident that they pertained to the crust of black stone enveloping the high plain which leads to the foot of the celebrated mountain. When I considered the inconsiderable thickness of this crust established on a bed of scoriæ and overlying a mass of granite which had undergone no action of fire, the idea at once occurred to me that here was the product of a current which had escaped from a neighboring volcano. Proceeding on this idea, I ascertained the lateral and extreme limits of the deposit, and still found the prisms, presenting in the perpendicular section their faces and angles, and on the surface their bases perceptibly distinct from one another. I was thus decided in the belief that the prismatic basalt belongs to the products of volcanoes, and that this constant and regular form is the result of the state of fusion in which the lava once existed. There can, I think, be no doubt that the groups of prismatic columns in Auvergne pertain to the same conformation with those of the county of Antrim, in Ireland, and that the constant and regular form is in Antrim the result of a cause similar to that which announces itself in so uniform a manner in Auvergne."—Desmarets: *Memoire sur l'origine et la nature du basalte a grandes colonnes polygones*, &c.—(Mem. de l'Academie des Sciences, 1771.)

The igneous origin of basalts, the action of fire, then, was established, but where did this formidable agent reside? It was another French geologist who ventured for the first time to answer, *at great depths beneath the solid crust of the globe;*[*] a revelation which we owe to the genius of Dolomieu, so severely tried with misfortune, but endowed sometimes with an utterance which might seem little less than inspired.

These extinct craters and melted basalts, these fires at profound depths, strangely interfered with the system of the excellent Werner, who would admit of nothing beneath the granite, and could see nothing above it but deposits of aqueous formation.[†] It was a step, therefore, towards independence when Leopold von Buch ventured, first among the German Neptunians, into the very focus of vulcanism, to assure himself whether Auvergne, as it was described, really pertained to the existing world. The surprise he had felt at Perugino was here, of course, redoubled. Here, not nature alone offered him her guidance, but the men of genius also who had preceded him. What might not this young and vigorous intelligence hope, if successful in recovering the clue of those grand ideas with which these localities and phenomena had inspired his predecessors!

His exploration of Auvergne was persistent and profound. He applied to it all the resources of his mind, and may be said, by this forcing process, to have here conceived the germs of all the lofty views to the development of which his after life was consecrated. The account of this visit is filled with the traces of hesitation and of effort. At the sight of the basalts, he exclaims, "How is it possible to believe in their igneous origin when we recall the rocks which accompany them in Germany; and yet *here* how is it possible to doubt of it?" In view of the subverted and displaced strata, he says: "I see the whole edifice fall to pieces which, by a sweeping arrangement of the series of rocks, gave us the structure of the world at the same time with its history." Contemplating that long chain of hights *(Puys)*[‡] which stretch in succession from the Mont Dore, he is struck with a preconception of the possibility of the upheaval of the entire mass of these volcanoes: "What, indeed, prevents us," he asks, "from conceiving the whole mass of the Mont Dore to have been thus lifted up?"

Voltaire tells us that a Frenchman who, in his time, had passed from Paris to London, would find things not a little changed. He had left the universe a *plenum;* he would find it a *vacuum.* He had left behind a philosophy which explained everything by impulsion; he would find one which explained everything by attraction. When our young savant passed from Germany into France, something of the same sort had occurred to him.

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[*] "The first conclusions to be drawn are, that *here* the volcanic products pertain to a mass of materials which differ from granites, and are situated beneath them; that the volcanic agents resided under the granite and wrought at depths very far below it."—(Dolomieu: *Report made to the National Institute on his travels in the years* 1798–'99.) "To be as exact as possible," adds Dolomieu (as if alarmed at the temerity with which he had overstepped received ideas.) "I have taken care always to use the adverb *here*, in order to restrict to the precise localities which furnished my observations the conclusions I draw from them. But there is reason for believing that the same is the case with all other volcanoes, whatever may be the nature of the surrounding formations; that it is at great depths within or below the solid crust of the globe that the volcanic agents as well as the bases of all their ejections reside, and that it is there that lie concealed the causes which supply the flame attending the eruptions and which produce the fluidity of the lavas."

[†] There is nothing in geology more celebrated or which longer prevailed than the *system* of Werner: a universal and tranquil sea deposits, in vast masses, the primitive rocks, distinctly crystallized, in which at first silex predominates. The granite underlies all; to this succeeds gneiss, which is but a granite beginning to foliate; by degrees clay gains the preponderance; schists of different sorts appear, &c.

Werner never quitted Saxony, and it may be said of him that he was too hasty in concluding that all the world was constituted like his own province.

[‡] [*Puech* or *Puich*, an old Aquitanian word, signifying mountain.—Tr.]

Werner had pronounced that all rocks, without exception, porphyry, granite, even basalt, were the product of water; *here* the granite, the porphyry, the basalt, bore irrefutable testimony to the action of fire. Werner had taught that the superposition of strata had observed always the same order; the granite below the gneiss, and porphyry below the limestone, &c. In Italy and Auvergne the whole order was reversed; in one place the granite, elsewhere the porphyry, occurred above the limestone. Werner had said that the seat of volcanoes did not descend below the limit of the coals, the source, as he taught, of the materials which maintain them. Here the focus of the volcanoes showed itself beneath the deepest rocks, the porphyry, the granite, the terrestrial envelope. Werner, in fine, had seen in volcanoes only accidental and local phenomena of comparatively small potency. In Auvergne everything demonstrated the extent and power of those hidden and profound forces which had sufficed to elevate immense rocks, and even entire mountains, such as the Cantal and the Monts Dore.

The exploration of Auvergne, in opening to Von Buch a whole series of sublime views, impressed him with the necessity of calling new resources to his aid. It was said of him, by an Englishman, "that he went everywhere to take the measure of those who cultivated his favorite science;" and what he had learned respecting the sagacity of the French savants seems now to have inspired him with the desire of *taking their measure.* He went to Paris, formed connexions there, and among others with Haüy, the kindness of whose reception he acknowledges in terms which show how highly he prized the words of encouragement extended to him by this great master. The museums, the collections, the libraries, were no less objects of eager interest than the conversation of accomplished men. Levying contributions from every source, he referred all to his one great task of active labor and incessant meditation. Among the common elements of character, vanity was one in which he seemed wholly deficient. Impelled to constant observation as if by a necessity of his nature, he may be said, on leaving Auvergne, to have made but one tour, but it was a tour which lasted his whole life. "What mode of conveyance do you prefer?" he was asked by somebody who thought himself an observer. "What!" replied M. Von Buch, leaning on his inseparable umbrella, "you do not know how a geologist ought to travel!" As regards himself, he might have been seen traversing afoot, at one time, the entire chain of the Appenines; at another, that of the Alps; passing, in the same way, from the craters of Vesuvius to the mountains of Scotland; from Etna to the snows of the polar circle; again at his favorite station of the Monts Dore, on his route to Paris, where the society of kindred minds might delay but could not detain him. He gave no notice of his arrival, and still less of his departure. A savant, surprised at receiving a visit from him and going to return it, would not improbably find that he had again disappeared, and learn by a letter from Naples, perhaps, or Stockholm, where it would be necessary to inquire for M. Von Buch. At Paris, one day, a geologist of note going to see him, met him on the threshold of his hotel, umbrella in hand. It was a bad sign. "You are going out; allow me to accompany you." "Willingly." "But where are you going?" "To Berlin."

Setting out, as was his wont, every spring, he took with him no companion but the faithful one just mentioned; no guide but his impulse; no baggage but his book of notes, his barometer, two or three favorite volumes, and above all, that indefatigable pick to whose blows so many rocks have resounded; all contained in the vast pockets of a double vestment, which, always the same and proof against every change of temperature, generally bore the marks of this manifold service. If night overtook him, he directed his steps to the nearest town and presented himself at the best hotel, where his odd equipment could not but lead occasionally to singular mistakes. But, as the fragrance of his probity and kindness survived all other impressions, these strange apparitions of his came at last to be regarded, by the villagers among whom he passed, somewhat in the light of those of the benevolent genii of the old German legends. Each season saw him return, at a stated time, to the paternal manor, where a brother, who was blind, awaited him, and whom he would allow no one but himself to conduct to the waters of Carlsbad.

In 1804, Vesuvius having shown some signs of disturbance, he repaired thither anew; this time in company with MM. de Humboldt and Gay Lussac. The combined observations of these eminent men resulted in a scientific exposition of all the effects associated with volcanic eruptions. Vibrations of the earth were recognized as their inseparable concomitants; the nature of the gases exhaled, the composition of the lavas, the force, development, and duration of these terrible phenomena were all, for the first time, submitted to a discriminative examination.

Nominated, in 1806, a member of the Academy of Sciences of Berlin, Von Buch read, on the occasion, a discourse on the progression of forms in nature. The philosophic view of the succession of beings had been advanced by Buffon, and the recent labors of Cuvier had furnished a wonderful commentary. Germany was struck with admiration at these sublime views, derived from France. In this discourse the author paints the successive gradations of the creation; inorganic bodies serving for elements in a world which is preparing for animated beings; animated beings taking their place one after the other, from the most simple up to the most complicated; up to man, the last term of the progress, whose appearance suggests these striking words: "To the existence of this being, the freest and most exalted of all, a vast concourse of physical causes was necessary. He alone encompasses the globe from one pole to the other; detaches himself, by an internal force, from matter; elevates himself above it, and, this achieved, who shall presume to trace for him a limit?"

Some thirty years before the date of these expressions the famous book of Pontoppidan had, in some sort, revealed to Europe countries which belong to it, but which were then as little known as certain tracts of India or America. The soil of the Scandinavian peninsula—at that time a virgin one, as regards researches—held out to Von Buch a promise of new impressions. No sooner, in fact, does he arrive at Christiana,* than he finds mountains of porphyry resting on limestone, and enormous masses of granite supported by fossil-bearing strata. Thus was the last blow given to his early faith, and from this time he thought no more of defending Neptunism.

He devoted two years to a study of the formations of Sweden and Norway. Proceeding with his accustomed energy, sometimes by land, sometimes by sea, he explored the singularly indented coasts of the Scandinavian peninsula, ascending as far as the barren rocks of the North cape. He was occupied with the solution of an imposing problem.

For more than a half century the inhabitants of the coast thought they had observed a gradual depression of the level of the sea. At the suggestion of the celebrated astronomer Celsius there had been marks cut in the rocks at Gefle and Calmar. Linnæus had himself traced a level on a block, which he describes with botanical precision. Here a maritime city having become an inland one; there an arm of the sea having been transformed into a highway; and all tradition concurring, the people of the country could no longer doubt of a diminu-

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* Porphyries in huge masses, in mountains even, are seated on a calcareous, shell-bearing rock. These porphyries again are covered by a sienite almost entirely composed of feldspar in large beds, and this sienite is buried under a granite which is in nowise distinguished, as regards its composition, from a granite of the most ancient formation.

"These phenomena, which give undoubtedly great geological interest to the environs of Christiana, have been observed with much sagacity, and described by M. Haussmann, professor at Göttingen, in a special memoir, inserted in the journal of the Baron de Moll."—(*Von Buch, Voyage en Norwege et en Laponie.*)

tion of the waters. "How singular a phenomenon!" exclaims Von Buch; "and to how many questions does it give rise?" After due consideration he adds: "It is certain that the level of the sea cannot subside; the equilibrium of the waters forbids it. Yet the phenomenon of their retreat is no less unquestionable, and there remains but one admissible idea—that of a general upheaval of the land from Fredericshall to Abo, and perhaps to St. Petersburg."

When this striking idea was announced, the full importance of its bearing could not be at once foreseen. The demonstration of an upheaval of part of our continent is the discovery which has most strongly contributed to fortify the new theory of volcanoes and that of the origin of mountains, while it has given the most general insight into the continual effort, the incessant reaction of the interior of the globe against its envelope.

At the extremity of the peninsula other phenomena awaited the observer. The eternal snows, which hover in an atmosphere still capable of developing organized beings, and which, in the torrid zone, maintain themselves at the level of the summit of Mont Blanc, occupy, on the coasts of Finmark, hills scarcely more than five or six times the height of our tallest buildings. Here our ingenious Regnard had once essayed to brave the rigors of a region then deemed inaccessible, and, in view of the interminable wastes of ice, had described himself in verses which, he says, "were destined to be read only by the bears," as having reached the end of the world:

Hic tandem stetimus nobis ubi defuit orbis.

Much further than this *end of the world*, and beyond the polar circle, after the long and dismal winter, M. Von Buch was witness of that *boreal summer*, so curious and so little known, which he calls the *season of day*—a day which lasts for two months. Writing on the 4th of July, he says: "The continual presence of the sun and constant serenity of the air give to the days of these countries a peculiar charm. At the approach of midnight, when that orb prolongs its course towards the north, the whole region enjoys a perfect calm; the clearness is at every moment the same. It is only by the sinking of the mercury that the advance of the evening can be ascertained. After no long interval all nature begins once more to be reanimated; the mists rise from the surface of the earth; small waves on the waters show that the air which comes from the north presses with more force towards the south. The sun ascends from the horizon, its rays operate, and the murmur of rivulets, swelled by the melting snow, sensibly increases, until, through the effect of another night, one feels nothing but a soothing warmth."

Nor is Scandinavia less characterized by its inhabitants than its physical phenomena. Its icy waters and its lichens suffice to sustain the agility and vigor of the reindeer, that noble and docile companion of the nomadic life of the Laplander, a specimen of our race who bears in his stunted form and rustic manners the impress of the zone into which he has ventured to introduce our common humanity. By his side, but with marked differences, appear the Norwegian of the coasts, disdainful of his shrunken neighbor, and the agricultural Finn, who, in his softened manners, has carried civilization to the limits of the habitable world, and even aspires to borrow from us our most refined enjoyments. "I have seen," says Von Buch, "in a town near the North Cape, a public library, in which, by the side of the Danish poets, appeared the masterpieces of Corneille, Moliere, and Racine."

As a scientific authority, Von Buch now stood so high that he might well feel conscious of being a master in the field of higher generalizations, a field so vast and so rarely attained. His return was greeted with respect by his country, his academy, by learned Europe in general. Recurring to the theatre of his earlier labors, he traversed, for several following years, the mountain chains of Central Europe, with an attention always fixed on the grand ideas which he had propounded, namely, that the disorder of the primitive strata of the globe pertains to a profound subterranean cause which is connected with volcanic action; that not only the basalts but all crystalline rocks have issued from the earth in the state of lava, and that to the reactions of the earth are to be referred the elevation of mountains and that of entire countries,* such as Sweden.

In the winter of 1814, while absorbed in these thoughts, he found himself at London, as he might at times be found everywhere, and there encountered an accomplished Norwegian, the botanist Smith. "Our conversation," says Von Buch, "happened to turn on the facility with which one may transport himself from that capital to almost every known region, and the desire of profiting by it became so strong that we presently resolved to set out for the Canary islands." A fortunate resolution, which has endowed geology with a work that will remain the mark of one of its most important advances.†

The Canary islands had been already visited by skilful observers, among whom we may distinguish one of our former and most valued colleagues, M. Cordier, the continuer of Dolomieu; but hitherto they had only been studied for themselves. Von Buch studied them in subordination and with reference to his general conceptions.

His book is composed of two parts. The first embraces all the details of description: the study of rocks, elevation of mountains, variations of climate, &c. In the second and most important, the author sets forth, in a few pages, equally admirable for precision of language and fullness of information, his whole theory of volcanoes; the result of long and critical observation of what is most general and constant in those grand but hitherto mysterious phenomena.

After succinctly defining a volcano to be "a permanent communication between the atmosphere and the interior of the globe," he distinguishes the effort which *elevates* from the effort which *ruptures;* the first gives him what he calls the *crater of elevation*, the second the *crater of eruption.* He shows that in each volcano there is a central point around which the eruptions take place, and that this central point is always the highest summit—the *peak*—of the volcano. He discerns, further, a common action between all the volcanoes of the Canary islands, connecting with the peak of Teneriffe the eruptions of the Isle of Palma, and these last with those of Lancerotte; for these eruptions are

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* It would be more exact to say *the elevation of entire countries and of mountains;* for, according to Von Buch, it is the *red porphyry* which in the first instance elevates countries or continents, and the *augitic*, the *black porphyry*, which transpierces the red porphyry and elevates the mountains.

"The upheaval of the *pyrogenic porphyry* is posterior to the formation of the red sandstone and of the calcareous strata; but these sandstones are essentially connected with the formation of the *red porphyry*, and can scarcely be separated from it. It follows that the *pyrogenic porphyry* must have pierced the *red porphyry* as well as the sandstone, and to have pierced it, must have raised up this porphyry itself."—Von Buch: *Lettre à M. de Humboldt, renfermant le tableau geologique du Tyrol meridional*, 1822.

"From these considerations, I should not have been surprised to see, somewhere in the interior of these valleys, *pyrogenic porphyries* below the *red porphyry.* I have, indeed, sought for them in the whole extent of this last formation, but almost everywhere without success. I was more fortunate in descending the valley of the Avisio. After having been constantly proceeding on quartz-bearing porphyries as far as Cembra, at some leagues above the opening of that valley, I recognized below that place, and at the edge of a kind of plain, a very considerable mass of the pyrogenic formation, whose black color contrasts singularly with the red of the prevailing quartz-bearing porphyry, and which is decidedly distinct from the latter. It is evidently a rock, whose mass pertains to the formation of the pyrogenic porphyry. Its aspect clearly shows that it is engaged in the red porphyry, except towards the base, where it is connected, probably, with a mass of the same nature, which extends under all the mountains of the Alps."—*Ibid.*

† M. de Humboldt, in reference to this work, said: "Leopold von Buch is the first who has recognized the interconnexion and mutual dependence of volcanic phenomena, and has thereby proved himself the greatest geologist of our epoch."

all associated, *(solidaires,)* and one never commences until the other has ceased. As, in hands so skilful, the thread of analogy, once seized, is never broken, from the volcanoes of the Canaries he passes to those of the entire globe, and ranges them all under two classes, central volcanoes and volcanic chains. The first form the centre of a number of eruptions which take place around them; the second are all disposed in line, each following the other in the same direction, like a great rent or fissure of the globe; being, as Von Buch adds, probably nothing else but such a rent. From these isolated points of rock, elevated by fire, transporting his view over the innumerable isles everywhere scattered in the ocean, he combines them all under the generic name of *isles of elevation,* thus dispelling the opinion which long regarded the former as the relics of a submerged continent.

Scarcely had he returned from the Canaries (about 1819) when some inquiry led him to the Hebrides, whose basalts formed the object of his visit, and thus the giant's causeway became the route which reconducted him to Germany. There, a new problem hurries him to Paris; and though it is the midst of winter, and a bruised arm, the result of his precipitation, threatens to detain him, he takes with him a young relative, and this time travels post, for his impatience is extreme. "If," said he, "Humboldt should have quitted Paris, the great city would seem a desert to me." He arrives, however, in season, and the two friends meet; but how is time to be found for long conversations? All the saloons are emulous of Humboldt's presence. The interviews, however, take place regularly, only they commence at midnight and do not terminate until morning.

This strain of scientific excitement, added to the cold, renders Von Buch really ill. M. d'Arnim,[*] his young relative, hazards some expressions of blame. "True, it is my own fault," replies the culprit, "the fire of the chimney near which we were talking had gone out and I felt chilled; but by making a movement to rekindle it I should have perhaps hastened Humboldt's departure. I preferred suffering to being deprived of his conversation, and am well content, for I have gained much by it."

Hitherto Von Buch had presented his leading idea of the upheaval of mountains with the reserve distinctive of the conscientious though bold inquirer. In 1822, after a new exploration of the south Tyrol, he shows himself more decided, and in a letter to Humboldt, on that country, has given us his ultimate determination in regard to those great and hazardous questions. Here he pronounces, with an authority which no one as yet had acquired on this subject, that all the projecting masses on our globe owe their present position to an actual upheaval.[†] In this he finds an explanation of the fact, till then inexplicable, that marine shells occur on the summits of the highest mountains; not that the seas have risen to those summits,[‡] it is the mountains which have been raised from the bottom of the seas. Never had a graver difficulty, nor one which longer resisted the efforts of ingenious minds, been solved in a sim-

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[*] I am indebted to M. d'Arnim for most of the private traits of character given in this narrative.

[†] "The pyroxenic porphyries of Fassa owe their actual position to an upheaval. But we must carefully observe that it is not the particular elevation of a rock which is in question, but the lifting up of the whole mass of mountains, and consequently of the entire country."—*Letter to M. de Humboldt*, &c. "It is now many years since I entertained a doubt that the whole chain of the Alps—at least the calcareous Alps—owed its elevation to the pyroxenic formation. This formation breaks the strata which oppose its egress. It pierces or upheaves first the red porphyries, then the sandstones, then the calcareous strata."—*Ibid.*

[‡] "Reflecting on the effects of these upheavals, we shall be less surprised at meeting with petrifactions of anomiæ in the sandstones and calcareous strata at the height of nearly 8,000 feet above the Sasso di Val Fredda. These same petrifactions, which are found at 5,400 feet above the passage of the Caressa, 3,800 feet above Seiss, 2,600 feet above Saint Paul and Caltern, were, perhaps, before the catastrophe of the upheaval, situated lower than the level of the seas."—*Letter to M. de Humboldt.*

pler manner. By reversing the fact and presenting it as it really occurred, the explanation at once presents itself and changes the face of the science.

With Von Buch it was inevitable that one discovery should lead to others. Thus, a first view reveals to him the upheaval of mountains and that of continents; a second, the mechanism of the formation of volcanoes; a third, the relation which connects the displacement of seas with the elevation of mountains. One of his most prolific views, that of the *discordance* of rocks, disclosed to our distinguished colleague, M. Elie de Beaumont, (a geologist who, by his own labors, has united the researches of Cuvier with those of Von Buch,)[*] the first germ of his learned theory of the *relative age* of mountains. We owe still another highly ingenious and novel conception to Von Buch. His explanation of the formation of dolomite,[†] or, more generally, of the alteration produced on deposited and sedimentary rocks by the incandescent rocks of elevation which have traversed them, though still subject to some difficulties,[‡] must always be looked upon as an indication of a high order, and as having marked out for modern geology one of its most important objects, the study of the secondary action of fire on the envelope of the globe.

After so many brilliant labors, the smiling banks of the Spree, with the return of every autumn, continued to recall this eminent and indefatigable man to the quiet retreat which he had chosen. There, a simplicity, the more charming as it was wholly voluntary, presided over the economy of his daily life.

[†] "Cuvier has shown that the surface of the globe has undergone a succession of sudden and violent revolutions. Leopold von Buch has indicated definite and marked differences between the several systems of mountains which diversify the surface of Europe. I attempt nothing but to bring into relation these two orders of ideas."—Elie de Beaumont: *Recherches sur quelques-unes des revolutions de la surface du globe.*

[†] By the *formation of dolomite*, Von Buch designs more precisely *the change of calcareous shell-bearing stone into calcareous magnesian stone.*

"How comes it that the magnesia can pierce, traverse, change the nature of the calcareous beds, which are many thousand feet in height, to make of them a rock uniform in its whole extent? It is a question which I have proposed to myself in all my excursions in the neighborhood of the valley of Fassa, without finding a solution. The calcareous stone does not contain magnesia. It comes, then, from another quarter, and it is quite natural to believe that it is the pyroxene which furnishes it, since magnesia is one of the constituent parts of this substance. I think I have discovered, in the environs of Trento, the process of nature in this operation, and this process has appeared to me so evident that at the instant of the observation I experienced the most lively satisfaction which I have ever felt in my excursions across the Alps."—*Lettre à M. de Humboldt*, &c.

"We can easily conceive that a mountain rent and fissured must lose every appearance of beds; that thousands of channels are opened for the magnesia to introduce itself and combine with the calcareous rock; that by little and little all the mass must change into rhombohedrons; and it is in this way that compact beds, filled with shells, may change into a mass uniform, white, granular, and saccharoidal, without a vestige of organized bodies or any horizontal fissures whatever"—*Ibid.*

"This splitting recalls the phenomena which may be daily observed in limestone furnaces when the fire is withdrawn from them. In going from Cortina, in the valley of Ampezzo, to Toblach, in the Pusterthal, one is surrounded, during the whole transit, by peaks of dolomite. The aspect of these places is so singular that we might think ourselves transported into the midst of an immense furnace. The fragments of dolomite are traversed by immense clefts; they appear rough to the touch, like all substances exposed to the fire. One is tempted to attribute these extraordinary effects to the high temperature which the pyroxenic porphyry had acquired when it penetrated through the inferior strata, and lifted up the dolomite in the form of columns, pyramids, and towers. I am persuaded that this same pyroxenic rock has converted the compact masses into granular masses, that it has caused the disappearance of every vestige of stratification and of organized bodies, and that it has given rise to those fissures which are strewn with crystals. We can no longer doubt that it is the compact limestone, which is constantly found under the dolomite and above the sandstone, that has been whitened, fissured, and transformed into a granulated rock."—*Lettre sur la dolomie du Tyrol à M. Alois de Pfaundler.*

[†] On these difficulties, see the important and ingenious labors of MM. Haidinger and Morlot. [The subject will be found also elaborately discussed in the article on the metamorphism and crystallization of rocks, by Mr. Daubrée, translated for and published in the Smithsonian report for 1861.]

The necessity of peaceful labor, and, therefore, of silence, had induced him to limit his personal retinue to unity, and when age had relaxed the activity of this one faithful domestic, Von Buch, like Leibnitz, had his food brought to him from without. Often his door was opened by himself. If the stranger was one whose presence seemed likely to be importunate, to the question, "Is M. Von Buch at home?" he would quietly reply, "No;" and, closing the door, return to his occupations. The young princes of the royal family were sometimes among those who hazarded the experiment, and their admission was due not so much to their rank as to the affectionate relations which existed between Von Buch and his sovereign, who, among other marks of his favor, had made him one of his chamberlains—a chamberlain, it must be confessed, of very slender assiduity in his office. If the interruption was occasioned by the arrival of a savant, on the very threshold, and without waiting to bid good day, he would encounter the visitor with some such question as this: "Is the *semi-bi-lobate divided ammonite* found also in Thuringia?"

An unappeasable curiosity had directed our geologists' inquiries to that part also of the terrestrial envelope which is traceable to the action of water, and which paleontology had recently occupied in its search for the remains of extinct races.

Since life appeared on the globe, it has undergone many vicissitudes and clothed itself with many forms; different species have succeeded one another, and as each has surrendered its spoils to the cotemporary strata, these relics determine the relative age of the deposits, and the history of life serves to illustrate and complete the history of the globe. Von Buch, after Buffon, aptly compares fossil shells to medals, and adds, in terms of his own, that these medals also have their *language*. In a series of memoirs on the *ammonites*, the *terebratula*, the *productus*, &c., he has taught us the means of interpreting that language; the new and difficult art of distinguishing with certainty the species which identify the several strata, by characters on which he had bestowed the most earnest and profound study. Nor were his efforts for restoring the ancient annals of the world limited to shells; to fossil botany he brought the same aid, a precise determination of characters, which he had conferred on fossil geology; so that the expressive epithet which he gave to certain fossil shells and leaves, calling them *guiding* ones (*conductrices*), might well be transferred to himself. He has truly proved, in these delicate investigations, a guide to other geologists.

But to be an intellectual *guide* did not alone suffice for this good and eminent man. Wherever he could discover young persons whose success seemed only trammelled by the rigors of fortune, he was sure to interpose; and, as if to compensate for the modesty of his own wants, he acted on those occasions with a regal munificence. Such instances were numerous and were seldom made public.

Towards a vessel ready to sail, a young savant was one day directing his steps; his baggage was light, though he had divested himself of his patrimony to procure the means of pursuing his explorations in America. By the wayside a stranger is waiting for him, and says: "A friend, impelled by a desire to promote the progress of science, begs you to employ this in its service;" he places a purse in the hands of the traveller, and disappears. Being once at Bonn, Von Buch received a visit from a youthful professor of that university, who desired letters of recommendation, as he was about to join a scientific expedition. Return to-morrow, replied the distinguished savant. The interval is employed in seeking information. At the hour prescribed, the young man presents himself, the letters are ready, they converse; Von Buch becomes animated, affectionate, gives advice, and finally says to the visitor at taking leave: "I have a service to ask of you." "Compliance will give me pleasure," is the prompt response. "Yes, yes," cries Von Buch, "they all say the same thing, and afterwards complain that I have charged them with commissions which annoy them." The young man protests, cannot conceive how he should be suspected of insincerity and ingratitude. "Very well," replies the adroit interlocutor, "give me your word of honor that you will not even answer me after receiving my commission." The other pledges himself. "Now that I have your word," resumes Von Buch, "here are 2,000 dollars which you are to make use of in your travels." As the injunction did not extend to silence, the recipient felt constrained afterwards to share the secret with others besides his benefactor. A young painter, tormented alike by the fever of art and the anguish of destitution, was languishing at Rome; there was nothing which singled him out but his talent and his misery. Von Buch charges one of the embassies with the remission of a considerable sum; and that the artist may be restrained by delicacy from attempting to penetrate the mystery, he is to be told that it is a family restitution of an ancient date.

As it was one of the chief pleasures of Von Buch's life to restore hope to the unfortunate, so it peculiarly suited his character to act as a peace-maker between the learned when divided in opinion; before all things, however, it was indispensable that science, his sublime mistress, should be treated with the most exact respect. Just and generous in his appreciation of men, he was always zealous in setting forth the merit of the labors of his cotemporaries. A sure and constant friend, though blunt, eccentric, and at times impatient, he was ever ready, if umbrage were taken, to make the advances necessary for conciliation. Among intimates he was fond of recounting the ludicrous mistakes which had been occasioned, during his travels, by the grotesque appearance under which he presented himself.

He loved society, but not what is called the great world. Those who had seen him at court, whither his office, as well as the proprieties of his station in life, sometimes led him, might have thought him drawn thither by his tastes, but his resort even there was to the circles in which intelligence supplied the attraction. In these, the graces of language springing from an active and full mind, re-enforced by a surprising memory, gave to his conversation when he was in the vein a peculiar charm. Polished in the company of females, he knew how to appreciate those who in the courteous collisions of which our saloons are the lists, and which we call conversation, furnish by their sprightly sallies often the best, but certainly the most graceful contingent. This admiration, however, never trenched upon the liberty which he had consecrated to science. Von Buch never married, but, in return, the family affections exercised over him the blandest and most potent influence, and his love for the young, towards whom he could find indulgence for everything but self-sufficiency, prompted many of the actions of his life.

When far advanced in age, he still quitted his domestic roof with the first breath of spring. "I shall travel," was his simple announcement, and a walk would conduct him from Berlin to Dresden, to the surprise of his more sedentary associates in the latter place; thence his course would be prolonged as far as Bohemia or Switzerland. It was when an old man that he scaled the mountain ranges of Greece, seeking among the extinct populations only those which ally themselves with the real world, and finding more attraction and instruction in the chronology of a shell than in all the brilliant fictions which animated Parnassus and Hymettus.

In 1850, a German university having summoned naturalists to a congress intended to celebrate the memory of Werner, Von Buch was present, and of course became the centre of all regards, a tribute which, with an affectionate simplicity, he studiously referred to his early master. "As for myself," he pleasantly said, in allusion to the only official title which he had ever adopted, "I am nothing more than the oldest of the royal pupils of the kingdom of Prussia." His return from this reunion conducted him through the country of his birth, and the view of those fair scenes which he animated with the memories of his youth, plunged him into reverie. It was observed that he passed a long night in deep meditation, in which he seemed to address to the places he was regretfully leaving a touching and silent adieu.

He came once more, however, to visit France, whose genius he loved, and to sit in that Academy to which he prided himself in belonging. He left Paris only in the last days of 1852, and peacefully breathed his last in the spring of 1853.

Von Buch, who had qualified himself for the direct contemplation of nature by always and everywhere pursuing her indications, has left us an example of one of the noblest of scientific careers. He had the happiness to consecrate a long life and a penetrating genius to the profound and unwearied study of one of the highest questions of natural philosophy. Descartes had suspected the igneous origin of the globe;* Leibnitz had inferred its incandescence† from the traces everywhere apparent of a vast pristine fusion; Buffon‡ had demonstrated the existence of the primitive fire, still subsisting, and more and more concentrated in the interior of the earth; Dolomieu§ finally had pronounced before this Academy the words adopted by Lagrange:‖ "This globe, at first incandescent and fluid throughout its whole mass, is still so in its interior, and has nothing solid but its crust;" but no one more contributed than Von Buch to prepare the vast and sublime generalization which dares to place in this profound and central fire, of which, however, he himself has nowhere pronounced the name or fully admitted the idea, the first and sole, the potent and terrible cause of all the revolutions of our globe.

The author thinks it his duty to acknowledge the assistance he has derived, in preparing the above memoir, from the eloquent and learned *Notices* of the great geologist, published in Germany, by MM. Geinitz, professor of the Polytechnic School of Dresden; Cotta, professor of the School of Mines of Freyberg; Dechen, director of mines at Bonn; Noggerath, professor at the University of Bonn, and a fifth, anonymous, pronounced April 6, 1853, before the Geological Society of Germany.

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* "Let us suppose that this earth on which we reside has been once a star composed of matter of the first element absolutely pure, so that it differed in nothing from the sun except in being smaller."—(Descartes: *Les Principes de la Philosophie*, IV part.)

† "It seems that this globe has been once on fire, and that the rocks which form the base of this crust of the earth are scoria remaining from a vast fusion."—(Leibnitz: *Protogæa*, &c.)

‡ "The internal heat of the globe, still actually subsisting, proves to us that the ancient fire which the earth has sustained is not yet by any means entirely extinct; the surface is more cooled down than the interior. Conclusive and repeated experiments assure us that the entire mass of the globe has an inherent heat, altogether independent of that of the sun. This heat we recognize in a palpable manner as soon as we penetrate into the interior of the earth, and it augments in proportion as we descend."—(Buffon: *Epoques de la Nature.*)

§ "While insisting on facts which seem to me of great importance, and again repeating that the unknown cause which produces the fluidity of lavas appears to me to exist under the *consolidated envelope* of the globe, I should add that it is not without design that I employ the expression *consolidated envelope;* for if I cannot doubt that our globe has once been fluid, there is nothing to prove to me that there can be anything consolidated about it but a crust more or less thick; nothing to show that the consolidation, which has been necessarily progressive, has yet attained the centre of this spheroid. I regard the general opinion which ascribes a solid nucleus to our globe as a gratuitous hypothesis, and the opposite hypothesis appears to me much more probable, since with it we can explain a multitude of important facts which, without it, are inexplicable."—(Dolomieu: *Rapport fait à l'Institut national sur ses voyages de l'an* V, VI.—*Journal de Physique*, 1798.)

‖ "The suffrage of the illustrious Lagrange is of too great weight and too flattering not to be insisted on when one has had the good fortune to obtain it. It was not without much timidity and circumspection that I hazarded this hypothesis before my colleagues, when the celebrated geometer, warmly seconding my opinion, asserted that it was not only highly tenable, but that to him it appeared the more probable inasmuch as there seemed to be nothing in direct opposition to it."—(Dolomieu: *Ibid.*)

MEMOIR OF LOUIS JACQUES THENARD.

BY M. FLOURENS,

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TRANSLATED FOR THE SMITHSONIAN INSTITUTION BY C. A. ALEXANDER.

Alchemy, the offspring of man's love for the marvellous and proneness to credulity, and therefore almost as old as the world itself, was introduced into Europe by the Arabs. It promised *riches* and *health:* no wonder it was received with general homage. Its immediate object was that mysterious substance the *philosopher's stone,* by means of which it proposed to effect the transmutation of all metals into gold, to cure all diseases, secure an indefinite term of life, and open for men an intercourse with spiritual beings. Thousands of ardent adepts dedicated their lives to this chimera, one of whom has thus described his fellows: "An eccentric, heteroclite, heterogeneous, anomalous sort of men, possessed of a strange and peculiar taste by which they ingeniously contrive to lose their health, their money, their time, and their life." From the midst of the darkness, however, leaped some vivifying sparks; these indefatigable seekers bequeathed us several enduring acquisitions; it is to them we are indebted for gunpowder, alcohol, the mineral acids and antimony. Roger Bacon, Arnaud de Villeneuve, Raimond Lully, Valentine, Paracelsus, Van Helmont, Becher, are the representatives of this heroic age of chemistry, which recognizes them as its authors.

Absurdity long shackled the progress of the new science. Saint Simon gravely tells us that the Duke of Orleans, "who diligently cultivated chemistry, had used all its resources to get a sight of the *devil,* but without success." That elder age of the alchemists, which had failed in supplying the means for getting sight of the devil, had been followed by one which *did* succeed in getting sight of the Arabian remedies, an achievement, according to Gui Patin, of just as little value. "I have made enemies, he complains, of all the Arabian cooks who, with antimony alone, slay more persons than the King of Sweden has done in Germany." He describes the physician of Cardinal Mazarin as one who "piques himself on three things which no wise man ever did—a knowledge of chemistry, astrology, and the philosopher's stone; it is not with such fine secrets as these that maladies are to be cured." One of these fine *secrets,* however, was destined to make its way in the world. Lemery, arriving at Paris in 1666, attached himself to Glazer, then demonstrator at the Jardin du Roi, as the best source of experiments and analyses. "Unluckily," says Fontenelle, "he found that M. Glazer was a true chemist, full of absurd ideas, and jealous even of these." Quitting him, therefore, Lemery entered himself as master apothecary, inseparable then from the character of chemist, and opened a course of public lectures. "His laboratory," Fontenelle tells us, "was less an apartment than a cavern, which might have been taken for a magician's, lighted as it was only by the glare of furnaces. Yet the resort to it was so great that the operator could scarcely find room for his exhibitions." This course was printed, and as it professed to divulge what was then called the *secrets of chemistry,* the book sold, adds Fontenelle, "like one of gallantry or satire." It is true that, by using intelligible language and precise ideas, Lemery cleared away much that was mysterious and gave an important impulse to chemistry. But a science only acquires consistency when known facts are united by a common bond. This the German physician, Stahl, attempted to effect in regard to the great phenomenon of combustion, and his explanation of that phenomenon, by the disengagement of a principle which he called *phlogiston,* held learned Europe in thrall for fifty years.

This system was overthrown by a Frenchman who, though idly charged with being too much of a financier for a savant, and too much of a savant for a financier, made his own epoch the great epoch of chemistry. Lavoisier began with teaching us that air, the medium in which we live, is composed of two gases, one of which, oxygen, serves for respiration and combustion, while the other, azote, is unsuitable for those purposes. He showed that an animal immersed in oxygen breathes therein with more energy than in common air, but dies if immersed in azote. He demonstrated that combustion can never take place without oxygen; that metals, in calcining, increase in weight, and that they acquire this increase because oxygen unites with them. This theory of combustion, by the decomposition of air and fixation of the oxygen, seemed to leave nothing wanting when the illustrious chemist further evinced that this same oxygen was also the principle of acidification.

Nothing could be more simple and satisfactory than this chain of discoveries. Under the impetus thus given the progress of chemistry became a series of marvels. France must ever mourn the sacrilege which prematurely terminated the life of her gifted son, but the interests of chemistry did not languish in the hands of the Berthollets, the Fourcroys, the Monges. Illustrated every day by some new application, this science rapidly advanced to a popularity which none of its sisters could emulate.

The story is told us that a boyish herdsman one day exclaimed, "Were I Emperor, I would tend my cows on horseback." "And I," rejoined his comrade, "would eat meat three times a week." "For my part," cried the third and youngest, "If such a thing should happen to me, I would be paid thirty farthings a day, that I might give twenty of them to my mother." Animated by some of these primitive and better inspirations, which find no echo in our large cities, three vigorous lads of Champagne were traversing, on a fine morning in spring, one of the great routes which lead to the capital of France. With swelling hearts and light purses they had quitted the paternal roof and the village of La Louptiere, near Nogent sur Seine, and had turned their faces towards Paris, not with a view to make their fortunes there, but from an ambition to add something to the stock of knowledge which they had gathered from the lessons of his reverence, the curate, and father Bardin, then the oracle of the department. One of the three looked forward to nothing less than being physician of his parish; the others proposed to occupy the same field, as apothecaries; the most enterprising of the three thought of adding something to the profits of the laboratory by a small trade in groceries. What justified the more avaricious projects of the latter was the circumstance that his parents, honest tillers of the soil, had lost some moderate resource through the undistinguishing violence of the revolution, and were burdened besides with the support of five other children. The one now departing, moreover, had been ever the ambitious hope of his mother; what more natural than that he should form plans for her gratification.

As our young adventurers neared the great city, the centre of so many illusions, it occurred to the most circumspect of the party that it would not be amiss to scrutinize the resources of their budget. Scrupulously told, the contents could by no dexterity of computation be brought to authorize an outlay of more than sixteen sols (eightpence) a day for each of them. This considera tion determined them to direct their steps to the furthest recesses of the Latin Quarter, and even there it was only in the highest story of one of the buildings that they found the refuge of a common chamber. Under the same roof there happened to be then domiciled a family of those hardy natives of Auvergne, who, that they may some day possess a rood or two of land and be enabled to die among their mountains, distribute for thirty years water and charcoal among the inhabitants of the capital. With the maternal head of this family the young financier, whose thoughtful foresight has been already signalized, opened negotiations for himself and his comrades, and although the difficulties of the situation were avowed with the ingenuousness of seventeen, and the worthy dame could not but feel the risk she incurred in undertaking to provide for the demands of three young stomachs on such scanty resources; although it was now the epoch of "ninety-four," and she a mother, or rather perhaps for that very reason, she agreed to receive them as boarders. Thus were physical needs provided for;

> Food and a shelter; who could ask for more?

It remains to say that the conductor of this negotiation, one of the most critical of his life, who thereby secured himself a footing in Paris, was Louis Jacques Thenard, born May 4, 1777. Once or twice in the beginning of this engagement it happened to him to be too late for the culinary arrangements of mother Bateau. The trying abstinence which such a lapse of attention imposed left its lesson. "I acquired from it," he said in after life, "a habit of punctuality from which I have never deviated, and which adds to the claims of that excellent woman to my grateful remembrance."

Two eminent men were then engaged in teaching chemistry. Fourcroy, by the clearness of his intellect and a ready and learned method, had achieved a success which secured him universal reputation. Vauquelin, less brilliant but more experimentative, had amassed by incessant labor the materials with which he has enriched science. Our young champagnard, all eyes and ears, lost not one of their lessons; he listened and still listened; at length conscientious self-examination satisfied him that he comprehended nothing. At this mortifying discovery, one which the incapable never make, he arrived on a sincere scrutiny of the obstacle at the conclusion, that in a science not purely speculative it is necessary to begin by a practical initiation. Vauquelin, who was then poor, gave admission into his laboratory to such of his scholars as could pay a fee of twenty francs a month, but with such a condition Thenard had no means of complying. Yet here alone could he see any resource, and therefore, taking courage, he presented himself before the professor, candidly disclosed to him at once his penury and his inclination to labor, and entreated to be received, if even on the terms of a domestic assistant. Vauquelin had, however reluctantly, before discarded such offers; the analogy of his own situation at one period did not prevent him from beginning to frame a refusal, when happily the interposing voices of his own sisters, who had entered at the moment and were touched by the mortification, the intelligence and even, through sympathy, by the provincial accent of the young candidate, came to his succor. "Ah, do not send him away; observe how modest, how docile he is; he would not only be useful in the laboratory, but would mind our pot of soup, which most of your dawdlers suffer to spoil by overboiling." Thanks to this lesson in practical chemistry, Thenard was accepted. "I have never been so ungrateful," he used afterwards to say, "as to forget that a pot which is allowed to boil can make but indifferent soup." His rapid intelligence and accommodating nature soon made him a favorite with the youth who frequented the laboratory and procured him at the same time the means of extending the circle of his studies and developing his singular dexterity.

Three years now passed by without bringing any marked alleviation of his condition, but without any abatement on his part of heart or hope. Vauquelin at length procured him a tutorship in an institution, and Thenard, though looking but remotely to the exigencies of a lecturer's chair, felt the necessity of reforming an accent and gesture which reflected the impressions of his native province. For this purpose, as well as from a very decided taste, he attended the theatre as often as his stomach would compromise for an abstinence sufficiently long to justify an expenditure of thirty sols. One morning Vauquelin said to him: "I am summoned to Rouen; my course has commenced; you must occupy my place." Unavoidable deficiencies could not but make themselves perceptible, at the first lecture, to the new professor as well as to his audience, but each succeeding one was marked by so much improvement that, at the fifth, Thenard ventured to cast his eye over the throng and discovered Vauquelin and Fourcroy, in a corner, smiling at his efforts. At the sight he precipitately abdicated the chair. But from that time those eminent men labored in concert for his advancement, and succeeded in securing him an assistant professorship at the Polytechnic School. The earliest accession of a little ease and leisure was but a signal to Thenard for the institution of original researches. Beginning with 1799, when his first Memoir was presented to the Academy, that body has known him, for more than half a century, to lay before it, several times in each year, the results of inquiries which have formed the basis of striking improvements in science, the arts, and industry. Summoned, one day, unexpectedly and not a little surprised, into the presence of the minister of the interior, the latter said to him: "There is a deficiency in the supply of ultramarine blue, which is, besides, always scarce and very dear, and Sèvres stands in need of a material which can resist an intense fire. Here are fifteen hundred francs; go and find me a blue which will answer the required conditions." Thenard began to stammer an excuse. "I have no time to lose," said Chaptal, the minister in question, in a petulant tone. "Go and bring me my blue as soon as possible." In a month from that time the rich tints of the beautiful fabrics of Sèvres bore witness to the success of the chemist.

In 1803, Thenard had shown that the supposed zoonic acid was but an impure acetous acid, and although Berthollet, then in the zenith of his reputation, was the discoverer of this acid, the circumstance produced no change in the generous appreciation which the latter always manifested for his young competitor. Nor was this the only occasion on which Thenard, firm in the expression of his own convictions, was called upon to contravene so imposing an authority. When occupied with the oxidation of metals, he unhesitatingly maintained the idea of oxides in fixed proportions in opposition to Berthollet, who denied it.

Thenard devoted much attention to organic chemistry, and although later inquirers have advanced beyond him, there still remains to his share the merit of having clearly conceived and indicated the relations which connect chemistry with physiology. This science of life rests on an art in which chemistry is pre-eminent, on the high and delicate art of analysis. It was this art which, in its higher and more subtle applications, Condillac first introduced into philosophy, and Lavoisier tells us that he himself derived it from that acute thinker.

In 1807 appeared researches of great interest on ethers. These, it was known, are formed by distilling certain acids with alcohol, and this was all that was known. Thenard announced several new ethers; and, yet more, laid a foundation for the theory of these agents, which have already revealed to us some of their surprising effects on life, and doubtless hold in reserve others more surprising still.

During this period of engrossing application, Thenard was, early one morning, surprised by a visit from Vauquelin. "Up, in all haste," cried the visitor, "and apparel yourself handsomely." Thenard, scarcely awake, asks an explanation. "The law respecting pluralities forces me to resign my chair at the College of France, and I require you to go at once and apply for it." Thenard feels a delicacy. "Come, come," rejoins the professor; "be quick; I have taken the cabriolet by the hour and you ruin me with these delays." The necessary visits being made, Thenard readily secured the position which conduced so much in the end to his extraordinary popularity. The students seemed to attach themselves with peculiar enthusiasm to one raised by toil from their own ranks and wholly unchanged by his elevation. Vauquelin, who continued to watch over his interests, and who greatly admired in Fourcroy the charms of delivery which he himself neglected, would fain have invested his favorite pupil with this additional attraction, and Thenard readily lent himself to the attempt. It was perhaps the only experiment in which he ever failed. In vain did he seek for models in society, counsels from his friends, instructions from our great actors, Molé and Talma; the *champagnard* was destined to bear to the end the original impress, somewhat rough perhaps, but thoroughly French, which definitely consigned him to a type well recognized and not a little vaunted by our national self-esteem.

A few years only separated Thenard from the period when foreign invasion had made it necessary for France to improvise nearly all the resources incident to war. To this end, none had contributed more efficiently than Monge and Berthollet, who afterwards accompanied Napoleon to Egypt, and were often consulted by him when subsequent successes had placed him at the summit of power. "Tell me," he said one day to Laplace, "why it is that I see at present so little of Berthollet?" "My friend," replied Laplace, "has become embarrassed through his undertakings for the advancement of industry, and is chagrined that it should be so." "Tell him to come and see me," said the Emperor. Soon after, seeing his old Egyptian at the extremity of the saloon, he goes directly to him and extends his hand. "Berthollet, you are unhappy, and you do your friends the injustice of not confiding your cares to them: name the sum you require, and think no longer of anything but your researches." Berthollet was then initiating in these researches a young man whose zeal and intelligence rendered him an invaluable assistant in the laboratory. Gay Lussac, in his earliest memoirs, gave evidence of that precision of thought and accuracy of judgment to which in the sequel chemistry has been indebted for so many important services. An analogy of position soon induced between him and Thenard relations of confidence and co-operation, while both were so fortunate as to enjoy the advantages of the scientific retreat which Berthollet had created for himself at Arcueil, and which Laplace often animated by his presence and patronage.

About this period a great sensation was produced in the scientific world. Berzelius had just revealed the power of decomposition exerted by the voltaic pile upon compound bodies. Davy, availing himself of more powerful apparatus, had succeeded in decomposing the two fixed alkalies, which till then had been considered simple bodies: in potash and soda he found, united with oxygen, two metals to which he gave the names of potassium and sodium. He afterwards undertook the analysis of the alkaline earths, each of which afforded a peculiar metal, while in all, oxygen presented itself as a common principle. Proceeding still further, he disclosed, in a paper full of original views, some of the profound relations which connect chemical with electric forces, affinities with electricity. With generous enthusiasm, the Institute of France awarded to this paper the grand prize founded for the progress of galvanism; and though war was raging between the two countries, the English savant was invited to come and receive it in person. This was an act of justice nobly accorded.

"Will you tolerate this triumph of the English?" impatiently demanded Napoleon of Berthollet. A gigantic pile was forthwith constructed by the Emperor's order, and confided to Thenard and Gay Lussac, who soon after were able to announce to the Academy that by means of the ordinary affinities they had succeeded in obtaining new substances more abundantly than by the pile. By employing potassium and sodium, they effected the isolation of a new and simple substance, which they named boron.

Davy recognized the superiority of the chemical method for the extraction of metals; but he claimed this boron as an element which had come to light through his own investigations. This Thenard and Gay Lussac would by no means concede, and they were right; but they maintained at the same time that sodium and potassium, so far from being simple bodies, were combinations of alkalies with hydrogen, or hydrurets. Their English rival justly answered that, if they adhered to this theory, it would follow of course that their simplo principle of boron was but a *hydruret of boric acid*—an argument which remained unanswered. This, however, was the commencement of a discussion which, with profit to science and credit to both countries, continued for not less than five years, and which marks the epoch at which the basis of existing ideas respecting simple bodies was definitely fixed.

In one of the memoirs in which they rendered an account of the different aspects of their controversy with the English savant, Thenard and Gay Lussac had said: "The conjecture is not inadmissible that oxygenated muriatic acid is a simple body." It was not without having first tested this acid with potassium, and strenuously sought to extort some evidence of oxygen, that they gave expression to such an opinion. For, if oxygenated muriatic acid were admitted to be a simple body, a new principle of acidification would be disclosed, and a serious breach be thus made in the theory of Lavoisier. Recoiling from this consequence, and restrained moreover by the immovable opposition of Berthollet, they hesitated to pronounce more decidedly. Hence the recognition which they evaded passed to the credit of England. Davy admitted the oxygenated muriatic acid as a simple substance, giving it the name of *chlorine* or *chlorium*, but at the same time he generously resigned to his two rivals the first indication of the new principle. Thus the grand theory of Lavoisier was subjected to modification, though without forfeiting its title to be considered one of the noblest contributions of French genius to science.

The two friends, whose resources and reputation had been constantly increasing with their labors, had, during this whole controversy, been so completely identified in effort and responsibility, that the learned abroad were disposed to confound them in a single individuality; and indeed the part borne by each remains to this day undetermined. When, in 1809, a course of instruction was opened at the Sorbonne, both were called to participate. Here Thenard proposed to conduct an elementary course, without discontinuing, however, his more abstruse labors at the College of France. So great was the concourse of pupils that space for accommodation was often deficient, and many who had waited long were forced to retire. This suggested to Thenard the propriety of publishing his lectures. They appeared accordingly in four volumes, the first edition in 1813, the sixth in 1836, each edition costing much labor, as the author continued to intercalate the discoveries and doctrines of successive periods. This work maintained an exclusive ascendency in the schools for more than a quarter of a century, so that it may be said that almost all Europe has learned chemistry of Thenard, and doubtless most of the great chemists of the present day, French or foreign, would take pleasure in acknowledging their obligations to his clear and comprehensive method.

When the Institute lost Fourcroy, numerous competitors disputed with Thenard the honor of succeeding him. His friend Gay Lussac had the satisfaction of completing, by his first vote, the unanimity of voices with which his comrade was called to a chair. On this occasion the first impulse of Thenard was one which sprang from his heart. "When I once felt assured of success," he said, "I immediately set out for Louptière, full of the joy which

I should communicate to my mother. To crown my good fortune, I carried with me a book which she had asked me for: The Imitation of Jesus Christ, in large letters, such as she could read without spectacles. When this copy, so rarely to be met with, fell into my hands, I had regarded it as the happiest of my discoveries." At the maternal fireside, the simple habits of his childhood were resumed and old associations cordially refreshed. Here he again listened to the tender counsels of his mother, who, at the moment of parting, said to him: "It is now time for you to marry."

This admonition fell on no unwilling ears. From the time when he first received the patronage of Vauquelin, Thenard had formed the acquaintance of a young chemist, named Humblot, to whom birth and fortune had opened a path as smooth as his own was rugged. In order to sustain the courage of Thenard, Humblot had often cited to him the instance of his own father-in-law, who, at first simply a laborer in a convent garden, had contrived to evince his talent as a painter, and by the opportune development of other talents in the service of his country during the Revolution, had achieved for himself both distinction and fortune; so that it was said of him by a great man, whose confidence he had won: "Conté is capable of creating the arts of France in the midst of the deserts of Arabia." Received into the intimacy of this family, Thenard, whose origin and mediocrity of fortune were well known to them, met with warm sympathy in all his successes; yet was it left to the sagacity of Madame Humblot to divine, which as a daughter of Conté she was well qualified to do, that he was silently waiting for some still greater success in order to acquire the boldness to ask for her daughter—whom Thenard confessed to be for him only too fair and too rich. This obstacle not proving insurmountable, our savant married; and as he was a man who ordered affairs with judgment, and knew how to enter into the details of practical life, he began from that time to build up the large fortune in which were blended the results of his labor, his alliance, and his skilful management.

The constantly increasing success of his lectures had become, with Thenard, the most sensitive test of his self-love. At each of them he seemed to put forth all the ardor of a general on the battle-field; leaving nothing unprovided for, and making but a limited number of experiments, he required them to be exact and striking, and to be presented at the precise moment. The slightest inadvertence or misapprehension on the part of his assistants drew upon them sharp reproofs, and they must have had a hard time of it but for the prompt return of good nature and the acknowledgments which followed. "In a lecture-room," insisted Thenard, "it is the students alone who have a right to be considered; professor, assistants, laboratory, ought all to be sacrificed to them." Before an auditory which had witnessed one of his outbursts, he soothed the not unreasonable susceptibility of him he had maltreated by saying, "Fourcroy has often done the like to me! It produces promptness of apprehension."

It was this same promptness of apprehension which supplied Thenard with one of those penetrating insights which open new horizons to science. The discovery of oxygenated water is recounted by himself in the following terms: "In 1818 I was delivering my first lecture on the salts at the Sorbonne: 'in order that the metals should unite with acids, I was saying it is necessary that they should be oxydized, and that they should be so only to a certain point; when the quantity of oxygen is too great, the oxide loses a part of its affinity.' As an example I was about to cite the deutoxide of barium, when the thought suddenly crossed my mind that the experiment had not been made. As soon as I re-entered the laboratory I called for oxygenated barytes; I diluted chlorhydric acid with ice, adding it in such a manner as to have a liquid at zero. I hydrogenized the barytes and reduced it to the state of paste. I then made the mixture; when, to my great surprise, the barytes dissolved without sensible effervescence. So anomalous a fact could not fail to arrest attention. When I returned for my following lecture, I perceived small globules attached to the sides of the vessel, like those which are seen in a glass filled with champagne wine; bubbles of gas were escaping from the liquid, though quite slowly. I then took a tube closed with the lamp at one of its extremities, and, pouring in some of the liquid, heated it. The bubbles were now rapidly disengaged and gas accumulated in the part of the tube which remained free; I introduced a match and it kindled—there was oxygen present. The hour for my lecture had arrived and I went through with it, but the preoccupation of my mind must have been deplorably apparent."

Thenard had fallen on the traces of a new fact; at first he was disposed to believe that he had made the discovery of suroxygenated acids, but he soon satisfied himself that these acids had no existence. Was it, then, water itself, simple water, which was oxygenized? The idea had scarcely entered his mind before it was proved by experiment, and oxygenated water was thus added to the acquisitions of chemistry.

A new and suggestive fact had been reached by Thenard, the report of which soon spread through scientific Europe. Foreign chemists came to assist in the experiments, and the arrival of Berzelius, at this time, in the French capital, seemed appropriately to welcome the recent discovery. Calling without form on Thenard, the Scandinavian philosopher saw him for the first time; yet these eminent men at once recognize each other, and find themselves, as if in virtue of the law of affinities, converted on the instant into old friends. "I come," said Berzelius, "to gather ideas in the domain of French chemistry, which you have so much aggrandized and enriched. You will, of course, let me see the oxygenated water." The conversation turned on Gay Lussac and his iodine, the new element which that chemist had so distinctly identified; as well as on his cyanogen, a compound substance which affects, in its combinations, all the characters of simple bodies. "We must not forget," said Thenard, "the admirable theory of definite proportions which we owe to you, and which, revealing the immutable laws by which bodies combine, has become the torch of chemistry." "I admit," rejoined Berzelius, "that I have been fortunate. Do you know," he added, "that your recent labors and those of your friend have given Davy occasion to say, 'Thenard and Gay Lussac apart are stronger than Thenard and Gay Lussac united?' " From this conference Thenard proceeded directly to the Sorbonne, and was conducting his lecture with his usual facility, when his eyes casually wandered to a corner of the apartment, and he immediately showed signs of discomposure. The audience, in turn, became uneasy, but Thenard, promptly recovering himself, exclaims: "Gentlemen, you have a right to know the cause of my embarrassment;" and, pointing to a remote part of the amphitheatre, "Gentlemen, there is Berzelius." At once the crowd rises, and a respectful circle surrounds the illustrious stranger with long and rapturous applause. Moved by such proofs of enthusiasm, and forgetting his usual phlegm, the Swede exclaims, as he is borne unresistingly to a seat near the chair: "With such pupils it is impossible to be other than a good professor." He afterwards observed to Thenard, "I had promised myself to verify, in entire secrecy, whether all that fame had taught me respecting your talents as a professor were exact. I find it even below your real merit."

Thenard was now investigating the properties of oxygenated water. One of them is extremely singular; Berzelius named it the catalytic force. Many bodies decompose oxygenated water without undergoing any chemical alteration, without seeming to act otherwise than simply by their presence. The phenomenon, therefore, depends not on the ordinary affinities; nor yet on electricity, so far at least as was apparent, for the most subtle examination had failed to discover the least sign of electrical action.*

Is it due, then, to a new force? So Thenard thought and said. The catalytic force, he believed, would furnish the theoretical bond of a whole class of facts, some of which were already known. As the fear of mistake is always associated, in a practiced mind, with the pleasure of discovery, he called to his aid the counsels of a friend, a bold and sagacious chemist; and the views of Dulong, after mature consideration, coinciding with his own, he might with confidence leave his conclusions to the judgment of after times.

Thenard, associated in 1810, as professor at the Polytechnic School, with the eminent men who shed so bright a lustre on that model institution, thoroughly identified himself with its progress and its benefits; each generation of pupils which he instructed seemed to afford him a new pledge of the perpetuity of his fame. In addition to this appointment, he received in 1814 that of member of the Committee of Consultation for Manufactures; in 1815, he became a member of the Legion of Honor; in 1821, Dean of the Faculty of Sciences; in 1825, he was created Baron by Charles X. Learning that he was about to receive this latter distinction, he demanded, with visible emotion, "Why is not Gay Lussac also named? He deserves it at least as much as I do."

At the moment he forgot, perhaps, that he had once been a courtier, and a skilful one: it had been at the promptings, however, of a kind heart. Few had admired more than he those superb paintings in the cupola of the Pantheon, in which the pencil of Gros has so admirably imbodied the legends of our national history. The enthusiasm of his cotemporaries seemed to guarantee to the artist the admiration of ages to come, when, at the expiration of only a few months, stains of different shapes and colors made their appearance on the surface of the nave, and it became evident that, from moisture having penetrated the stones, this great work of genius was hastening to decay. The mortification of Gros could be consoled neither by the public sympathy nor the real concern of the sovereign, who saw with regret the threatened ruin of a monument, in which a conspicuous place had been allotted to himself. Thenard, between whom and Gros there existed a sincere friendship, no sooner heard of the catastrophe than he commenced in secret a series of experiments, by which he was led to the discovery of a means of rendering the most porous stones impermeable to moisture. Once sure of the result, he repaired to the cabinet of the artist and inquired whether he would repaint the cupola if satisfied that the colors would stand. "Away with you," roughly replied Gros, "and let me hear no more about it." Fourcroy, it will be remembered, had, in the words of Thenard, often done the like to him, so he tranquilly withdrew to his laboratory to await the coming of Gros. This was not long deferred; the door presently opened and the artist inquired, in a voice of anxious emotion, if what had been spoken of were practicable. That evening Thenard was summoned to the Tuilleries, his method explained to the satisfaction of the royal personage, Darcet at his own request was united with him, and he was dismissed with the promise of a grateful requital.

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* See on this subject a very remarkable note of M. Becquerel, *Annales de Chimie et de Physique*, t. XXVIII, p. 19, (1825), entitled: "On the electro-dynamic effects produced during the decomposition of oxygenated water by different bodies." The following is an extract: "M. Thenard discovered that the metals, with the exception of iron, tin, antimony, and tellurium, tend to decompose oxygenated water; that those which are most oxidizable become oxidized, while those which are not so preserve their metallic lusture. It has been observed by M. Becquerel, that during the decomposition of oxygenated water by the sponge of platinum, gold, &c., electrical effects are produced similar to those which would take place if those bodies were chemically attacked by the oxygenated water. He inferred that the decomposition and the chemical action proceed from the same cause; a conclusion which strongly interested M. Thenard." [See on this subject the prize essay from the Holland Academy of Sciences, published in the present Smithsonian report.]

That requital our savant was convinced he should never solicit; but who can count upon anything? One day, at the exit of the students from his lecture, the door is found to be guarded by a force of the police, whose suspicions involve the whole assemblage. Certain fugitives from a popular tumult which had just been quelled had found means to make their way into the hall, and confound themselves among the audience. In the clamor which results the professor is drawn to the spot; the students are at once quiet, but the police refuse to surrender the prisoners. The most that he can obtain is, that those found with notes shall be liberated as students, and others are enlarged on satisfactorily answering some scientific interrogatory which he propounds to them. Fifty, however, of the more unlucky are conducted to prison. At seeing them led away, the heart of Thenard is touched; he hastens to the minister of the interior, but is badly received; to the prefect of police, with no better success. Suddenly a thought crosses him: "They promised me so much on account of the cupola!" Immediately his steps are turned to the Tuilleries, and with difficulty obtaining an audience, he states the case respectfully but warmly; they are his cherished pupils, his children; he will be responsible for them. "Yes," replies the king, with a smile, "but those who are ignorant of chemistry have been put in prison. See my minister, however; the case has not been provided for." At midnight the gates of the prison open before Thenard. "Gentlemen," he cries, "you are at liberty;" then pausing a moment on the threshold, he adds, "On one condition, however—that you will learn chemistry."

Appointed counsellor of the University in 1830, "Thenard," says M. Girardin, "not only rendered to science the great services expected of him, but proved himself an admirable man of business. Severe against abuses and negligence, no one lent himself with more lavish facility to all true reforms. Much as he had to be proud of in this world, I have never known him prouder and happier about anything than the right conduct of the state colleges." For four years he occupied a seat in the Chamber of Deputies, and as he had accepted it with reluctance, so he left it without regret, saying, as he repaired to the scene of rejoicing for the election of his successor, "I am going to assist in celebrating the restoration of my own liberty." His declaration that "he did not meddle with anything but what he thoroughly understood," may be held to have been the rule of his public life. When a member of the higher chamber he moved a revision of the laws of instruction, a reimpression of the works of Laplace, and the national protection of the widows of learned men; he gave also a profound consideration to some of the questions relating to public industry. The spirit of party exercised no dominion over him. Swayed by reason, he set no value on administrative parade, preferring to all other authority that which he exercised as an undoubted master in the domain of science.

During an Academic career of forty-seven years, he constantly yielded a zealous support to whatever views or undertakings appeared to envelope a germ of progress, and there was scarcely one of his colleagues who was not indebted to him for the suffrage of an applauding voice. It was natural that he should cherish a profound regard for the Academy where his fame, his services, and, above all, his habits of conciliation, assured the highest authority to all his expressions of opinion. In private life he cheerfully accepted the obligations of his eminent scientific position, and his house, open to merit of every description, was the abode of amenity and grace. A certain vestige of its rustic origin, a simplicity which recalled the character of our central populations, gave to this amiable household only a new and peculiar charm. In person Thenard was large and vigorous, bearing erect a head covered with a redundance of black hair, with features well marked and animated by an eye of lively intelligence. It was impossible not to recognize in him one of those organizations on which nature has lavished all the elements of a complete existence. That attachments, both of a public and private nature, should gather about one thus constituted, was inevitable; complaisant and just, to him all was easy and simple; neither reproach nor ill-will ever troubled a heart which, more than once, was agitated by the expressions of grateful acknowledgment.

During his lectures at the Polytechnic School, it happened, on one occasion, that something essential to the demonstration was wanting. Thenard impatiently calls for it, and while the attendant runs to seek it, lays his hand, as if to gain time, on a glass, and carries it, without examination, to his lips Having swallowed two mouthfuls, he replaces it, and with entire self-possession observes, "Gentlemen, I have poisoned myself; what I have drunk is corrosive sublimate, and the remedy is the white of eggs; bring me some." The students, to whom his first words had conveyed an electric shudder, precipitate themselves through doors and windows, ransack the neighboring stores and kitchens, and, as each one brings his contribution, soon an immense heap of eggs rises before the professor. In the mean time, one of the students has flown to the Faculty of Medicine, and, interrupting an examination, exclaims, "Quick, a physician! Thenard has poisoned himself at the school in delivering a lecture." Dupuytren rises, seizes a cabriolet on his passage, and rushes with breathless haste to the scene of the accident. But, already, thanks to the albumen, the life of Thenard was saved. Dupuytren, however, insists on the use of a probe, in order to be sure that none of the corrosive substance is absorbed by the stomach. An inflammation of the organ is thereby produced, and Thenard, saved from the poison, is put in danger by the remedy.

During his illness, the students of all the schools manifested the most poignant anxiety; with affectionate zeal they watched around his house night and day, in order to avert every possible cause of disturbance, and listened in uneasy silence for tidings from the interior. Every morning exact bulletins were posted in all the principal establishments, without its being known who were the authors. When Thenard reappeared in his chair at the Sorbonne, the delight manifested was proportionally great. Every one sprang to his feet without seeming to know in what way to express his joy, and the professor for once confessed himself overwhelmed by a torrent of profound and grateful emotions.

It might now have seemed that long years of happiness were in reserve for Thenard, but his fortitude was destined to terrible trials. By a succession of bereavements he lost almost all which could sooth the decline of life: first, his mother-in-law, the early friend who had propitiated his happiness; then the devoted wife who had been its chief dispenser, the latter escaping, by her sudden removal, the pain of seeing their last child expire in the bloom of youth; a brother, a sister, and a nephew followed. When one only and tenderly beloved son remained, the afflicted father exclaimed: "I dare no longer believe in his existence."

The counterpoise which he opposed to these often renewed sorrows was the suggestion of a benign and wise compassion; the foundation of the *Society of the friends of Science* seemed an inspiration from his memories of the past. After bequeathing it a considerable legacy, and associating with it all his friends, Thenard expired June 21, 1857, showing by his latest words that his solicitude still dwelt upon the cherished "Society." "I trust," he said, "that I have formed a union which nothing will ever break. I hope that those who cultivate the sciences, those who are occupied with their application, and even those who only recognize their value, will continue united for their protection." Let the orphan, the widow, the indigent aspirant, salute with grateful accents the tomb of the excellent man whose last thoughts were for them.

MEMOIR OF M. ISIDORE GEOFFROY SAINT HILAIRE.

BY M. DE QUATREFAGES.

[From the Bulletin of the Imperial Society of Acclimatation.—Translated for the Smithsonian Institution.]

Isidore Geoffroy Saint Hilaire was born the 16th December, 1805; and on the 10th November, 1861, he sank under an illness whose insidious progress had set at naught all the efforts of scientific skill and devoted affection, before he had completed his fifty-sixth year. What this short life had been has already been related by those whose eloquence was heightened by grief and friendship, and I have myself said a few words on this subject. What the *man* was has thus been declared, but the appreciation of the *savant* required a little more development. It is for this reason that I return to the theme. I wish to sketch, at least, the principal features of that scientific existence which was cut short at the moment of bearing its finest fruits.

A child of the museum, Isidore Geoffroy took, as we may say, his first steps in that collection founded by his illustrious father, in those galleries which had grown, as if by magic, under the united efforts of the Brogniarts, the Cuviers, the Geoffroys, the Jussieus, the Lamarcks. This daily spectacle would have inspired even an ordinary mind: judge, then, of its effect on an intelligence of early thoughtfulness. To this influence add that of family traditions*—the example and inspiration of a father like Etienne Geoffroy, the lessons of a mother, whose firm and affectionate heart the most bitter trials have never shaken, and whose elevated judgment has always been recognized by some of the greatest minds of our time†—and it will be seen that few men have entered on their intellectual career under more favorable auspices.

Isidore Geoffroy profited by these gifts of Heaven. He was but nineteen, when, in 1824, he made his debut as a zoologist, by the publication of a memoir on a new species of American bat, (*Nyctinomus Braziliensis.*) He afterwards returned at different times to this group, which had first been disentangled by Etienne Geoffroy, and which for that very reason attracted his special attention; but in 1826, at the age of 21, he laid aside for a time these descriptive labors, to turn to a subject much less restricted, and which at once revealed the secret of studies of deep and long continued interest. He published in the *Dictionnaire Classique d'Histoire Naturelle*, and soon afterwards in the form of a volume, *General Considerations on the Class of Mammifers.* Let us dwell a moment on this early work, the first in which Isidore Geoffroy presented a grand general view of facts and ideas. We shall find in it almost all the germs which were to obtain a rich development in his subsequent works.

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* One of the branches of the Geoffroy family gave, in the 18th century, three members to the Academy of Sciences.

† Madame Geoffroy Saint Hilaire, (Pauline Anois) belongs to a family of the magistracy, which still adheres to its old traditions. Her father, M. Briere de Mondetour, was successively Receiver-général des *Economats* under Louis XVI, *Maire* of the 2d *arrondissement* of Paris, and deputy of the corps legislatif, under the Empire. In all these situations, he knew how to merit the esteem of the sovereigns and the respect of the public. In 1804, Mademoiselle Briere de Mondetour married Etienne Geoffroy, who was already celebrated. She survives her husband, the twin daughters, and the son, who were the fruit of this union.

In the description of species, our young naturalist had shown that he could discern and describe with exactness and clearness the most minute characteristic traits. These qualities, so necessary to the zoologist, are seen in the work of which we speak. Already he had shown a sort of innate tendency to ascend from the details to the whole, to connect isolated facts with general principles. For example, in speaking of the caudal development of mammalia, the author does not content himself with noting the very considerable variations presented by the number of vertebræ which compose it. He aims to take account of them, and for this purpose ascends to the phenomena of their first formation. He reminds us that in the human embryo, the coccyx, until the end of the second month, is quite as long as the tail of the dog of similar age. He agrees with M. Serres in attributing to a retreat about the upper part of the spinal marrow, the arrest of development which in the human species intercepted the appendage so prominently developed in the dog. He compares these facts with those presented by the tadpoles of the frog and the toad, and concludes by saying: "Thus the mammal is metamorphosed like the batrachian, and all the changes which surprise us in the latter are not even anomalies; they take place equally in the mammal, and in man himself; and are the general phenomena of embryogeny." All the anatomical and descriptive part of the work is executed in the same spirit.

The hairy coating of mammalia, the variations of color that distinguish races, the influence of domestication on external characters, the result of the crossing of species and races, likewise conduct Isidore Geoffroy to general considerations, the greater part of which had escaped his predecessors. In several passages we see the dawn, more or less advanced, of a great number of ideas which, ripened by reflection and continued study, served as the basis of the great work of which we shall speak hereafter.

An order of considerations which occupies an important place in this work, and which we cannot pass without notice, is that which embraces zoological geography. From the philosophic point of view where the son of Etienne Geoffroy had placed himself at twenty-one, the grandeur and truthfulness of the conceptions of Buffon on this subject could not escape him. We can see that he has been deeply impressed with them; that already he has been seeking to verify them by facts; and that if he undertakes the defence of his illustrious predecessor, it is with a full knowledge of the subject. This enlightened conviction, which Isidore Geoffroy shared with his father, is evinced in many of his other writings. If the unjust prejudices inspired by the Linnæan doctrines, imperfectly understood, have been partly dissipated; if, in our day, naturalists admit Buffon to be still greater as a savant than as a writer, it is certainly in great part due to the efforts of these two penetrating minds, so well formed to comprehend, develop, and inculcate a right appreciation of what had too long been misunderstood in the genius of their predecessor.

The complete list of the works of Isidore Geoffroy, already published in this bulletin, renders it unnecessary for us to enumerate here several treatises of different kinds which succeeded each other rapidly until 1832. We shall merely point out the tendency, more and more marked in their author, to subordinate facts of detail to complete views, and to attach himself to general and philosophic zoology, such as had been comprehended, though from different points of view, by Buffon, Lamarck, and Etienne Geoffroy. These prevailing ideas were manifested officially, as we may say, in a course of lectures given at the Athenæum in 1830, which turned entirely on the fundamental relations of the animal species among themselves and to the external world. By this course of lessons, which had no precedent in public instruction, Isidore Geoffroy began to assume his special place in the phalanx of those who followed the same banner with himself, and was not long in placing himself in their foremost rank. Two years later appeared the first volume of

The General and Particular History of the Anomalies of Organization, (1832.) This time it was no longer a simple memoir, nor a resumé enlightened by new ideas; but a work sufficiently new in substance and in form to found, at once, a whole branch of natural science.

It is well known how much the anomalies of organization, designated by the name of *monstrosities*, have, at all times, struck the imagination of the vulgar and excited the curiosity of the learned. Long regarded as *prodigies*,* they became afterwards *freaks* or *errors of nature*. They were viewed as proofs that the laws governing the formation of living beings might suffer exceptions and infractions. Later, it was understood that physiology was deeply interested in the study of these supposed abnormal beings. But it had required the great progress accomplished during the first years of the present century in anatomy and embryogeny, to demonstrate the extent of the services which the study of *monsters* was to render. Etienne Geoffroy had often insisted on this. Resting partly on the doctrines of his predecessors, but especially strong in his own, he had been the first seriously to take account of the perfectly natural conditions under which these *alterations* are produced. Isidore Geoffroy followed his father in this track. As he says himself, he proposed to make anomalies better known, to trace their characters, their mode of production, their relations, their influence, and thus to lead to the more perfect knowledge of the normal order.†

A single fact will show how completely this multiplex aim of the author has been accomplished. These beings, so various, so complex, which had been considered the product of as many special infractions of general rules, have conformed to all the exigencies of the classification invented for normal beings. Isidore Geoffroy has divided the slightest deformities as well as the greatest monstrosities, those characterized by excessive complication, as well as those resulting from defective parts, into *classes*, *orders*, *families*, and *genera*, as had been done with the mammals or the birds. And this classification has remained unchanged. Some new genera have been added to it, some new species described, but all have fitted naturally into the framework so skilfully traced out by the author between his twenty-sixth and thirtieth year.‡

The importance of this work was immediately understood. The first volume, which appeared in 1832, was a guarantee of those which were to follow. This consideration decided the Academy, and on the 15th April, 1833, Isidore Geoffroy, at the age of twenty-seven, took his seat beside his father in the section of zoology.§

The History of Anomalies was completed, and other labors and publications succeeded. Of these we can notice only a few.

We must first point out the views which Isidore Geoffroy frequently put forth relative to classification. It is well known what importance has justly been attached, since the time of Linnæus, to the forms for the arrangement, in an order determined beforehand, of the numerous beings with which naturalists

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*The Greek and Roman laws condemned to death every child affected with certain organic deviations. In the middle ages it was nearly the same: and even in the 17th century Riolan thought himself very bold, and really was so, in maintaining that they need not be put to death; that it was enough to shut them up.

† *Analytical Notice of the Zoological, Anatomical and Physiological Labors of M. Isidore Geoffroy Saint Hilaire*, 1833.

‡ The third volume of the *History of Anomalies* appeared in 1836.

§ M. Delaunay has preserved the remembrance of an incident which marked this election. Our confrère has recalled, in happy terms, that Mr. Gay Lussac, president of the session, after having counted the votes, yielded the chair to Etienne Geoffroy, then vice-president, in order to give him the happiness of verifying the triumph of his son himself and proclaiming his election. M. Delaunay has well depicted the emotion of the Academy in witnessing the verification of the vote.—(*Funeral of M. Isidore Geoffroy: Discourse of M. Delaunay* in the name of the Faculty of Sciences.)

occupy themselves. We know that these groupings, at first purely *methodic*, became *systematic* in the hands of Jussieu for vegetables, and of Cuvier for animals. But the latter had deeply felt how incomplete are our classifications when we wish to represent the multiplied connexions of living beings.* Isidore Geoffroy had also felt the same, and he tried at least to diminish the imperfections.

Linear classifications, however arranged, never place a being except between two others, that which precedes and that which follows it. Thus they represent only *direct affinities;* they are powerless to represent, even imperfectly, *collateral affinities* or zoological analogies. Now the latter have, in a general view, an importance which must naturally have attracted the special attention of our author. He soon perceived, like some of his predecessors, that the primary zoological groups may be divided into secondary groups, composed of species which correspond to each other, as it were, term by term. He thought, with reason, that these series ought to be represented, and he was thus led to that *parallelic classification* which he has applied especially to the mammifers. However, Isidore Geoffroy, no more than Cuvier, regarded his classification as presenting all the relations of beings with each other. He only saw in it a less imperfect representation of what exists. He has several times expressed himself very clearly on this point, regretting to see his ideas presented in a manner too absolute, by some pupils who had not half understood them. Here, as elsewhere, the master was more cautious than his disciples.

We can only name the volume entitled *Essays on General Zoology,* (1845.) It is less a book than a collection of memoirs on distinct subjects, connected only by the common thought indicated by the title. We shall refer to some of them hereafter.

To construct a *general system of zoology* was, in fact, the constant object of Isidore Geoffroy. It betrayed itself everywhere, and even his public instruction served to manifest it. At the museum, and still more at the Sorbonne, several of his special courses of lectures were partly devoted to the exposition of ideas connected with this purpose, which never quitted him, even when he seemed farthest from it.

But these ideas, as they ripened by incessant study, expanded more and more. He perceived that in general questions, living and organized beings cannot be isolated. "Even at the limits of the animal kingdom, the application of the method remains incomplete, the demonstrations for the most part unfinished, the synthesis only partial."† Thus he was led insensibly, and, as it were, in spite of himself, to publish, not a *general zoology*, but a *General Natural History of the Organic Kingdoms.*

The first volume of this book, which was to be the epitome of the labors of a whole life, appeared in 1854; the second in 1859. The first half of the third volume was published in 1860. This is all that Isidore Geoffroy has been able himself to give to the public. What pious hands have collected will, perhaps, complete this volume and the second part of the work; the rest is forever lost.‡

It is especially for this reason that the premature death of Isidore Geoffroy

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*The formal declarations inserted by Cuvier in one of his last works on the essential distinctions between the *classifications* and *the method*, has too often been forgotten.—(*General History of Fishes*, by MM. Cuvier and Valenciennes.—(*Introduction* by Cuvier.)

†General Natural History of the Organic Kingdoms, preface.

‡This first part extends from Chapter VIII to Chapter XI, inclusive. Besides, the family have found four sheets, entirely corrected and ready for the press; two partially corrected; three sheets in the first draught, and a manuscript reaching to the end of Chapter XIX. We may then expect that the third volume will be completed, and that there will, perhaps, only be wanting the definitive conclusion which an author sometimes reserves till the last moment, till he has reviewed and reconsidered his work. But for the latter parts, the most original of this great work, neither notes nor fragments could be discovered. All was in the head of the author.

is a real misfortune to science. During twenty-six years this man, of the first order of mind, had in view, in all his works of detail, in all his lectures, the development of a class of ideas of the highest importance. We have said already, and repeat here, who shall take up the work? And even though some one should step forward to replace him, can it be hoped that his successor will lay hold of this immense problem with the materials and ability which Isidore Geoffroy had at his command?

As if to increase and justify our regrets, the author places at the head of the first volume an analytic programme of what his book was to be. He divided it into six portions, and we have seen that the second part, at most, will appear. Two-thirds of the work will be forever known to us only by this epitome, the whole of which, representing at least five or six volumes, hardly occupies three pages.

Unfinished, or rather only commenced as it is, the *General Natural History of the Organic Kingdoms* has rendered essential services. Isidore Geoffroy had time to pronounce on some questions which touch on the very foundations of biological sciences, and it is of importance that his judgment on the greater part of them should be known. Heir of Buffon, Lamarck, and of Etienne Geoffroy, having constantly held aloft the banner of the philosophic school, no one can less be suspected than he of having sacrificed to considerations foreign to science. His opinions are the most formal condemnation of certain very ancient doctrines, which some have lately sought to revive in the name of science and philosophy.

Such, for example, is that which puts in doubt the reality of species, by admitting that plants and animals may vary indefinitely, and bring forth series of individuals so distinct as not to be confounded. No one can pronounce against it more clearly than our author. He does more. He shows that in spite of theoretical ideas, *all* serious naturalists have arrived at the same conclusion on this question, as soon as they abandon the vague ground of hypothesis to place themselves on that of facts. He has said, and he could say with reason, that in spite of the profound difference of their general doctrines, Lamarck and Cuvier agreed on this fundamental question. Both, in this, contradicted some of their abstract principles; each had to take some steps towards the truth in an inverse direction. The one had to abandon the theory of *indefinite variability;* the other, that of *absolute fixity.* Thus they met in the belief which was that of the better years of Buffon, that of Isidore Geoffroy: the belief in the *limited variability* of species, the result of which is, that the forms and certain functions may sometimes be modified within very extended limits, while the essence of the being remains unaltered.

In Buffon, no more than in Geoffroy, was this belief the result of mere hypothetical views; in both it was the result of a deep study of facts. The former, before reaching it, had passed through the extreme doctrines indicated above; the latter, enlightened by this example, and taught by what he had under his eyes in the menagerie of the museum, saw the truth from the first, and supported it by new proofs.

Man could not escape the study of the savant who embraced the whole of the animated creation. He was a prominent object of the researches and meditations of Isidore Geoffroy. As early as 1842, in a short article of the *Dictionnaire Universel des Sciences Naturelles,* the author rejected the views generally adopted on the authority of Blumenbach and Cuvier, as to the subject of the relation between man and the lower animals. He insisted that the *order* of bimana should be struck out, as removing us too far from the monkeys, if we see in man only the material being; and bringing us too near them if we regard the whole of human nature. At a later period, in his lectures,* and in his

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* *Lessons on Anthropology,* given at the Faculty of Science, and summed up by M. Devaille, 1856.

General Natural History, he repeated the same criticism, and contended strongly for the admission of the *human kingdom*, first proposed by a Frenchman, the Marquis de Berbençois,* and since adopted by a number of eminent men in Germany and France.

Is this kingdom, like the others, divided into groups distinct and, to a certain degree, independent of each other? Does it contain a great number of species which may be compared to the animal and vegetable species, or else does it include but one, namely, *man?*† We know that this question is still agitated, and even with redoubled ardor. The answer of Isidore Geoffroy is that of Buffon, Cuvier, Etienne Geoffroy Saint Hilaire, Müller, Humboldt, and others. He pronounces in favor of the unity of the human species.‡

The *General Natural History of the Organic Kingdoms* stops at the fundamental principles of biology. The author proposed to present, in the third part, *the general facts relative to organized beings, considered in themselves or in their organs;* the fourth was to be devoted to the *general facts relative to the instincts, the habits, and, more generally, to the exterior vital manifestations of organized beings;* the fifth, to the *general facts relative to the successive and present distribution of organized beings on the surface of the terrestrial globe;* in short, the sixth was to comprise the expositions of what the author calls *natural philosophy*. There he was to show the *convergence of all science towards philosophic unity;* to explain his *views on the totality of organic nature;* to show in the *perpetual changes of the details* and the *permanence of general laws*, whence results *unity through variety;* the *harmonic succession of individual and general phenomena*, which produce *progressive harmony*.§ Certainly no one can glance over this magnificent programme without a feeling of bitter regret. The *History of the Organic Kingdoms* will remain like one of those unfinished edifices whose factitious ruins sadden the mind by merely giving a glimpse of what the edifice would have been, by revealing the grandeur of the plan and the genius of him by whom it was conceived.

There is an additional proof of the intellectual worth of Isidore Geoffroy. Profoundly devoted by sentiment and conviction to the doctrines of Etienne Geoffroy, he had to guard against a very natural inclination to tread too closely in the steps of this venerated guide. While he retained his filial regard and erected monuments of it‖ to his father, no one can mistake him. Isidore Geoffroy was like one of those eminent artists who, after having been a docile pupil of a great master, after having copied his *manner*, have created one in their turn; have conceived and executed works stamped with their own

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* *Journal de Physique*, 1816. M. de Barbençois had called it the *moral kingdom*.—(See *General Natural History of the Organic Kingdoms*.)

† A polygenistic belief had sometimes been attributed to Etienne Geoffroy Saint Hilaire. In the work which he has devoted to the memory of his father, Isidore Geoffroy has warmly protested against this assertion.—(*Life, Labors, and Scientific Doctrine of Etienne Geoffroy Saint Hilaire*, 1847.)

‡ According to the lectures reported by M. Delvaille, Isidore Geoffroy, in 1856, only presented this doctrine as having in its favor the largest share of probability. To judge by the conversations I had with him less than a year before his death, his convictions on this point had become much more decided. Unfortunately, he did not reach this part of his work.

§ Abstract of the Analytic Programme placed at the head of the first volume.

‖ It is well known that the son of Buffon caused to be placed at the town of Montbert, where his father had worked, a column which bore this inscription:

Excelsa turris, humilis columna,
Parenti suo filius Buffon. (1780.)

Isidore Geoffroy conceived the nobler thought of raising to his father a more durable monument by publishing the work entitled *Life, Labors, and Scientific Doctrine of Etienne Geoffroy Saint Hilaire.* The General Natural History of the Organic Kingdoms has as a dedication this verse of Dupoty:

"Même etait fait per moi, cet ovrage est le tien." (Even though done by me, this work is thine.)

genius, and have thus taken rank beside their teacher. We have pointed out what our author was as a man of pure science; it remains to show him under other relations. But here we may be more brief. His labors of practical science are more generally known, and what is important to point out is the filiation, too often unperceived, which unites them to the preceding.

To every one who applies his mind to general questions of zoology, the domestic animals have an importance of the highest order. The extent and number of modifications presented by each of their species at once raise and resolve a crowd of problems which touch on the most delicate questions of physiology, even on the history of man himself. Thus they early attracted the attention of Isidore Geoffroy. We find the proof of this in the article *Mammifers,* in the *Dictionnaire Classique*, which we mentioned above, and still more in the *Essays on General Zoology*. We find, amongst others, a memoir relative to the *possibility of elucidating the natural history of man by the study of the domestic animals.*[*] The author examines at first the analogy which exist between the variations of the domestic animals and those of the human races, and points out the close connexions presented by these two orders of facts. Then he shows how the determination of the original country of a domestic species may throw light on the history of the migrations of a people. These ideas were to be afterwards extended and completed.[†]

In the same volume we find a treatise on the domestication of animals,[‡] which was the first step in a path in which the author was so greatly to distinguish himself. This simple memoir furnished the basis of labors more and more multiplied and important, and was transformed into an octavo volume of more than five hundred pages, entitled *Domestication and Acclimatation of the Useful Animals*. We know, also, that the ideas put forth by the author of this book have assumed a concrete form, and have been reduced to practice by the foundation of the *Society of Acclimatation* and the creation of the *Garden of Acclimatation*. These two establishments are truly the works of Isidore Geoffroy, and should rank amongst his best. If, in their beginning, they excited some distrust and some raillery, the first of these sunk under the conciliating and prudent direction of the founder; the second disappeared before established facts. Thus their progress was rapid, their future soon assured. Isidore Geoffroy thus left, beside his books, two institutions not less durable than his fame; and if his loss excited uneasiness in the minds of those who had adopted his plans, it was soon dissipated by the choice of his successor.[§]

The results of the publications of Isidore Geoffroy on practical zoology, in the foundation of the two organizations which I have just named, deserve to be pointed out no less for their immediate and visible effects than for the influence which they have already exercised, and which must continue to be more and more felt. Hitherto the natural sciences, zoology especially, had been somewhat despised by those who claim the title of *practical men.* They were merely considered as a species of knowledge calculated to amuse the mind, but without practical utility. For this cause they were rejected, as were chemistry and geology by the metallurgists and miners of the last century. Thanks to Isidore Geoffroy, and to the movement which he originated, these prejudices begin to be dissipated; they may, perhaps, disappear slowly, but certainly it will be at length understood that zoology has also its applications; that it ought to be to the breeding of animals, and to all that we procure from them,

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[*] This treatise, an abstract of which was communicated to the Society of Natural Sciences in 1835, had been read to the Academy in 1839. It figures in the *Reports*.

[†] *General Natural History of the Organic Kingdoms.*

[‡] This work had appeared, but less complete, in the *Encyclopédie Nouvelle.*

[§] It is known that this successor is M. Drouyn de l'Huys.

that which the physico-chemical sciences are to operations upon brute matter. It is true, Buffon, and especially Daubenton, had acted upon this idea; but, less fortunate than their successor, and perhaps beginning too early, they left no real impress on the minds of the people. It will not be so with the work of Isidore Geoffroy; and this is one of the special results of that life so well occupied. Here, as everywhere, pure science appears *the mother of practical science*—a mother fruitful in proportion as she is exact and elevated.

In speaking of the works of Isidore Geoffroy, M. Milne Edwards has said: "All display a profound erudition and bear the stamp of a mind wise, elevated, and generalizing, and the purity and elegance of the style enhance their merits."* In speaking of his colleague's public instruction at the Faculty of Sciences, M. Delaunay expressed himself in a similar manner: "M. Isidore Geoffroy," says he, "was a most distinguished professor. He had an easy elocution, and expressed himself with graceful simplicity, without any pretension to eloquence, and captivated the attention of his audience at once by the clearness of his explanations, and by the art with which he could group isolated facts around the principal ideas which he sought to illustrate."†

These appreciations are just, and they characterize well the eminent man of whom we speak. His lucid mind embraced at once his whole subject; consequently his ideas arose logically one from the other, and, as it were, co-ordered themselves. His words translated faithfully his thoughts, clearness of expression only reflecting clearness of conception. Thus his speech kindled at times, and he never wanted striking images and happy comparisons to render the most comprehensive or profound ideas, becoming thus an orator without effort. His lectures were always as well attended as his works were widely read.

It was at the museum, in 1829, that Isidore Geoffroy, at the age of twenty-four, first appeared as a professor. He was then an assistant to his father, and took ornithology as the subject of his lessons. The following year he delivered, at the Athenæum, the remarkable course of lectures of which we have already spoken. Having been appointed, in 1837, assistant to his father at the Faculty of Sciences in Paris, he soon quitted this temporary chair to go to Bordeaux, with the title of dean, to organize the Faculty of Sciences created in that city, (1838.) But this task finished, he returned to Paris, and in 1840 was named inspector of the Academy, and charged with the duty of inspector general of the University. At the same time that he fulfilled these high functions, in which civil administration is so closely connected with the best interests of science, he replaced his father at the museum, the latter being struck with blindness, as had been Lamarck and Savigny before him. In short, in 1841, this position having been made permanent, the veteran of science yielded the place to the young soldier whom he had trained; and Isidore Geoffroy received, during his father's life, the succession which, for a long time previously, he had in great measure administered.‡

In effect, from 1824, the youthful savant had discharged the duties of assistant naturalist at the Jardin des Plantes. In this office he had to superintend and direct not only the collections of mammals and birds, but also the menagerie, founded by Etienne Geoffroy, (1793.) He had devoted himself, heart and soul, to this double task; but he became, perhaps, still more earnest in it when he was made the official chief of this very important part of the museum. All those who have seen him at work know with what steady ardor he labored to

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[*] *Funeral of M. Isidore Geoffroy Saint Hilaire. Discourse of M. Milne Edwards, president of the Academy of Sciences.*

[†] *Funeral of M. Isidore Geoffroy Saint Hilaire. Discourse of M. Delaunay*, in the name of the Faculty of Sciences.

[‡] In 1844 he became titular inspector general, and he exercised the functions up to the time when he replaced M. de Blainville as professor of zoology at the Faculty of Sciences, (1850.)

enrich these collections of dead and living animals. The galleries soon became too small to contain all that Isidore Geoffroy procured for them; sometimes by using the slender resources which the too scanty funds of the museum placed at his disposal; sometimes by availing himself of the authority of the establishment and his own personal influence.* At a time when it was the fashion to find fault with all that concerned the museum, objection was often made to the *crowded condition of the cases*, and the professor who had the administration of this portion of our riches was reproached for neglect. It was forgotten that this was the most striking proof of his exertions, for had he been less industrious, the localities sufficiently capacious for his predecessors would have sufficed for him.

What we have just said of the galleries applies equally to the menagerie. But could it be otherwise? The number of living specimens collected in the parks of the museum was tripled in twenty-five years,† while the disposable space remained almost the same. What minute and constant care did it not require to utilize this ground so parsimoniously granted to the first scientific menagerie ever formed; to struggle against conditions often deplorable; to meet expenses constantly increasing, even when the budget was restricted in consequence of political events. It was in the midst of such difficulties that Isidore Geoffroy succeeded in developing the noble creation of his father, and in making it serve the advancement of pure as well as practical science.

Some of the first acclimatations attempted in our age have been produced in this menagerie. I need only recall the Egyptian goose.‡ It is known that this fine species, brought to France by Etienne Geoffroy, and since then almost constantly reared in the museum, has furnished, for the first time, a race truly European, characterized by an increase of size and a change of color, but especially by a delay of about four months in the time of laying its eggs—a delay which brings the mother and her young into harmony with the new climate to which they are subjected. We may also mention, in passing, the yaks, three of which, arriving at the museum in 1854, have increased to a herd of more than twenty head, without the death of a single one, young or old.

But let us dwell a little on the part which the menagerie, often considered as only fit to satisfy a useless curiosity, has furnished to the scientific labor of Isidore Geoffroy. This collection of living animals was to him a field of continual experiment, and he owed to it the solution of some of the most delicate questions of physiology and of general zoology. It was by means of it that he was able to vanquish the greatest, one may say the last, difficulties opposed by Cuvier, Blainville, and other naturalists, to the opinion of Guldenstädt and Pallas, as to the filiation which connects the jackal with our domestic dog. It was from it that he sought instruction on the fecundity of metis and hybrids. It is to the facts collected in this enclosure, and appreciated with rare clearness of judgment which none could fail to recognize in him, that he was indebted for avoiding, in the solution of such delicate questions, the opposite exaggerations which have alternately reigned under the sanction of great names in science.

The museum, with its its galleries and its menagerie—the Society of Acclimatation and the garden of the Bois de Boulogne—formed the world in which

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*In 1828 there were at the museum 7,500 stuffed birds and mammals; in 1835, 11,750; in 1861, 15,500. Besides the magazines contained at the latter period, 12,000 skins in a perfect state of preservation.

†In 1824 the menagerie possessed 283 birds or mammals; in 1842, 420; from 1850 to 1861, 900, on an average.

‡The Egyptian goose lays naturally about the end of December. Those reared at the museum for some generations laid, in 1844, in February; in 1846, in March; and since then, in April. (*Acclimatation and Domestication of the Useful Animals.*) It is one of the most striking examples that can be quoted of the *influence of the surrounding medium.*

the life of Isidore Geoffroy was passed; a world very small, to judge by its extent, but very large to him who could see in it an epitome of the living creation; and our lamented compatriot found it extensive enough to exercise all the faculties of his mind and the peculiarities of his character.*

The books of which we have sketched the tendency and the results attest the activity of his mind, while his character was not less seen in the mixture of firmness and gentleness with which he exercised his functions. At the institute and the museum his word had always a real authority over his colleagues. At the Society of Acclimatation and elsewhere many thought they followed the inspiration of their own minds when they only yielded to an influence that could hide itself, the better to reach a desired end. Everywhere—and this is not his least praise—his subordinates cheerfully obeyed his orders, always clear and precise, or accepted his decisions, dictated by kindness and justice.

Isidore Geoffroy had known all the joys of the heart, and he had felt, too, all its sorrows. A twin sister, worthy to understand him, had been the companion of his childhood.† At the age of twenty-five he married Mademoiselle Louise Blagne‡; and all who have known this young lady remember her cultivated mind, affectionate disposition, and graceful manners. A son and daughter, the fruits of this union, brought the gaiety of childhood into this united family, over which was shed the influence of the gentle good sense of his mother and the brilliant glory of his father, while he himself was becoming daily more distinguished. It was a happiness too complete to last. Etienne Geoffroy lost his sight in 1840, and although the patient resignation of this martyr of science§ tended to soften to his family the pain of this trial, all must have felt that their happiest days were over. The head of the family passed away the 19th June, 1844, and shortly after Madame Isidore Geoffroy was seized with one of those incurable maladies which wear out slowly the springs of life. Her husband had long to endure the anguish of witnessing the suffering of a beloved being whose days are numbered. She died the 20th November, 1855. The sister forgot her own griefs in consoling her brother and mother. She, also, died, almost unexpectedly, the 13th June, 1860.

One should have known Isidore Geoffroy intimately to comprehend the effect produced by these successive losses on this savant, apparently so calm and even cold, but, in reality, so warm and loving. "This poor, torn heart felt incessantly the loneliness of its home, the absence of that which comforts, tranquillizes, vivifies." Everywhere he found "the bitterness of regret, the remembrance of happiness once possessed."|| To escape, at times, these heart-rending thoughts, he plunged into study with a sort of desperation. He accepted, on

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* In speaking of the wonderful development of the collections of this establishment, Etienne Geoffroy had said: "The museum is becoming a Noah's ark." *(Progressive Studies of a Naturalist.)*

† Isidore Geoffroy had had twin sisters. M. Mademoiselle Anaïs Geoffroy Saint Hilaire died at the age of 19. The other, Madame Stephanie Geoffroy Saint Hilaire, survived till June, 1860. The pen of a friend has rendered a just and pious homage to this lady, whom it is impossible to know without loving, and who had herself known every grief that could afflict a delicate and affectionate heart. *(Les puit de la douleur,* by Madame Marie Pape Carpentier.)

‡ March 20, 1830.

§ During his abode in Egypt, and in consequence of hard work, Geoffroy Saint Hilaire had been seized with ophthalmia, and had remained twenty-nine days without sight. "I shall become blind again in my old age," he had often said. He did not the less continue to write great part of the night, reserving the day for his researches. This habit certainly contributed much to bring on the infirmity, the anticipation of which never stopped him for a single moment.

|| Sixth annual session of the Society of Acclimatation. *Discourse* of M. Drouyn de l'Huys, president. *(Bulletine de la Societé.)* If other orators here dwelt more on the merits of Isidore Geoffroy as a *savant,* none has so well displayed the amiable qualities of the *man.*

all sides, the occupations which presented themselves to him, and found time for all. He felt, indeed, that his life was at stake in this struggle of grief against labor. "Can the bow be always bent and not break?"* said he; and at that moment he doubtless felt the attacks of the malady which was to carry him off, but he did not the less continue this consuming strife.

He tells us himself that from the month of November, 1860, he had felt his head much fatigued, and had sometimes been obliged entirely to suspend his labors. These symptoms returned in the course of the summer of 1861 in a more threatening manner. By the advice of his physicians he set out for Switzerland, in the course of September, in the hope of regaining the strength which seemed failing him, and he even went as far as Italy. Returning about the middle of October, he thought himself able to resume his studies, but his weakness soon reappeared. Some uncertain periodical symptoms having given the idea of an influence of malaria, he was advised change of air, and went to Neuilly.† He remained there scarcely a fortnight, when, his condition becoming worse, he was taken back to Paris, only to lay himself upon a bed from which he never rose again; and in that house, in the museum which, for sixty years, had attracted the most distinguished men of all countries, there remained only a widowed mother mourning over all that she had loved. May the veneration with which she is regarded, and that which is attached to the memory of those who were so dear to her—may the affectionate attentions of the young family which has gathered around her mitigate her sorrows.‡

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* These expressions occur in a letter of Isidore Geoffroy quoted by M. Drouyn de l'Huys.

† M. Albert Geoffroy Saint Hilaire, first director of the *Jardin Zoologique d'Acclimatation*, displays, in developing and carrying on this establishment, the same intelligence and filial devotion which animated his father, where, at about the same age, he labored in the menagerie of the museum.

‡ Madame Geoffroy Saint Hilaire still occupies the home where she has lived since 1804. The ministerial decision which assigns it to her has only ratified the universal wish of the former colleagues of Etienne and Isidore Geoffroy. The daughter of the latter, and his son-in-law, M. d'Audrey, have come to live with their grandmother, bringing with them her great-grandchildren.

THE CATALYTIC FORCE,

OR

STUDIES ON THE PHENOMENA OF CONTACT.

A PRIZE MEMOIR, BY T. L. PHIPSON, D. S.—TRANSLATED FOR THE SMITHSONIAN INSTITUTION BY C. A. ALEXANDER.

QUESTION. "The existence of what is called the catalytic force, in which the explanation of many phenomena has been heretofore sought, having become more and more doubtful, the society desires a rigorous examination of the phenomena which some savans continue to explain by that force."—*Question proposed by the Societe Hollandaise des Sciences. Haarlem*, 1858.

The phenomena attributed to the catalytic force, such as the incandescence of platina in the vapor of alcohol, the union of oxygen and hydrogen by means of platina, of pumice-stone, of humus, &c., fermentation, eremacausis, putrefaction, &c., &c., are extremely common, and occupy a prominent place in chemical studies. At first glance they all appear to offer an exception to the general laws of chemical affinity. There is, however, nothing of the kind; and these phenomena which are so often met with, and which, so to speak, have become general, afford, when we rigorously examine them, the same character of every other chemical reaction.

One thing which has especially conducted me towards what I regard as the true interpretation of catalytic phenomena, is the study which has been recently made of ozone and the allotropic conditions of certain bodies other than oxygen. The production of ozone by the contact of phosphorus and moist air may be regarded, with the combination of hydrogen and oxygen by platina, as the type of catalytic phenomena.

The experiments of MM. Marignac, Fremy, Becquerel, Andrews, &c., &c., prove, in an unexceptionable manner, that ozone is pure oxygen, as the diamond is pure carbon. If, then, we could succeed in determining by experiment the modifications which the molecule of oxygen undergoes in passing to the state of ozone, an important step would be taken towards the exact interpretation of all the phenomena of contact.

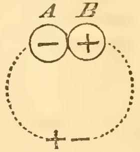
The properties of ozone, when compared with those of oxygen, are extremely remarkable and too well known to chemists to require full recapitulation here. It will suffice, I think, to say that ozone is to oxygen what oxygenated water is to water—it is oxygen whose polarity is the most decided possible; that is, whose tendency to combine with other bodies is developed in the highest degree.

We must dwell a little on the phenomenon of polarity. Let us consider what chemists understand by this word. When two different bodies are about to combine, we know that one passes into the electro-positive state, the other becoming, at the same time, electro-negative. Thus, when we add a *base* to an *acid*, the base becomes electro-positive, the acid electro-negative; and when the combination is effected, the electricities of opposite names are given off. It is then well established, at the present time, that a molecule A, on the point of combining with a molecule B, takes an electricity opposite to that of the latter.

Fig. 1.

We call this *polarization;* the molecules seem to assume *poles.* The combination completed, the opposite electricities are given off, as the second figure shows, and, by intercepting them with a galvanometer, we identify their nature; we prove, by direct experiment, that electro-*negative* bodies, like the acids, give off the positive electricity, while the bases or electro-*positive* bodies, under these circumstances, give off the negative electricity.

Fig. 2.



This principle governs all chemical reaction, however complicated it may appear. And whatever may be the modifications recently introduced into our ideas respecting light, heat, electricity, affinity, &c., by the imposing doctrine of the *correlation of physical forces,* we shall continue, in speaking chemically, to consider chemical reaction, the combination and decomposition of bodies, in this manner. For, without affirming that this is, at the present epoch of science, the most rational manner of consideration, we are able, in this way, not only to present a theory easily apprehended and borne out by all experiments, but have it further in our power to explain, to a certain point, by a profound study of the phenomenon of polarity, almost all the facts thus far observed in the vast domain of chemistry.

I shall not inquire, in this place, whether chemical affinity be a force apart *sui generis,* or a necessary effect of the polarization of molecules. Something will be said on this subject towards the close of this memoir. I affirm only that the phenomenon which we call *polarity* accompanies and controls all combinations and decompositions of bodies.

This being premised, it still remains to demonstrate that the polarity of a body is not fixed; that it varies according to the bodies with which it is brought into contact. To cite an example familiar to all my readers, chlorine, one of the most electro-negative of metaloids, becomes electro-positive in its relations with organic bodies, and even in relation to oxygen, under certain circumstances. What is thus said of chlorine is also applicable to iodine, bromine, fluorine, &c.; in a word, to all analogous bodies. Sulphur, agreeably to the experiments of M. Berthelot, is sometimes positive, sometimes negative, according to circumstances. Chlorine, in the chlorides, is electro-negative; in the chlorates, it is electro-positive; and in them the nitrate of silver does not evince its presence, &c., &c. Thus we find that the polarity of a body changes according to the bodies with which we place it in contact. The change consists in the body submitted to experiment becoming electro-negative or electro-positive, from *having previously been electro-positive or electro-negative.*

But we can modify in a different manner the polarity of a body; that is, we can render it *more or less electro-positive, more or less electro-negative,* and that without the body entering into combination with one of those which induce this change. Thus, by the contact of a stick of phosphorus, the polarity of the oxygen contained in a volume of humid air passes into an allotropic state—into ozone. Its polarity is thereby modified to an extraordinary degree; it is much more electro-negative than in other circumstances. If we decompose water by means of the pile, and collect the oxygen produced in the pores of a charcoal pole, this oxygen is modified in the same manner.—(*Osann.*) If we disengage it from the bi-oxide of baryta by sulphuric acid, at the ordinary temperature, the effect is the same.—*(Houzeau.)* If, observing certain precautions, we disengage it by heat from the peroxide of lead, from the oxide of mercury, &c., we find that a certain quantity is in the state of ozone.—*(Schönbein.)* When ordinary oxygen enters into combination with certain organic bodies, it passes into the state of ozone.—*(Schönbein, Kuhlmann,* and *Phipson.)* Not needlessly to multiply these examples, we shall only say that the same is the case with chlorine, with bromine, *(Andrews,)* and with hydrogen, *(Osann,)* as shown by experiments results of which can bear but one interpretation. Ordinary hydrogen, disengaged from water by zinc and sulphuric acid, exerts no action on a solution of sulphate of silver; the hydrogen disengaged from water by the pile decomposes that salt.

Electric action is peculiarly effective in augmenting or diminishing the polarity of a body. Thus oxygen may be converted to the state of ozone by the electric spark. Let a piece of copper and one of tin be placed in two capsules of porcelain, and an acid be poured on the two metals; they will be attacked very nearly in an equal degree. But if the tin be placed in the same capsule, and in contact with the copper, and the acid be then added, the tin *alone* will be attacked, and more actively than in the first case, because its polarity on account of its contact with the copper has become greater than that of this metal, and the acid will not act on the copper until all the tin has been dissolved.

We particularly desire our readers to bear this experiment in mind, for what we shall adduce in the sequel for the purpose of demonstrating what passes in catalytic reactions will be, in effect, but little else than modifications of that which has just been cited..

When the polarity of a body is strongly modified, such body is styled "allotropic," (from *αλλοτροπος*, of *different nature.)* Thus ozone is allotropic oxygen; the chlorine of Andrews is allotropic chlorine; the hydrogen produced by the pile, and which decomposes sulphate of silver, is allotropic hydrogen, &c.

But this *allotropic state* of bodies is but slightly permanent, as will be hereafter seen; and, further, we shall endeavor to prove that bodies assume this state at the moment when they enter into combination. In regard to this I shall first cite my own experiments and observations on oxygen. M. Schönbein having expressed an opinion that the action of oxygen on certain organic bodies, such as venous blood, &c., depends on the presence of a substance capable of transforming oxygen into ozone, I was led to study this allotropic modification of oxygen under the same point of view.

First, I repeated and confirmed the ingenious experiments of the learned chemist of Basle, in which he studied the action of ozone in the mushroom; and these studies appear to me to cast light on the subject with which we are occupied It is known that the flesh of certain species of the mushroom and boletus (among others the *Boletus luridus*, *B. cyanescenus*, &c.) possesses the remarkable property of changing color when broken and exposed to the atmosphere. For instance, the internal tissue of the *boletus luridus* becomes blue under these circumstances. Now, experiment shows us that this boletus contains a colorless, resinous principle, soluble in alcohol, and this alcoholic solution is affected, as regards oxygen and ozone, in the same way with the alcoholic solution of the resin of guaiacum, which has been employed by M. Schönbein and myself in some experiments as a test for ozone. This resinous principle, which is separated from the boletus by means of alcohol, can assume no color spontaneously from the air when it is separated from the mushroom; but in the parenchyma of the vegetable it promptly becomes blue on the least contact with oxygen. Experiment further proves that the expressed juice of several mushrooms contains an organic principle possessing the property of transforming oxygen into ozone, so that, by causing a current of air to pass through this juice, a certain quantity of oxygen is retained in the liquid in the state of ozone, the presence of which is indicated by mixing with the liquid a little of the alcoholic solution of guaiacum, or that of the colorless resin of the boletus. If the liquid contains no ozone, no coloration occurs; if ozone be present, a deep blue tint is obtained.

I have repeated experiments of this kind upon a great number of different mushrooms, and on the juice of different phanerogamous plants. I have found that the *peculiar matter* which transforms oxygen into ozone may exist in mushrooms, where the colorless resin of the boletus tribe does not occur; the peculiar matter and the resin may be wanting in certain other mushrooms, but the first may be formed in the expressed juice after a certain lapse of time. I entertained no doubt, therefore, that this peculiar matter, which effects the transformation of oxygen into ozone, is of the nature of ferments. I have verified its presence in all phanerogamous plants whose juice gives a blue color with the resin of guaiacum. It seemed desirable, furthermore, to determine whether the *organic acids* had anything to do with this color, for some of these acids react, we know, on pectose, starch, sugar, &c., as *yeast* does. My experiments have, however, satisfied me that the organic acids have no immediate action on an alcoholic solution of guaiacum; that these acids do not produce a blue color in the tincture of guaiacum when alone or when previously mixed with the liquid obtained from mushrooms, the juice of which does not act upon this reagent. On the contrary, certain acids, for example tartaric, citric, and oxalic prevent the coloration of the juice of certain mushrooms, whose juices act on the alcoholic solution of guaiacum when these acids are not present. Although nature then appears sometimes to employ these acids for inducing metamorphoses, it would seem that they do not act in this case.

From these facts I have come to the conclusion that the peculiar matter spoken of above must be of the nature of a ferment, and the experiments of M. Schacht lend support to my opinion; for that chemist believes that the juice of phanerogamous plants owes its property of turning the resin of guaiacum blue to the presence of what he calls vegetable gelatine, and he affirms that this substance no longer acts when boiled.

Among my experiments of this kind I will cite one which is easy to repeat. We know with what facility a slice of apple changes in the air; its surface becomes brown in the course of a few minutes. This coloration is known to be owing to the action of oxygen; it is a commencement of eremacausis.

Now I think I have proved, as will be seen in the sequel, that when oxygen acts thus on organic bodies, producing the phenomenon known by the name of *eremacausis* or *slow combustion*, this oxygen is always in the state of ozone at the moment when it reacts. To verify this with the slice of apple, we need only diffuse over the fruit a drop or two of the tincture of guaiacum; at the end of three or four seconds the reagent changes to a bright blue color. Let it be observed that this tincture, exposed to the air on glass, porcelain, clean paper, &c., will not change color for a long time; and, moreover, as has been already seen, the organic acids are inactive in this phenomenon.

I have carried my experiments further, and have arrived at the conclusion that the first phase of all *fermentation* or *eremacausis by the influence of the air* consists in the transformation of the oxygen of the air into ozone. Thus, as in the most ordinary cases of inorganic mineral chemistry, all commences with a phenomenon of polarity.

What has just been said is particularly striking, as regards the phenomena presented by the *resinification* or oxidation of certain *essential oils*. The extraordinary manner in which the oil of turpentine (térébenthine) exalts the activity of oxygen is now known to all chemists. The aerated turpentine transforms sulphurous acid into sulphuric acid, the oxide of lead into peroxide, arsenious into arsenic acid, &c.—*(Kuhlmann.)* Other essences act in the same manner on oxygen by transforming it into ozone; and in my experiments I have never yet met with an essential oil which did not exercise this property.

Still further, in examining different fats and fixed oils, I have been convinced that they possess, in a certain degree, as regards oxygen, the same properties with the essence of turpentine and the volatile oils.

The substances with which I have most frequently experimented are crude turpentine, the extract of bitter almonds, of cinnamon, of caraway, the balsam of Peru, most of which were acid and reacted on litmus paper. They produced, however, no effect on the ozonoscopic paper until an absorption of oxygen had taken place.

If we place a very small quantity of one of these bodies on ozonoscopic paper, and expose the whole to the air, we shall perceive that at the end of a quarter of an hour the paper begins to be colored, and the coloration proceeding from the deposit extends itself further and further. The same thing takes place with solid fats, stearine, margarine, &c., and with the fixed oils. One of the substances which most readily undergoes eremacausis is liquid sugar, especially if it has been extracted from honey. If we place some of this sugar, already embrowned by the action of oxygen, at the bottom of a flask, and suspend from the stopper an ozonoscopic paper, this last becomes colored in a very short time.* There are reasons, also, for thinking that starch itself, under certain circumstances, transforms oxygen into ozone, and here lies, in my opinion, the cause of the discussions which have lately arisen on the efficacy of this paper as a reagent for ozone; ioduretted starch, when dry and placed beyond the influence of disturbing agents, such as hyponitric acid, &c., is an excellent test for ozone; but ioduretted starch, wet and exposed to the solar light, can furnish only doubtful results, to say the least, for, in undergoing eremacausis, it should transform oxygen into ozone like other organic bodies. There can never be a doubt of the presence or absence of ozone when we employ mineral reagents, such as sulphurous acid, the black sulphuret of lead, which ozone immediately transforms into sulphuric acid and into white sulphate of lead; or even with the blue sulphate of indigo, with which ozone produces colorless *isatine.*

The ethers and the alcohols while absorbing oxygen also transform it into ozone. Sulphuric ether, in this instance, promptly discharges the color from a solution of indigo. A solution of iodide of potassium in alcoholic ether soon yields a precipitate of iodine through the action of the air, (a fact well known to photographers who employ collodion,) because ether attracts oxygen and converts it into ozone. Ordinary oxygen, it is known, does not precipitate the iodine from the iodide of potassium. Moreover, the acetic acid produced during the reaction of air on alcoholized ether is not the cause of the precipitation of the iodine, as we may convince ourselves by direct experiment. In fact, if acetic acid be added to the solution of iodide of potassium in alcoholized ether, the iodine is not precipitated until after the lapse of several hours. When this mixture is oxidized, on the contrary, the precipitation of the iodine commences immediately. This phenomenon may be artificially produced by plunging an iron wire, heated to redness, into a mixture of the vapor of ether and air; by then pouring iodurct of potassium into the flask the iodine is at once disengaged. This last experiment was devised, I believe, by M. Hardwich.

We may add to what has been just said in relation to organic bodies, that M. Schönbein, by employing the alcoholic solution of the resin of guaiacum, has succeeded in proving that the oxygen furnished by the action of heat on the oxides of gold, of mercury, of platina, of silver, as well as on the peroxides

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* This is one of the first experiments which demonstrated to me that organic bodies can transform oxygen into ozone. This action of sugar on oxygen may be made manifest in another manner. If we plunge a bit of metal—iron, for instance—into a solution of any sort of sugar, we observe that the iron is rapidly and thoroughly oxidized where the metal is in contact with the sugar and the air; that is, at the surface of the liquid. If the metal is completely submerged in the solution it is not oxidized at all, or, at least, a very long time must elapse for oxidation to manifest itself. The corrosive action which sugar exerts on iron was long since observed, and the captains of *iron* trading-vessels are often averse to carrying cargoes of sugar for this reason. This corrosive action is owing to the fact that sugar (especially impure sugar or molasses) transforms the oxygen of the ambient air into ozone, which actively attacks the iron where it is in contact with the sugar and the air. The result, in the experiment above cited, is the formation of an abundant red precipitate, the larger part of which is oxide of iron. A combination is at the same time formed, which has been analyzed by Dr. Gladstone, and which is ($C^{12} H^{11} O^{11} FeO$.) It is impossible to produce this combination directly with sugar and the iron oxide. Many other metals are attacked like iron when placed in these circumstances—copper less than the others.

of lead and of manganese, contains traces of ozone. The quantity of this last depends on the temperature at which the oxide gives up its oxygen; the oxides which give up their oxygen most readily yield the most ozone.

Again, every one knows that by decomposing the bioxide of barytes at the ordinary temperature by the action of sulphuric acid, M. Houzeau has obtained ozone in great quantity. It is known, since chemistry has existed as a science, how much the *nascent state* of a body influences the combinations which it is capable of effecting; one combination is possible *only* when one of the bodies which enter into it is in the nascent state; another only when the two bodies which form it are in that state. What is called "nascent state" is, in my view, nothing else but the allotropic state of the bodies entering into combination; an opinion which I long since made public, and in which M. Houzeau seems to concur with me.

From all these facts it appears incontestable that *in every case in which oxygen enters into combination or abandons its combinations it is in the state of ozone.* If, then, we reflect on the results already obtained with hydrogen, chlorine, bromine, sulphur, phosphorus, &c., we shall be led to the conclusion that it must probably be the same with all simple bodies; that is to say, that all these bodies may assume an allotropic state analogous to ozone; that they are in that state at the moment of entering into combination, or when they are in the "nascent state."

When a body in this allotropic state is isolated, as, for example, ozone prepared by sulphuric acid and the bioxide of barytes, or in any other manner, we may observe that it passes by degrees, and in a very short time, to its ordinary state; and this is as it should be, for this body has necessarily a tendency to the state of equilibrium which is observed in all parts of nature. We have said that ozone is an allotropic state of oxygen; it is sometimes called "electrified oxygen."* As has been shown, it is oxygen whose polarity is developed in the highest possible degree. When ozone is produced, it is as though we took a molecule of oxygen in a neutral electric state and deprived it more or less completely of its positive fluid. It is clear that this molecule will constantly tend to reabsorb this lost fluid, that it may return to its natural state of equilibrium. This is the reason why the stability of bodies in this state is so slight.

We have noticed the influence exerted on the polarity of bodies by *electricity*. The action of *light*, in this respect, is not less manifest. The properties of chlorine exposed to diffused light, and of chlorine exposed to the solar light, constitute a striking example of what has just been said. Besides, many of the phenomena before spoken of, are best produced under the influence of light. To what extent *heat* operates in the development of polarity has been known since it was observed that certain combinations, which could not be effected at the ordinary temperature, are successfully brought about when the temperature of the reacting bodies is raised. To cite one example, which will be sufficient; when we have a mixture of oxygen and hydrogen in a glass receiver at the ordinary temperature, these gases will not combine; but if the mixture be heated, combination takes place immediately, because the heat develops the polarity of the two gases. But how do heat, light, and electricity develop polarity? This question we shall answer hereafter. The *contact* of a third body, not susceptible of combining with one or other of the two reacting bodies, is another condition which greatly modifies the polarity of bodies. This third body acts by its presence like heat, electricity, &c., in the development of polarity. Berzelius thought he had detected in these phenomena of contact the existence of a new force, which he called *catalytic force*, but in reality there is nothing here but a phenomenon of polarity, as will be presently shown.

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* It is thus designated by M. Scoutteten in his treatise on ozone, which name, however, is erroneous.

Among the phenomena commonly designated by the term *phenomena of contact* or *catalytic phenomena*, we may mention, as one of the most remarkable, the discovery in 1817, by Sir H. Davy, that a wire of platina will continue incandescent in certain gaseous mixtures. He observed, in this manner, the slow combustion of alcohol, of ether, of spirits of turpentine, of the oil of naphtha, of carburetted hydrogen, &c. He found, still later, that *palladium* possessed the same property. Some years afterwards, M. Doebereiner invented his hydrogen light apparatus, in which he employed spongy platina, and nearly at the same time Thenard observed the curious and apparently inexplicable phenomena which are exhibited when *oxygenated water* is added to certain oxides, such as the oxide of silver, &c. Dulong and Thenard subsequently studied the catalytic influence of *palladium*, (previously noticed by Davy,) of *rhodium*, of *iridium*, of *gold*, of *silver*, of *nickel*, and even of substances of a different nature, such as *charcoal*, *pumice-stone*, *porcelain*, *glass*, and *rock crystal*. Still later, Th. de Saussure announced that certain organic bodies, such as *humus compost*, wheat, cotton, silk, and lignine, possess properties analogous to those of the bodies previously named. He observed that these bodies effect a diminution of volume in a mixture of oxygen and hydrogen, and that the volume which disappears is in the proportion requisite to form water. MM. Mitscherlich, Faraday, Reinsch, Boettger, Millon, and others, have still further enlarged the field of catalytic phenomena. According to M. Reinsch, fibres of asbestos, when gilded or plated, exhibit all the phenomena which have been remarked with wire of platina or palladium, and it is nearly the same if these fibres be covered with iron, nickel, cobalt, or lead. M. Bœttger thinks that it is not always the *metal*, but often its oxide, which is the active body in these latter cases. He covered fibres of asbestos with oxide by plunging them in the solution of a metallic salt, then in ammonia, whence they were rapidly passed through boiling water, and were heated to redness in the flame of an alcohol lamp. It results, then, that the oxides of chromium, iron, nickel, maganese, &c., act like the platina wire of Davy. One of the last labors of the illustrious Thenard proved that in the case of any body whatever capable of disengaging a gas by heat, *such disengagement always takes place at a lower temperature when a third body, a metal, an oxide, &c., is added, although we are sure that this third body takes no part in the reaction.* The presence of chips of beech-wood in the manufacture of vinegar is another example of the phenomena with which we are engaged. Yeast and the cognate bodies may be placed in the same class.

In all these instances it is *chemical changes* which are in question; the combinations and decompositions of bodies. But *contact* may sometimes determine *changes* exclusively *physical*; as, for example, in the sudden crystallization of a saturated solution of sulphate of soda by the contact of a slip of glass. In 1856 I discovered that certain salts might effect the same molecular change on the phosphorus which Wœhler had observed in fusing ordinary phosphorus in a mixture of sulphuric acid and bichromate of potassa. These are salts whose cold solutions phosphorus does not decompose, or at least decomposes but slowly. The following is what I stated at that time: "It is a very remarkable fact that certain salts, whose cold solutions phosphorus does not decompose, appear to exert a peculiar action on it, causing it to undergo a molecular modification. If a portion of opaque reddish phosphorus be placed in nitrate of iron, diluted by about once its own volume of water, no chemical action is remarked even after a long space of time. If the whole be then heated until the complete fusion of the phosphorus is effected, and the tube with which we operate be then removed to a quiet place, the phosphorus will remain liquid a long time, and on decanting the liquor, will suddenly become solid, resembling, in all respects, a globule of melted glass. In decanting the salt after cooling, the liquid phosphorus will sometimes flow off with the solution. If after receiving the whole on filtering paper, we touch this liquid phosphorus with a rod of glass, it suddenly becomes solid, and without any sensible change of volume. With a magnifying glass, it is easy to see that no trace of crystallization exists in the globule. It is a limpid drop, and colorless as melted glass. By experimenting in different ways, I ascertained that this limpid phosphorus differs, as regards its chemical properties, in no respect from ordinary phosphorus. It does not, however, take fire when poured out in a still liquid state in contact with the air. This, then, is not an allotropic state of phosphorus analogous to ozone, but a molecular state peculiar to this body. I have satisfied myself that other salts may act on phosphorus like the nitrate of iron. If this colorless phosphorus be melted under water and then exposed to the light, it reverts to the state of ordinary opaque yellow phosphorus."

Other examples of physical changes, analogous to those just mentioned, are furnished by sublimation of the black sulphuret of mercury, which, by this treatment, becomes red, by the red and yellow iodides of mercury; and since in nature all phenomena are linked with one another, these two categories of facts, to wit: the chemical and the purely physical changes induced by contact, are connected by the phenomena of the explosion of different fulminating powders by mere contact with the barbs of a feather.

But these two last classes of facts stand somewhat apart from the domain of catalytic phenomena, inasmuch as they are regarded as *chemical phenomena.* Thus my experiments have taught me that phosphorus assumes the limpid and colorless state whenever its refrigeration, after fusion, takes place under the prescribed conditions, whatever may be the liquid with which we operate. It is a phenomenon exclusively physical. If certain saline solutions cause it to pass into this colorless and limpid state, it is because the phosphorus melted in these solutions can cool more regularly than in pure water, and because such cooling allows the molecules of phosphorus to arrange themselves in a position suited to transmit the light entirely without reflecting or dispersing it. The black and opaque modification of phosphorus, discovered by Thenard, is an analogous though inverted example of a molecular change.

An analogous case is presented in fibrous iron, when it is made to undergo frequent and prolonged vibrations. There is established in the mass, according to Pelouze, Liebig, and others, a molecular movement which occasions the crystallization of the metal; so that it is not rare to see a bar of iron of good quality slowly transformed under the influence of the vibrations into iron crystallized with large facets.

Phenomena of this sort belong, as we have seen, to the domain of physics, and have nothing in common with catalytic phenomena. Another order of facts, which more nearly approaches the latter, appears in the case of certain oxides, which, when submitted to a certain degree of heat, become perfectly *indifferent* in regard to reagents, giving place to a sort of phosphorescence. They are then no longer dissolved in acids, or if so, are acted upon with much difficulty. This may be referable to a loss of water, sustained by the calcination of these oxides, but it is more probable that these phenomena of *indifference* are analogous, but inverse to what is presented by oxygen when it passes into the state of ozone.

The *passivity* of iron, of cobalt, and of nickel—three of the most magnetic metals—appears due, according to recent experiments, to a slight layer of oxide, or another combination which forms suddenly on the surface of the polished metal, and preserves it from the contact of the acid. From some trials which I have lately made, but which are not complete, I am led to believe that other metals may show the same property in certain conditions.

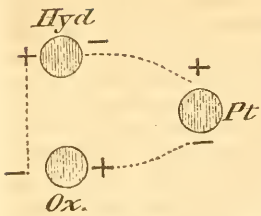
Let us pass to the examination of what occurs in catalytic phenomena, properly so called, and let us take as a first example and type the combination of hydrogen and of oxygen under the influence of the presence of platina. This phenomenon is sometimes known under the name of *slow combustion*, a term which serves to designate, at the same time, a great number of like facts. Davy sought to explain the slow combustions which he had observed by saying: "Platina and palladium conduct heat badly, being of feeble capacities in comparison with other metals; and in this, it seems to me, resides the principal cause why they maintain, produce, and render sensible these slow combustions." But we have seen that sundry other bodies act in the same manner as the wire of platina and palladium, when they are placed in the same conditions. M. Döbereiner, on the contrary, regarded the combustion of hydrogen by the influence of platina as "an electric effect resulting from a circuit in which hydrogen represents the zinc and the platina the other metal. It is the first example," he added, "of an electric circuit formed of a gaseous substance with a concrete body, whose activity has been verified." Since then, Grove, in England, has constructed his gaseous battery, of which we shall speak directly. Berzelius imagines a new force generated in the contact of this third body, (platina;) a force which he calls the *catalytic*, but of which he establishes no property unless it be that of causing the two bodies to combine which, before the contact of the third did not react. The influence which this great man exercised in chemistry has caused his theory to be blindly adopted by many chemists. Nevertheless, in proportion as catalytic phenomena became more common, as they began to acquire the features of generalization, doubts respecting the existence of this new force sprang up. And we shall endeavor, presently, to prove that this is not only a gratuitous hypothesis, but that, with the knowledge even then possessed regarding chemical combination, there was no need of supposing, for an instant, its existence. Doebereiner was, it seems to us, much nearer the truth than Berzelius, and this great chemist himself was subsequently led to regard the phenomena of the catalytic force as electrical.

Let us return now to the combination of hydrogen and oxygen under the influence of platina; and, to render things more evident, let us suppose for an instant that we have a molecule of hydrogen, A, and a molecule of oxygen, B, covered with neutral fluid. When we place these molecules in contact at the ordinary temperature, they will remain neutral; we shall observe neither chemical phenomena, nor electrical phenomena. But let them be heated; polarity manifests itself, combination takes place, and the needle of a galvanometer will sufficiently attest what is passing. By the action of the heat the two molecules, A and B, become the one positive, the other negative, in relation to one another. At the instant when the combination takes place the hydrogen gives off the negative fluid, the oxygen the positive fluid, and these two fluids passing into the wires of the galvanometer move the magnetic needle. There are here, then, two neutralizations of fluids; one in *the water*, formed by the combination; the other by the fluids given off.

This phenomenon which we have just been examining is the type of what is called *combustion.* Now, what heat has effected in this case the presence of a *third body* may likewise effect. This third body has the power, by its presence alone, of exciting "*combustion*" *at a temperature much lower than that which is necessary for the production of this phenomenon when this third body is not present.* It effects this by a *phenomenon of polarity identical with that which is observed in every other chemical reaction.*

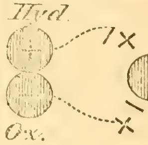
Before proving this by experiment, I shall show, with the help of a small diagram, what passes under these circumstances. Let us imagine the three bodies, hydrogen, oxygen, and platina sponge, in presence of each other:

Fig. 3.



The polarity which was developed between the hydrogen and oxygen by the action of heat is now developed between these by the presence of the platina. The *negative* electricity of the hydrogen tends to combine with the positive fluid of the platina, while the *positive* electricity of the oxygen tends to unite with the negative fluid of the metal. The oxygen and hydrogen become thus negative and positive in relation to one another, and can, consequently, unite. Thus the two gases combine in neutralizing their proper electricities. When, in place of using a simple wire of platina, we augment the surface of the latter by employing what is called the *sponge of platina*, the action is much more intense. In this case the platina becomes incandescent in consequence of the electric action exerted on the surface of that metal, and the heat developed is such that the hydrogen is eventually kindled.

Fig. 4.



I shall doubtless be asked if it is possible to prove, by direct experiment, that these phenomena really take place, and if we have not here, also, an hypothesis or gratuitous supposition. I shall proceed at once to support by experiment what I have just advanced, having only wished, in order to make myself more clear, to indicate the theory first and enter upon the facts afterwards:

Take two tubes of glass, one containing oxygen, the other hydrogen gas, and place them upon water which is acidified with a drop of sulphuric acid to make it a better conductor. Let there be then introduced into each tube a strip of platina, so that it shall be plunged in the gas of the respective tubes and be then bent back under the water in order to project from the surface of the liquid. So long as the two strips of platina thus disposed remain separated, we shall realize nothing; but let them be united by a galvanic *multiplier*, and there will instantly be perceived on the magnetic needle the action of the current proceeding from the strips of platina. We have thus, therefore, created a voltaic arrangement, by means of which we may produce all the effects which would be yielded by an ordinary pile. Fifty of these elements united constitute the celebrated *gas battery* of M. Grove, which is well known to all our readers. When the slips of platina present to the gas in the tubes a small surface, the action is feeble, and it increases with the extent of the surface of metal in contact with the gas. When this species of pile is exclusively employed in decomposing water, the volumes of the gases proceeding from the decomposition and collected in the voltameter are, both for the hydrogen and the oxygen, exactly equal to the sum of the volumes of these gases which disappear in the tubes of the pile. In proportion as the work goes on, the volumes of the gases in the tubes diminish; they are visibly absorbed, and the volume of hydrogen which disappears is always double that of the oxygen. We see the same process here as when we burn hydrogen by means of oxygen, at the ordinary temperature, by the contact of the spongy platina. Here, therefore, is one experiment in support of our assertions. Let us pass to others:

If we place a small gauge, made of a tube from two to three millimetres in diameter and filled with hydrogen gas, on a vessel containing a concentrated solution of chloride of gold, at the end of some days, *if the temperature has not sensibly varied*, the level of the chloride in the interior of the tube is little different from what it was previously; but if a platina wire be introduced from below into the gauge in such manner that it shall be in part immersed in the hydrogen gas and in part immersed at its lower extremity in the solution of gold, we see the gas diminish in volume in the interior, and even completely disappear, at the end of a certain time, if the platina wire ascends to the top of the tube. At the same time that the hydrogen disappears metallic gold is precipitated on the portion of the platina wire which is immersed in the metallic solution. In this experiment the hydrogen is burned by the chloride of gold, instead of being burned by the oxygen, as in the previous experiment. The positive electricity of the platina unites with the negative electricity of the hydrogen, and the negative fluid of the platina unites with the positive electricity of the salt. When the hydrogen reduces the latter, there are formed chlorhydric acid and metallic gold. The same thing would take place if we operated *without the platina*, but *at a high temperature* only, as has been already observed in reference to hydrogen and oxygen alone; for heat, as well as the presence of a third body, develops polarity.

We have here exactly the phenomena which are presented by the voltaic pile; in fact, a pile is composed of two different metals and of a liquid which can act on one of the metals. If we take water as the liquid, pure iron and pure copper as the metals, and plunge the pure iron alone in the water, there is no action; but if we add a third body—the copper—action exhibits itself, as the galvanometer indicates, the iron is oxidized, and hydrogen is disengaged. Volta introduced, from the beginning, *three substances* into the composition of his pile—two metals and moistened cloth.

The following are additional experiments: Take a porous vessel and place it in a glass. In the porous vessel put hydrochloric acid; in the glass, nitric acid; then let a strip of gold be placed in each receptacle; so long as the two strips remain separated there is no action; but, on bringing them together, they are immediately attacked, and the gold in the hydrochloric acid is dissolved; the nitric acid is at the same time deoxidized by the nascent hydrogen conveyed through the sides of the porous vase, while an electric current traverses the strips of gold.

Further, hydrochloric acid mixed with nitric acid does not act on the latter if the two acids are diluted; but if we add a third body, iron, copper, gold, platina, &c., action commences instantly; the chlorine is disengaged, the nitric acid is deoxidized, and the metal combines with the chlorine.

It is known that zinc oxidizes slowly in water, because it is an electro-positive metal; the disengagement of hydrogen in this case is very slow *at the ordinary temperature*—so slow as scarcely to be perceptible. But if a highly electro-negative body, such as sulphuric acid, be added, the polarity of the zinc is augmented; it becomes so electro-positive that the hydrogen is driven with violence from its combination, and the zinc becomes its substitute. Before the addition of the acid the polarity of the zinc was analogous to that of the hydrogen; but after that addition the metal becomes much more electro-positive than the hydrogen. Here is a phenomenon of polarity identical with that which takes place when a strip of platina is introduced into a detonating mixture. The sulphuric acid here acts, in regard to the zinc, as the platina towards the hydrogen.

I shall here cursorily cite some circumstances in which the influence of the presence of a third body makes itself felt in the most ordinary cases:

1. Pure iron in dry air is not attacked; in humid air it oxidizes.

2. Iron, in boiled water, (not aerated,) does not oxidize; in aerated water, oxidizes.

3. The peroxide of manganese (MNO^2) calcined in oxygen (or in air) does not alter;* heated in presence of oxygen and a base, a manganate is formed, (RO, MNO^3.)

4. When electric sparks are passed into a mixture of *dry* nitrogen and oxygen, no reaction is observed; if the two gases are moistened, nitric acid is formed.

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* If the temperature is very high, it loses oxygen.

5. Sulphurous and hyponitric acids, (SO^2+AZO^4,) when dry, do not react, but if water be added, we obtain ($AZO^3SO^3+HOSO^3$) which constitutes the crystals of the leaden chamber.

I would also call to mind that the action of *aqua regia* on the metals, which I have just cited, has been, for some time past, perfectly explained by chemists, (especially by Professor Koene, of the University of Brussels,) and all are nearly agreed as to the phenomena which present themselves when the two acids (CLH and AZO^5) are poured on a metal. Now, have we not, in this case, exactly the same phenomena which are observed when we place spongy platina in a mixture of oxygen and hydrogen gases?

The following experiment, easy to repeat, is a type of many chemical operations, and has not escaped being sometimes attributed to the "catalytic force:" when dry chalk is placed in a gun barrel, open at the ends, and heated, the chalk will yield its carbonic acid at a certain temperature, A. But if, while being heated, a current of vapor of water be passed over the chalk, it will yield its carbonic acid at another temperature, B, considerably lower than the temperature A. The explanation of this fact is quite simple. The vapor of water acts here by what is called in chemistry the *influence of the mass.* The water is a weak base; the chalk a very strong one; yet the volatility of the carbonic acid by heat, joined to the influence which the mass of water exercises, causes the carbonate to yield its acid in presence of the water at a temperature much lower than that which is requisite to produce this phenomenon when the water is not present. It does not follow thence that the water which has induced this phenomenon combines with the lime produced. The vapor acts solely as the platina does in Davy's experiment of slow combustion.

If, in one of the preceding experiments, in which oxygen gas, hydrogen gas, and platina have been brought into contact, we substitute vapor of alcohol for the hydrogen, we shall observe a phenomenon identical with that which is presented by the two simple gases. The alcohol will be burned like the hydrogen. In place of *water* we shall have *aldehyde* as the product, and the explanation of the phenomenon will remain the same. There is, in effect, a development of polarity between the three bodies, and a pile might be quite as well constructed by replacing the hydrogen in the pile of Grove with vapor of alcohol. Alcohol, which is always more or less electro-positive in relation to oxygen, becomes, by the presence of the platina, much more electro-positive; the oxygen tends, thereby, to combine with it, and this tendency naturally determines the formation of aldehyde and acetic acid. The same phenomenon presents itself when other vapors are substituted for that of alcohol, such as those of different essences or essential oils, of ether, oil of naphtha, &c.

In these circumstances, when in place of platina other metals or non-metallic bodies are employed, it is often necessary, in order that the action should manifest itself with all its intensity, to add the agency of heat, that the phenomenon of polarity may manifest itself and the combinations take place. But, in all cases, it will never be necessary to raise the temperature to so high a degree, in order to produce these phenomena, as it would be without the presence of the third body.

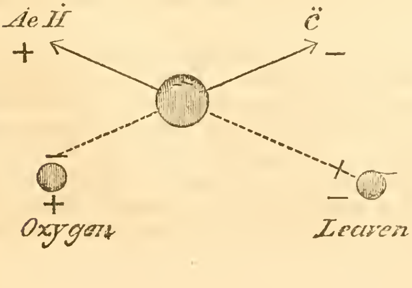
The presence of a third body may effect or facilitate *decompositions*, as well as combinations, in accordance with the same law. Thus, in a mixture of nitrous acid gas and hydrogen, the former gas is decomposed under the influence of the platina sponge. All the oxides of nitrogen in this case yield *ammonia.* Iron, copper, gold, &c., have the property of decomposing ammonia, if a little heat be applied: and, according to Davy, nitrous acid may be formed under these circumstances, (the presence of a metal,) at the expense of cyanogen and oxygen, whereas, in the same case, cyanogen and *hydrogen* give cyanuret of ammonia.

It may be asked why the spongy platina acts more efficiently in phenomena of contact than the wire of the same metal. It is because in proportion as a body is divided its surface is increased, and we have before seen that in the gas battery the action is so much the more intense as there is a greater surface of the platina in contact with the gas. Hence it is that we have in chemistry the general law, that the greater the division of bodies the better they react. Now, platina sponge is platina extremely divided, that is to say, presenting a very large surface. We further see the effect of division in the inflammation of pyrophorus.

In the phenomena which we have been examining the presence of a *fourth* body usually prevents the phenomenon of polarity from taking place, and consequently destroys the influence of the third body. Thus the presence of carbonic acid, for example, in the case of oxygen and hydrogen, prevents their combining through the presence of a metal. It is thus, too, that a drop of essence, will prevent fermentation. The spongy platina condenses in its pores from thirty to forty times its volume of ammoniacal gas. Now, in this state, it is altogether unfit to effect the combination of oxygen and hydrogen, and the platina does not recover this property so long as it contains ammonia. The presence of a fourth body does not, however, always exercise this influence. It is necessary that it should be a body suited to react with the others to place them in a state of equilibrium. Thus the presence of a little nitrogen does not prevent the combination or the slow combustion of hydrogen mixed with oxygen, under the circumstances we are considering. It has been seen, from my experiments cited above, that the presence of an *acid* (as a fourth body) prevents the production of ozone in certain special cases.

In ordinary fermentation, namely, in that, which is occasioned in a solution of sugar by *leaven*, it has been shown by Gay Lussac that the presence or the contact of air or of oxygen is an essential condition of the manifestation of the phenomenon, but that a single particle of oxygen suffices to excite, by leavening, the fermentation of a great quantity of sugar. Now, we have already seen that organic bodies may act upon oxygen to transform it into ozone; that is, may induce a phenomenon of polarity; and, moreover, in the experiments of De Saussure, that organic bodies may cause the combination of hydrogen and oxygen. We have already said that, in our opinion, fermentation commences with a phenomenon of polarity; oxygen or the leaven (it matters not which, as action is always equal to reaction,) acts here as the platina does in the mixture of oxygen and hydrogen. Polarity once brought into play in the saccharine solution, there is no reason why it should cease as long as there remains any sugar. The facility with which oxygen, as we have seen, is transformed into ozone by its contact with organic bodies, determines, from the very first, this phenomenon of polarity. The ozone produced being negative and the leaven positive, in relation to the gas, this oxygen tends to combine with the leaven; the sugar is, as it were, placed between the two poles of a pile; it is therefore divided into two other bodies, of which one is negative (CO_2) and the other positive (AEH.)

Fig. 5.



The presence of a fourth body often prevents this phenomenon; thus, when the *alcohol* produced becomes sufficiently predominant, all action ceases, and it is necessary constantly to remove this product, if we wish the fermentation to continue for an indefinite time.

It is probable that something analogous takes place when caseine converts sugar of milk into lactic acid.

It has quite lately been placed beyond doubt that there are three bodies present in this phenomenon, to wit: oxygen, sugar of milk, and a peculiar ferment. M. Pasteur, in effect, has recently isolated a body which he calls *lactic ferment*, and which, in his opinion, is the principal cause of the lactic fermentation. It is a gray and flocculent substance, which proceeds, without doubt, from the caseine. The lactic fermentation, therefore, would be altogether analogous to the alcoholic fermentation but with another ferment.

With regard to the viscous and butyric fermentations, &c., I have nothing to add to what is already known respecting these phenomena—which is very little. Respecting *eremacausis* (the slow combustion of organic substances by oxygen at the ordinary temperature) or *putrefaction*, we have already seen how it is induced, and how it is always accompanied with the production of ozone.

It remains to speak now of the action of acids on organic bodies, for, in this action it has been often supposed that catalytic phenomena were distinguishable. When an organic substance is treated with an acid it often happens that this acid oxidizes it while it is itself decomposed, such being the case when sugar, &c., is treated with azotic acid, sulphuric acid, &c. At other times the acid, or a derivative of the acid, combines with the body, either after having oxidized it or directly. We observe this when we treat cellulose with concentrated nitric acid. At other times, again, the oxide of a radical compound is formed in the action of the acid on the organic body, with which the deoxidized acid combines, as may be seen when hyponitric acid reacts on the oxides of certain radical compounds which may become peroxidized, and themselves unite in this state with the nitrous acid, giving rise to nitrites ($RO+AZO^4=RO^2AZO^3$.) In fine, when acids are added to organic bases, or to alcohols, there are formed salts, or compound ethers, which are also salts. In these two cases the acid replaces the water of the base or of the alcohol. There are, however, cases where acids occasion a change of a wholly different kind, and give rise to products which may be termed abnormal.

Such is the case where sulphuric acid acts on fecula or on cellulose producing sugar. In this case the influence of contact has been seen, and many chemists have supposed that the sulphuric acid acts only by its presence, and that the fecula combines with a certain quantity of water to produce sugar. It is, however, not so; the action of sulphuric acid on starch or cellulose appears to be altogether analogous to the formation of ether by the action of this same acid on alcohol. In this latter case there is, as we know, the formation of an intermediary compound, sulpho-vinic acid, which, by the action of heat, separates into ether and free sulphuric acid. Now, sulphuric acid forms with starch or cellulose, an analogous compound, *sulpho-lignic acid*, which, like sulpho-vinic acid, forms soluble salts with lime and barytes, and separates by ebullition into sugar and free sulphuric acid. The sulpho-lignic acid cannot be formed except at the ordinary temperature. If the temperature be raised we obtain other products, sulphuric acid is then decomposed, and produces, with the cellulose, &c., brown substances resembling *humus*.

The *diastase* of sprouted barley gives rise to the same phenomenon as the sulphuric acid in the former case, namely, the production of glucose, but this is accomplished in a different manner. According to all appearance the transformation is here direct. There is no intermediate combination; but the circumstances of that remarkable modification which the starch undergoes from contact with the diastase are still very incompletely known. It would seem, however, that the diastase acts as a *ferment*, (as the ferment, for example, in the alcoholic fermentation;) and M. Magendie has observed in this connexion that blood, bile, urine, sperm, &c., in a word, the other ferments act like the diastase. Starch, in these circumstances, is first transformed into dextrine, then into glucose. It is highly probable that hereafter it will be discovered that the presence of a third body is essential to this phenomenon, and that this last being better understood, will be susceptible of the same explanation as chemical reactions in general.

It remains to examine the effects produced by the oxygenated water of Thenard, and which are usually regarded as catalytic phenomena.

Oxygenated water is one of the most electro-negative bodies known; hence it acts in all cases as an oxidizing and energetic acid. When placed in contact with the powder of platina a lively effervescence is observed, the platina is not altered, all the oxygen of the oxygenated water is disengaged, and we obtain water, pure oxygen, and pure metal. This reaction is precisely what was to be expected: in effect the oxygenated water acts towards the platina as an acid; it tends to oxidize the metal, that is to say, in this phenomenon the platina is electro-positive, the oxygenated water electro-negative. The platina tends to form a peroxide, but this oxide not being susceptible of being formed under these circumstances, the phenomenon of polarity takes place nevertheless, and the oxygen is disengaged.

The same thing occurs if we treat comminuted platina with nitric acid. This acid, which is an energetic oxidant, like oxygenated water, is decomposed at a temperature much lower than if the platina were not present. I have observed lately that this phenomenon takes place with certain sulphurets, especially with certain specimens of sulphuret of copper from the environs of Lake Baikal, in Siberia, which were sent to me for analysis. If we take this sulphuret of copper, calcined and dry, and in a state of great division pour upon it concentrated nitric acid, and then heat with an alcohol lamp, the nitric acid is decomposed, and gives forth ruddy vapors, without attacking the sulphuret, and at a temperature much inferior to that at which nitric acid is decomposed by heat alone. If we would transform the sulphuret into a sulphate we must not operate in this manner, as we lose the acid without obtaining the desired effect. In order to oxidize the sulphuret we must add the acid drop by drop whilst we are applying heat; the sulphuret is then actively attacked.

Molybdenum, treated with oxygenated water, passes, on the contrary, to the acid state, as is the case also with arsenic, selenium, &c., while silver, gold, mercury, palladium, rhodium, &c., &c., act in these circumstances like platina.

In this action of platina and of metals in general, on oxygenated water, an extreme tenuity in the metal is an indispensable condition for obtaining a prompt reaction. This has been equally indicated in speaking of the action of divided platina, and it is the same in every chemical reaction, whatever it may be.

The oxides which can be oxidized, ($\mathrm{C}a'$, $\mathrm{B}a'$, $\mathrm{C}a'$, &c.,) decompose oxygenated water in passing to the state of peroxides, ($\mathrm{C}\ddot{a}$, $\mathrm{B}\ddot{a}$, $\mathrm{C}\ddot{a}$, &c.) The oxides which cannot be oxidized may sometimes effect the decomposition of oxygenated water, because they are highly electro-positive in regard to it, and the phenomenon of polarity takes place. The peroxide of manganese, for example, cannot be oxidized, except in contact with a base with which the manganic acid produced may combine. When this peroxide is added to oxygenated water, manganic acid cannot be formed; but oxygenated water, being a body but slightly stable and highly electro-negative in regard to $\mathrm{M}n$, reacts on this peroxide and tends to oxidize it. The peroxide of manganese is then in the same situation with platina, silver, &c., cited above.

The presence of a third body in phenomena of contact does not act solely by exciting a phenomenon of polarity, which, without the presence of the third body, would not occur, unless brought about by a high temperature. It often exercises, besides, a very marked influence on the products of the reaction, especially if we operate on organic bodies; thus: Take two small tubular retorts of the same capacity, and place in the one tartaric acid, in the other tartaric acid and platina sponge; then let the two retorts be heated at the same temperature, which is easily done by placing both in the same sand-bath.

The first retort will, in these circumstances, yield gases and the ordinary empyreumatic products. The other, on the contrary, will give crystallized products. This, however, is but one example in a thousand. In one case the reaction is more ready than in the other, consequently the products should be different. A body susceptible of yielding a gas by the action of heat will yield this gas at a temperature much lower—that is, more easily, when it is mixed with another body which cannot combine with any of the elements submitted to experiment.

Supposing that we submit to the action of heat a *binary* body to which we add a third element; whether this third body combines with one of the elements of the binary body, or does not so combine, its presence will always facilitate the reaction which takes place, by the development of polarity in which this third body bears a part.

That the exact knowledge of the modes of action of a third body may become of great utility to the chemist the following example, familiar to all, will show. To procure oxygen by means of the chlorate of potassa, we shall economize combustible matter by introducing a body foreign to the reaction, for instance, clippings of platina, sand, peroxide of manganese, &c., &c.

It is some years since M. Millon remarked, that zinc dissolves much more rapidly, and produces at the same time much more hydrogen, when a metal less electro-positive is added to the sulphuric acid. This is the type of catalytic phenomena in general. A little of the chloride of platinum, mixed with the sulphuric acid, gives rise to a disengagement of hydrogen one hundred and fifty times stronger than that which the same surface of zinc would produce in the pure acid, of the same degree of concentration, and in the same time. This experiment is, as we see, a simple modification of another well known one, which consists in surrounding the zinc with a wire of platina to accelerate the solution; for the platinum of the chloride is precipitated on the zinc, and forms with it a voltaic element.

Hydrochloric acid mixed with a little chloride of platinum dissolves lead and copper with a disengagement of hydrogen. The first dissolves it in this way when cold, the second only when heated, but the disengagement is as rapid with the zinc.

Before concluding this memoir, we should endeavor to compare the relations of the phenomenon of *polarity*, with what is called *chemical affinity*. And in the first place, when we speak of polarity, we express a fact verified by experiment—a fact which has nothing in common with the so-called "catalytic force," *of which it is impossible to verify a single property.* Every chemist knows that the phenomenon of polarity manifests itself in *every chemical reaction.* We have shown in the present paper that it manifests itself equally in catalytic phenomena. We verify polarity by direct experiment, as has been shown. This, then, is not a word exchanged for the word *catalysis*, but is a fact well established.

We have already shown how much the phenomenon of polarity influences chemical reaction. Is it the cause of affinity, or is affinity itself a force *sui generis?* Berzelius remarked, in regard to the before-cited experiments of Millon, "These experiments show clearly that the greater or less affinity of the acid for the metal, and, reciprocally, is due to the more electro-positive state which results from the contact of the metal deposited; and that it is this state which induces the affinity, but not inversely the affinity which induces the electric state." In effect, one party thinks electricity or polarity produces chemical action, while another supposes that all electricity is produced by chemical action. Either explanation is simply absurd. Experiment shows that the phenomenon of polarity is an electrical phenomenon if you choose—accompanies chemical action *everywhere;* but we cannot thence conclude that it is the cause of the latter, just as we sometimes see electricity excited, without being able to assign to this electricity a chemical origin.

We may say that in every case, without exception, an electric current may determine the combination or the decomposition of bodies, and *polarity* seems to be an *indispensable condition*, in order that chemical action should manifest itself. Chemical affinity cannot be regarded as the *cause* of the polarity; polarity cannot be regarded as the *cause* of the affinity. When, in heating a plate of bismuth soldered to a plate of antimony, electricity is produced, what is the *cause* of its production? Is it in the affinity of the bismuth for the antimony? Certainly not! Is it in the heat? We enter here upon considerations of a very high order, and which it is not competent for us to discuss on this occasion. Let us say, however, a few words respecting them.

In all that has been said, we have employed the term *force*. Although it be now nearly demonstrated that nearly all force is but a molecular movement, and that, consequently, the idea which we generally attach to the word *force* is inexact, yet the epoch has not arrived when we can suppress these terms in the language of science, and, at the same time, make ourselves intelligible. I have thus found myself compelled to employ them. In the remarkable theory of M. Grove on the correlation of physical forces, things are considered, according to all probability and verisimilitude, under their true point of view. Movement, which is universal, manifests itself to us sometimes under one form, sometimes under another, in order to give rise to effects which we are in the habit of attributing to particular forces. These forces, or, rather, these movements, may be transformed into other equivalent movements, according to the circumstances in which they are made to act. Thus it is that if we apply friction (movement) to a piece of dry wood, it is transformed into another movement which we call heat. If we apply friction to sealing-wax, the friction (movement) is transformed into current electricity. If we heat water a large quantity of the heat is transformed into a motive force, into movement. If we heat a plate of bismuth soldered to a plate of antimony, the heat is transformed into current electricity. In like manner, heat is transformed into light, light into heat, into electricity, into affinity, &c.; affinity into heat, into electricity, into light, &c., &c. There exists no "force" which we cannot transform, equivalent for equivalent, into one or several other "forces." These transformations are manifested throughout all nature, and we see what we call "physical and chemical forces" transformed into "organic forces," and governing the phenomena of life. Just as atoms combine or replace one another by equivalents, so are different movements or "forces" substituted, one for another, *equivalent for equivalent*. A given quantity of *chemical action* will always give the same quantity of *electricity;* a given quantity of electricity will, in its turn, give the same quantity of chemical action; a given quantity of heat will always give the same quantity of motive force; an equivalent of *chemical action* will always give an equivalent of heat, &c., &c.

In fine, everything is connected or linked together in nature; movement, like matter, is universal; its modifications produce all that we regard as "forces" or effects of forces. For the details and experiments on which this striking theory is based, (and it is now admitted by most physicists,) we must refer our readers to the memoir of M. Grove,* of which we have a French translation from the learned pen of M. F. Moigno. But, from the few words which have been said, it will, I think, be conceived how heat can act to excite polarity and chemical action; how the latter, in its turn, can excite polarity and heat, without one of these "forces" being the *cause*, properly speaking, of the other; how the movement of the barbs of a quill can act so as to produce explosion with fulminating powders, &c.

**Correlation of physical forces.*

CONCLUSION AND CONCLUSIONS.

Although many of the experiments cited in this memoir are my own, and others have been repeated and confirmed by myself on more than one occasion, I could have wished, nevertheless; to have added more in confirmation of the views which I have enunciated on the *phenomena of contact*. For, however conclusive the experiments cited may seem to myself, it is not certain that they will appear so to every one. But two things essential to all scientific research fail me at this moment, and prevent me even from completing researches already commenced. For what concerns the present attempt, I trust that it has been demonstrated to a certain point:

I. That the allotropic states of bodies, analogous to ozone, are due to a phenomenon of polarity acting in special circumstances, and having the effect of rendering the body, which is the subject of experiment, much more electro-negative, or more electro-positive, than it was; and that the state which we call in chemistry the "nascent state of bodies" is nothing else but this allotropic state. Further, that all simple or compound bodies assume this state at the moment when they enter into combination, and at the moment when they abandon their combinations.

II. That in general the phenomena attributed to the *catalytic force*, as well as those which every other chemical reaction presents, admits of a very simple explanation.

III. That the facts observed and explained by the action of a force called *force of contact, or catalytic force*, are due to an electro-chemical phenomenon, known under the name of *polarity*, which, without being the cause of the chemical action, accompanies the latter in all cases, may be verified by direct experiment, and seems to be an essential condition of its manifestation.

IV. That the force known under the names of *catalysis, force of contact*, or *catalytic force*, is a pure creation of the imagination.

ON ATOMS.

BY SIR JOHN HERSCHEL.

"I sing of atoms."—*Rejected Addresses.*

Dialogue.—Hermogenes et Hermione interloquuntur.

Hermione.—What strange people those Greeks were! I was reading this morning about Democritus, "who first taught the doctrine of atoms and a vacuum." I suppose he must have meant that there is such a thing as utterly empty space, and that here and there, scattered through it, are things called atoms, like dust in the air. But then I thought, "What *are* these atoms?" for if this be true, then, these are all the world, and the rest is—nothing!

Hermogenes.—Yes. That is the natural conclusion: unless there be something that does not need space to exist in; or unless there be *things* that are not material substances; or unless space itself be *a thing:* all which is deep metaphysic, such as I am just now rather inclined to eschew. But, dear Hermione, how am I to answer such a host of questions as you seem to have raised—all in a breath? The Greeks! Yes, they were a strange people—so ingenious, so excursive, yet so self-fettered; so vague in their notions of things, yet so rigidly definite in their forms of expressing them. Extremes met in them. In their philosophy they grovelled in the dust of words and phrases, till, suddenly, out of their utter confusion, a bound launched them into a new sphere. There is a creature, a very humble and a very troublesome one, which reminds me of the Greek mind. You might know it for a good while as only a fidgety, restless, and rather aggressive companion, when, behold, hop! and it is away far off, having realized at one spring a new arena and a new experience.

Hermione.—Don't! But a truce to the Greek mind with its narrow pedantry and its boundless excursiveness. The excursiveness was innate, the pedantry superinduced—the result of their perpetual rhetorical conflicts and literary competitions. I have read the fifth book of Euclid and something of Aristotle; so you need not talk to me on that theme. Do tell me something about these atoms. I declare it has quite excited me, 'specially because it seems to have something to do with the atomic theory of Dalton.

Hermogenes.—Higgins, if you please. But the thing, as you say, is as old as Democritus, or perhaps older; for Leucippus, Democritus's master, is said to have taught it to him. Nay, there is an older authority still, in the personage (as near to an abstraction as a traditional human being can be) Moschus (not he of the Idyls.) But the fact is that the notion of THE ATOM—the *indivisible*, the *thing* that has *place*, *being*, and *power*—is an absolute necessity of the human thinking mind, and is of all ages and nations. It underlies all our notions of being, and starts up, *per se*, whenever we come to look closely at the intimate objective nature of things, as much as space and time do in the subjective. You have dabbled in German metaphysics, and know the distinction I refer to.

Hermione.—You don't mean to say that we are nothing but ATOMS?—Place! being! power! Why, that is I, it is you, it is all of us. Nay, nay. This is going too fast.

Hermogenes.—Perhaps it is.—(You have forgot thought, by-the-by, and will.) But I am not going to make a single hop quite so far. We shall divide that into two or three jumps, and loiter a little in the intermediate resting-places. But, to go back to your atoms and a vacuum. What does a vacuum mean?

Hermione.—Vacuum? Why, emptiness, to be sure! I mean empty space. Space where *no thing* is. I am not so very sure that I can realize that notion. It is like the abstract idea of a lord mayor that Pope and Atterbury talk about; and in getting rid of the *man*, the gold chain and the custard are apt to start up and vindicate their claim to a place in the world of ideas. And yet I do mean something by empty space. I mean *distance*—I mean *direction:* that steeple is a mile off, and not *here* where we sit; and it lies southeast of us, and not north or west. And if the steeple were away, I should have just as clear a notion of its *place* as if I saw it there. There now! But then distance and direction imply two *places.* So there are three things anyhow that belong to a vacuum; and let me tell you, it is not everything that three things positively intelligible can be "predicated" of (to speak your jargon.)

Hermogenes.—Dear me, Hermione! how can you twit me so? Jargon! Every specialty has its "jargon." Even the law, that system of dreams, has its "jargon"—the more so, to be sure, because it *is* a system of dreams, or rather of nightmares, (God forgive me for saying so!) Well, then, you seem to have tolerably clear notions about a vacuum—at least, I cannot make them clearer. Much clearer, anyhow, than Descartes had, who maintained that if it were not for the foot-rule between them, the two ends of it would be in the same place. Still, there is much to be said about that same *Vacuum*, especially when contrasted with a *Plenum*, which means (if it mean anything) the *exact opposite of a vacuum.* In other words, a "jam," a "block," a "fix." But, on the whole, I lean to a vacuum. The other idea is oppressive. It does not allow one to breathe. There is no elbow-room. It seems to realize the notion of that great human squeeze in which we should be landed after a hundred generations of unrestrained propagation.* One does not understand how anything could get out of the way of anything else.

Hermione.—Do come back to our dear atoms. I love these atoms: the delicate little creatures! There is something so fanciful, so fairy-like about them.

Hermogenes.—Well; they have their idiosyncrasies. I mean they obey the laws of their being. They comport themselves according to their primary constitution. They conform to the fixed rule implanted in them in the instant of their creation. They act and react on each other according to the rigorously exact, mathematically determinate relations laid down for them *ab initio.* They work out the preconceived scheme of the universe by their—their——

Hermione—Their? Stop, stop! my dear Hermogenes. Where will you land us? Obey laws! Do they know them? Can they remember them? How else can they obey them? Comport themselves according to their primary constitution! Well, that is so far intelligible: they are as they are, and not as they are not. Conform to a fixed rule! But then they must be able to apply the rule as the case arises. Act and react according to determinate relations! I suppose you mean relations with each other. But how are they to know those relations? Here is your atom A, there is your atom B, (I speak as you have taught me to speak,) and a long interval between them, and no link of connection.

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* For the benefit of those who discuss the subjects of Population, War, Pestilence, Famine, &c., it may be as well to mention that the number of human beings living at the end of the hundredth generation, commencing from a single pair, doubling at each generation, (say in thirty years,) and allowing for each man, woman, and child an average space of four feet in height, and one foot square, would form a vertical column, having for its base the whole surface of the earth and sea spread out into a plane, and for its height 3,674 times the sun's distance from the earth! The number of human *strata* thus piled one on the other would amount to 460,790,000,000,000.

How is A to know where B is, or in what relation it stands to B? Poor dear atoms! I pity them.

Hermogenes.—You may spare your sympathy. They are absolutely blind and passive.

Hermione.—Blind and passive! The more the wonder how they come to perceive those same relations you talk about, and how they "comport themselves," as you call it (*act*, as I should say) on that perception. I have a better theory of the universe.

Hermogenes.—Tell it me.

Hermione.—In the beginning was the nebulous matter, or *Akasch.* Its boundless and tumultuous waves heaved in chaotic wildness, and all was oxygen, and hydrogen, and electricity. Such a state of things could not possibly continue; and as it could not possibly be worse, alteration was here synonymous with improvement. Then came——

Hermogenes.—Now it is my turn to say, Stop! stop! *Solvuntur risu tabulæ.* Do let us be serious. Remember, it was you who began the conversation. *Je me suis seulement laissé entrainer.* The fact is, I have only so far been trying you, and I see you are apt. There lies the real difficulty about these atoms. These same "relations" in which they stand to one another are anything but simple ones. They involve all the "ologies" and all the "ometries," and in these days we know something of what that implies. Their movements, their interchanges, their "hates and loves," their "attractions and repulsions," their "correlations," their what not, are all determined on the very instant. There is no hesitation, no blundering, no trial and error. A problem of dynamics which would drive Lagrange mad, is solved *instanter* "*Solvitur ambulando.*" A differential equation which, algebraically written out, would belt the earth, is integrated in an eye-twinkle; and all the numerical calculation worked out in a way to frighten Zerah Colburn, George Bidder, or Jedediah Buxton. In short, these atoms are most wonderful little creatures.

Hermione.—Wonderful, indeed! Anyhow, they must have not only good memories, but astonishing presence of mind, to be always ready to act, and always *to* act without mistake, according to "the primary laws of their being," in every complication that occurs.

Hermogenes.—Thou hast said it! That is just the point I knew you must come to. The *presence of* MIND is what solves the whole difficulty; so far at least as it brings it within the sphere of our own consciousness, and into conformity with our own experience of *what action is.* We know nothing but as it is conceivable to us from our own mental and bodily experience and consciousness. When we know we act, we are also conscious of will and effort; and action without will and effort is to us, constituted as we are, unrealizable, unknowable, inconceivable.

Hermione.—That will do. My head begins to turn round. But I hardly fancied we had got on such an interesting train. We will talk of this again. More to-morrow. Now to the feast of flowers the children are preparing.

ON THE CLASSIFICATION OF BOOKS.

BY J. P. LESLEY
LIBRARIAN OF THE AMERICAN PHILOSOPHICAL SOCIETY.

LIBRARIES are of two kinds, general and special. The one now catalogued* is of the most general description, and affords an opportunity for some thoroughly philosophical arrangement, based upon an analysis of human knowledge which will leave nothing disregarded. However imperfectly the attempt to accomplish the object may succeed in this or other instances, failure will only stimulate to renewed endeavor. A reasonable arrangement of every collection in the hands of man is a call of the soul, to be obeyed. A merely empirical adjustment of minerals to the drawers which contain them, or of books to the shelves on which they stand, fortuitously numbered as they are obtained, and indexed alphabetically for the convenience of servants, justly embarrasses, depresses, and disgusts the thinker.

Two arrangements of a catalogue, systematic or raisonnée, present themselves at once for selection: the analytic and the synthetic. The synthetic corresponds with the teachings of nature and art, by the experience of which we are instructed in items to the knowledge of the whole. The analytic corresponds with the method of the schools, and of their reliquiæ, books; which state principles, and then describe their applications; announce laws, and then show their utility; first furnish knowledge in its most advanced or abstract condition, and afterwards embody it in illustrations. Face to face with nature, the form of man receives its noblest *inspiration*, but by libraries of books the spirit of man obtains its largest *information*.

The book of nature and the book of the library being thus opposed, the one is not consulted in the same manner as the other. The book of nature begins with its illustrations, follows with descriptive text, sketches out indistinctly a few broad statements, suggests a summary, and omits the index altogether. The book of the library, on the contrary, carefully places its table of contents in the front, makes of its preface an epitome, and of its body an argument, leaving notes and pictures to be consulted, at the pleasure of the reader, at the close. In these antagonistic gymnasia two antagonistic tribes are bred, mere scholars and mere naturalists, characterized by opposite tendencies: by the looseness with which the former state facts, and the pertinacity with which they maintain doctrines; by the scrupulous narrowness with which the latter examine things, and the facility with which they adopt new theories. To be a mere scholar is to run the risk of becoming inaccurate in facts and dogmatic in judgment. To be a mere naturalist is to become materialistic and unimaginative, narrow-minded and pedantic.

The true philosopher resides alternately in nature and in the library. Writing in the first and reading in the second, he weaves shuttle-like the stuff of thought out of which he and his fellows array themselves in goodness, truth, and beauty. But of these two homes, the philosopher has two very different stories to tell; he regards them with very different sentiments. The one is ancestral—he was born into it and belongs to it; the other he makes proper to his hand. The philosopher must accept his rural residence as it grows, enclosing him with

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* Library of the American Philosophical Society, Philadelphia

powers of arrangement regardless of his will, yet amiably inviting his attention. But his urban residence, the library, he arranges as the master of it, according to his own convenience, to correspond with his own necessities and to illustrate his tastes. The character of the library is therefore determined by the principles of art, not nature.

The general library, therefore, is a picture of a generous intellect, well stored, well ordered, and open to enlargement in all directions.

Its compartments represent the grand natural divisions of knowledge.

Its classification should be in an ascending and advancing series.

Its treasures, like those of memory, should be preserved in the natural order of time, and the natural order of space should be ancillary and complementary, wherever applicable.

These are the maxims by which the cataloguing of the library of the American Philosophical Society has been governed. Eight principal classes carry from the universal to the special, from the abstract to the concrete, from the inorganic to the organic, and from matter to mind. Each class begins with the theory of its subject and follows with its practice. Excepting the first, which represents the abstract conception of knowledge itself with its universal applications, each class advances the theme beyond a point at which the class preceding leaves it. More scientific names might be invented for these classes, but only by having recourse to a pseudo-classical, harsh, unknown compound terminology, and therefore the names which have been adopted are those best known and in common use, as follows:

1. GENERAL SCIENCE.

1. Encyclopædias, &c. 1^2. Learned Societies. 1^3. Catalogues of Libraries.

2. THE MATHEMATICAL SCIENCES.

2. Mathematics. 2^2. Astronomy, &c. 2^3. Geodesy, &c. 2^4. Physics.

3. THE INORGANIC SCIENCES.

3. Chemistry. 3^2. Mineralogy. 3^3. Mining. 3^4. Geology and Palæontology.

4. THE ORGANIC SCIENCES.

4. Biology. 4^2. Botany, &c. 4^3. Zoology, &c. 4^4. Medicine, &c.

5. THE HISTORICAL SCIENCES.

5. Chronology. 5^2. Ethnology. 5^3. Archæology. 5^4. History.

6. THE SOCIAL SCIENCES.

6. Sociology. 6^2. Manufactures. 6^3. Commerce. 6^4. War. 6^5. Law.

7. THE SPIRITUAL SCIENCES.

7. Language 7^2. Belles-Lettres. 7^3. Fine Arts. 7^4. Logic, &c.
7^5. Education, &c. 7^6. Religion.

8. PERSONAL SCIENCE.

8. Biography.

The divisions of the eight classes are naturally made by separating the pure sciences from their applications, and by grouping the latter according to their relationships. Thus the mathematical sciences are divided into—Mathematics pure (2^1); mathematics applied to astronomy (2^2); mathematics applied to geodesy (2^3); mathematics applied to mechanics and physical questions generally (2^4.)

It may be thought that this order ought to be reversed if the rule of an upward advancing series be inflexible and absolute; and that the first application of pure mathematics should be to physics or pure mechanics; the second to geodesy; and the third to astronomy or celestial mechanics. But a little consideration will teach that the first, most common and closest practical application of pure mathematics has been to astronomy and navigation; then to geodesy; and then to the mechanic arts; whereas physics proper have been based hitherto much more on experiment than on calculation. The rule of an upward advancing series is indefinite in one of its factors, if not in both; for questions of dignity among the sciences are not always easy to settle; nor can astronomy maintain so easily as once she could her right to precedency at court before geodesy, now that the personal characters of the planets and fixed stars have been so critically discussed. The rule of common usage, therefore, which is also the rule of convenience to some extent, must have some power given it over these arrangements; especially where, as in the case in point, still further subdivisions must be made to reach the last or most concrete applications of the science pure. Astronomy is indissolubly connected with meteorology, and finds its practical utility in navigation. Geodesy cannot be separated widely from geography; while this last involves voyages and travels, and this again maps and charts, which are the direct objects of geodesy. No less does the division of physics create, if the library be extensive, the distinct sections of light, heat, magnetism, electricity, &c.

But there is another reason for the order adopted. A third and most important rule of arrangement remains to be stated. It will be seen that certain subjects are essentially transitional, and must be placed at the end of the class to which they belong, for the reason that they carry the train of development over to the beginning of the class next following.

Thus, in Class II, that of the mathematical sciences; division 2^4, that of physics, stands last because the next Class III, that of the inorganic sciences, begins with chemistry. In Class III, division 3^4, geology comes last, because it carries with it palæontology, mediating between the inorganic and the organic world. In Class IV, division 4^4, human physiology comes last, because it brings into view the close and consummation of the whole organic system, man, and thereby prepares the way for historical research.

Leaving the first or natural series, and coming to the second or human series, we find the same rule reigning. In Class V, that of the historical sciences proper, which still regards mankind from a naturalistic point of view as inhabiting the earth like the other orders of created beings, but with peculiar and higher relationships to it, namely, the relationships of time, progress, development, and accomplishments, and therefore commences with chronology, it is plain that division 5^4, that of history proper, must follow ethnography and archæology, instead of preceding them, because it alone can introduce the discussion of society.

In Class VI, that of the social sciences, regarding mankind with still increasing respect—no longer as a mere herd of intelligent animals spread abroad at first, and afterwards migrating to and fro upon the earth by virtue of cosmical influences, and absolutely under their direction, but as groups of thoughtful people, endowed with the genius of arts and arms, and skilled in manufactures, commerce, war, and law, it is evident that these four applications of human genius in society maintain the ascending movement, and pass us seriously from this over to the next. Legal science, division 6^5, stands last in order, because related nighest, through language, to the spiritual sciences, which occupy the seventh plane.

In Class VII, its last division, 7^6, that of religious science, is not merely the culmination of this particular class, considered as the range of man's instincts of self-expression: language, belles-lettres, fine arts, rhetoric, logic, education, and philanthropy; but is a worthy engrossment and presentation of all the classes gone before, and the fittest introduction to Class VIII, biography, the science of the individual man *per se*, of the microcosm, the summary and conclusion of knowledge, the return of the circle into itself.

Regarded as divisions and subdivisions of human knowledge, it is very likely that other minds consulting this catalogue will take a different view of the relative value of these classes and divisions, and therefore of their respective claims to the prominent places which they occupy. But there is not one of them that has not been called and is not well recognized as a separate branch of science, having societies specially founded for its investigation and text-books written for its special elucidation. The list also seems complete. There may be question of the arrangement in certain parts, but the uninterrupted progression of the whole will be allowed on the ground of the accepted hierarchy of the numerical, inorganic, organic, mental, moral, and religious worlds. No argument can change this order of sequence and dignity.

Certain exigencies of the librarian, nevertheless, or of the student, and certain inosculations, interferences, or cross-relationships among the many members of the corpus scientiarum, cause perplexity, and introduce an apparent element of discord. But when the principal instances of its appearance have been mentioned, they will be seen to cause no serious disturbance in the order of the whole, and may be compared to those threads of shade which throw themselves across the colors of the spectrum, only to suggest new revelations of the harmony which reigns throughout the universe of light.

The following are the principal suggestions to be made in view of the practically abnormal occurrences to be expected in this arrangement of a library.

Class I. Under societies' proceedings (1^2) a complete list of learned societies, whose publications are represented in any degree in the library, is given. But as many of these societies devote their publications to special sciences, the detailed description of their issues must appear under such special heads. In such an immense collection of these issues as that now making by the Smithsonian Institution at Washington, it is found necessary to devote one entire hall of the library to their reception, and, in fact, to organize a separate department of correspondence. Even with smaller collections, this is by far the best arrangement for the librarian. But for the student it is evidently more desirable to have the issues of all botanical societies in the alcove devoted to botany, all those of medical societies in the alcove of medicine, all those of antiquarian societies in archæology, those of historical societies in the cases of history, &c. But for the same reason it is proper to classify the titles of their issues under corresponding heads in the catalogue raisonnée. If, at the same time, the titles of all general societies and references to the titles of all special societies be resumed under the division (1^2) of learned societies, a *coup d'œil* is obtained of the learned world. In some few instances double entries must be made of so-called special societies, such as those of natural history, the titles of which must be referred to under botany, and under zoology also. But they are few in number.

Class II. Meteorology is a science with a literature of its own, and must therefore have a place of its own; but its *disjecta membra* give much trouble, and would be thrown by a close criticism into several divisions or subdivisions. Its observations are almost always made in connexion with astronomy; some of its meteors are cosmical, and come under a sub-section of astronomy (2^{2n}); others are atmospheric, and come under another sub-section of astronomy (2^{2o}); others are of a so-called physical kind, and would come under magnetism, &c., (2^4); others are mineralogical, and connected with geological phenomena, and would come under 3^3 and 3^4. It has been placed in this class and made a correlative branch of its second division with astronomy, because of their practical connexion in the observatory, and because of their literal connexion in the proceedings and transactions of academies and societies. Magnetism would follow it into the same position were it not for the fact that all the earlier literature of magnetism is bound up with that of light, heat, and electricity, and the other subjects of physics so called; and the only practical connexion that it maintains with astronomy and meteorology, that of the observatory, is not universal, and is not so close as that which it maintains with geodesy.

Navigation also is so connected with astronomy and meterology that it finds its proper place with them. As a science it is not to be confounded with the arts in which it has become embodied, even the special art of ship-sailing, which lies nearest to it. Its enlargement in modern times with special practical reference to steam (6^2), and its more general relationship to commerce (6^3), has caused the titles of a few of its books to be duplicated under those heads.

Mechanics, in the order of sciences, should come under mathematics, as 2^4, but owing to the number of books which go into the details of machinery and describe processes, it has been placed with *applied* mechanics, under manufactures (6^2). When the arrangement was first made out, 2^4 was called mechanics, or technology, and 2^5 physics; but after a multitude of cards had been written with the latter mark upon them, and still none appeared for pure technology, or only one or two, while many were observed to require close connexion with manufactures, the mistake was unfortunately committed of suppressing the division 2^4 entirely. No complete arrangement, however, can be made without it.

Civil engineering occupies an anomalous station. Its correlate among the scientific arts is geodesy (2^3); among the mechanic arts, architecture (7^3). Works on civil engineering, as a science, are extremely few, and these few ought certainly to form a subdivision either with geodesy or with architecture. Yet after trial in both places they were finally grouped with a variety of other kindred matter under manufactures (6^2). For to this apparently alien locality had been also banished the numerous reports on railroads, canals, and works on steam, under the pressure of a hardly describable convenience, which only those will understand who are obliged to handle masses of such literature. The convenience here obeyed was, however, a true index of the natural relationships which this whole branch of literature sustains to the class of the social sciences and arts. But the very epithet of civil, applied to this kind of engineering, places it in the sixth class, with the same certainty that mining engineering goes into the third.

Class III. Chemistry, as the abstract science of molecular life, must be considered the mathematics of the inorganic material world; and it is only at first confusing that the name (which cannot now be changed) means also the practical applications of this knowledge, and all that flows from it, to the arts of life. Chemistry may be as necessary to medicine and to manufactures as to metallurgy; yet it will not be doubted by any man of science that in a correct classification chemistry and metallurgy will go together, and with mineralogy and geology; and will leave medicine and manufactures to find their own, perhaps very distant places, for themselves. Medicine will carry pharmaceutics with it, and under manufactures will go dyeing, salt-making, soap-boiling, and a hundred other chemical arts of common life; but chemistry will still maintain its true position as the opening division of the class of the inorganic sciences, with two chief subdivisions into inorganic and organic chemistry.

Mining engineering might, with some propriety, be grouped with mineralogy in 3^2, as agriculture is with botany in 4^2. But this place is preoccupied by metallurgy, between which and geology the department of mining engineering practically and naturally interferes. It has therefore been made a division by itself, 3^3.

Palæontology, merely as a transition from the study of the inorganic to the study of the organic, might be placed either at the end of the one or at the beginning of the other class. Its essential spirit allies it with Class IV of the organic world. Its literature, however, is so entirely geological that no room is left for questioning, and it cannot even form a separate division, but must be grouped with geology, in 3^4.

Class IV. Natural history (4^1) should be called biology, or the science of organic life, as chemistry is the science of inorganic life. Its specifications follow in botany (4^2 A), zoology (4^3 A), and human physiology (4^4 A), with which again are grouped their practices in life, agriculture (4^2 B), acclimation (4^3 B), and surgery, medicine, &c., (4^4 B.) But there are many books of mingled botany and zoology, especially those written in or previous to the revival of natural science in the eighteenth century, which either discuss the nature of life, or systematize the phenomena of life in so general a way that it seems best to retain the old name for the science of organic life in general for the first division of the class, and to put all such memoirs and synopses into it.

It would have been easy to have adopted the name of physiology for this leading division, had it not become engaged to a special branch of biology, although some have endeavored to distinguish physiology from human physiology. . The name, zoology, has been still more closely confined to a specialty, and cannot now be released to assume its natural place at the head of the sciences of organic life. Natural history, therefore, vague and unsatisfactory as the name is, seems to be the best within our reach to designate not only purely biological works, but works of general description and classification. Its subdivisions, then, ought to be into three, natural history societies' publications, biological treatises, and principles of classification of genera and species.

Acclimation (4^3 B), with the few books treating of it, was at first considered only an insignificant or accidental part of agriculture. But having attained the rank of a self-sustaining science, lackeyed by one of the most powerful societies in France, la Société Zoologique d'Acclimatation, it must be allowed to assume its normal place beside zoology, making this fourth class the most symmetrical one of the eight.

An apparent anomaly, however, will be noticed by naturalists in the order of subjects under some of the divisions, such as botany and zoology; they descend instead of ascending. But this is an inherent and unconquerable difficulty in the sciences themselves, forcing itself upon the classification of their books. In geology, especially, the order of time and of running description is from lower to higher rocks; but the order of illustration and of minute description is from above downwards, both in nature, in the field-book, and on the printed page. Hugh Miller has made it an argument for the orthodoxy of the fall of man and eternal damnation, that nature involves this very anomaly, and has seen herself obliged by the creative destiny to usher in her several creations of higher and higher types *per saltum*, only to mortify herself with the sight of their relapses into degradation and decrepitude, followed by extinction.

Class V. With the completion of the physico-organic we enter the organo-spiritual range of sciences. And here is encountered perhaps the principal difficulty in developing the whole theme on a consistently advancing and ascending scale. We have seen it take root in the abstract soil of number, form its stem and branches of the inorganic world, blossom with forms of organic life, and bear its fruit in man. There remains the discussion of the varieties, the uses, and the reproduction of this fruit. A new theme thus arises out of the body of the old, like a star-fish from a jelly-fish, to repeat, with a distincter and nobler pronunciation, the fading outlines of the mother theme. The practical question may be put in several ways.

We have ceased to regard mankind physiologically as animal. How shall we now consider man as personal? Shall we consider him, first, intellectually as a mechanic, then æsthetically as an artist, then morally as an immortal? Many would prefer this order; and it has an outward show of regular development. But, when examined closely, it is seen to confuse the natural system. It is empirical. It establishes a doubtful precedency for one kind of art before another. It leaves several chief heads, such as law and philanthropy, to say nothing of political economy and the whole historical department, unprovided for, and is therefore incomplete.

To state the question in another way: Having considered mankind in nature, shall we next consider mankind in society, and afterwards the individual person? This also, although a reasonable order, is practically too large and crude. Something more specific and precise is needful.

Having considered mankind as a physiological idea, correlative with but generically (or ordinally) distinct from and superior to the rest, and in fact closing up the statement of the whole organic world, we pass to the consideration of the realizations of this highest physiological idea in time and in space, which are its two most abstract and universal formulæ. In other words, having described the earth, and its genera and species of inhabitants, and mankind as one of these, we arrive at the description of the relations which this mankind holds to the world so inhabited—relations first of time and also of space. Now then come up in proper series all those questions of the origin and the migrations of human races, to settle which exist the sciences of chronology, ethnology, archæology, mythology, and general history. That these questions involve discussions of intellectual and spiritual phenomena is true, but only by the way, and incidentally. Their sciences are essentially humano-terrestrial, and only prepare the way for the nobler social and moral sciences. Chronology (5^1) leads off, because it is the mathematics of the class, and on its deductions ethnology (5^2) relies. Archæology (5^3 A) follows, bringing forward with it, and retaining at its side, its protégé mythology (5^3 B), in spite of the kindred ties between the latter and religion (7^5). History (5^4) sums up the whole, and invites attention to the next great group.

The only serious difficulty met with in reducing these ideas to practice occurs in the matter of historical documents (5^4 B), which form so large a collection in this and other libraries. Their proper place, in the most perfect system, is not exactly with general history, for they are chiefly the records of single acts and individual lives, and illustrate much higher relationships than that of man with earth. But, on the other hand, the distinction between history and its documents is obscure. Historical text-books, monographs of particular eras, reigns, and individual events, graduate insensibly into pamphlet forms; while bound volumes of historical addresses, rare political squibs and speeches in Congress or in Parliament, can, after all, stand nowhere in a library so usefully and naturally as on the shelves of history.

Class VI. Sociology (6^1), including, of course, statistics, holds the same relation to the sciences of affairs in the world of men (on which we now enter) that mathematics holds to the sciences of measurement in the world of number, that chemistry holds as the science of molecular arrangement in the world of matter, and that biology holds to the sciences of individuality in living beings. Furthermore, its relation to the world of the present is the same as that of chronology (5^1) to the world of the past. Its questions which now meet us are those of man with man. It leaves to the last-named class all those questions of preliminary fact respecting man and the earth. It takes the past for granted, and proceeds to determine the values of societies of men, considering their status on the earth, their accumulations of industry, the wealth of nature at their command, the energetic forces of invention and association, and the intelligent self-construction and self-preservation of society. Manufactures (6^2), commerce (6^3), war (6^4), and law (6^5), are its four groups of phenomena, coming in their natural order of advancing intelligence. The legal science is the logical consummation of this class, as religion concludes the next.

Simple as is this arrangement in the whole, there are many obstacles to its perfect application in detail, but none of them insurmountable.

Financial science, for instance, including pamphlets on free trade and tariff, stands intermediate between manufactures and commerce, dealing with both. Its treatment is, however, necessarily fundamental, involving the most recondite principles of sociology. It is sociology in one of its governmental aspects. It has, in all ages, been considered as the main body of the science of government. It is in fact political economy. Unfortunately for the world, but fortunately for the arrangement of libraries, benevolence and religion are not considered sufficiently allied to politics to make any close connexion between the sixth and seventh classes necessary, except through law and language.

There is still another reason for throwing finance into the first and general division of political economy (6^1), as books which cannot go either into botany or zoology are placed in natural history.

Manufactures and the mechanic arts (6^2), as has been already said above, are grouped together, and involve the building of steam-engines, steamboats, canals, and railroads, and therefore the discussion of steam as a power.

For a similar reason, under commerce (6^3) come the subjects of money, coins, and medals, although the science of money value belongs with finance in political economy, and medals ought to go into archæology and history.

Legislation is in like manner grouped with law (6^5), although it carries a far wider range, and is, in fact, the science of applied sociology—the effort to embody social ideas *of every kind* in statute form. Its relation to history, also, is very intimate.

While this sixth class is apparently the simplest of all in its arrangement, it is in reality the most confused and difficult to adjust, as it is by far the most important of all in the number of its titles.

Class VII. The class of sciences of which language forms the first division (7^1), places us, on leaving the world of the social sciences, in the world of man's highest and largest relationships—as an individual, with other beings as individuals, and with ideas as if they were individual beings. Sociology is the language of societies; language is the sociology of man's allied intelligences. The science of language is the mathematics of the soul. Language, as the analogue of pure mathematics, is the science of man's power to express his thoughts and feelings. Its applications, therefore, are to belles-lettres (7^2), the fine arts (7^3), ethics (7^4), education (7^5), and worship or religion (7^6.) These are all parts of speech. They are the utterances of successively higher and higher elemental and essential forces of the being. Contracting their areas as they ascend, they terminate in a highest point, where the one man regards the one God. This is the end of the sciences. Here is no more language, but silence. Nothing can follow but retrospection and personal narration, which is biography, destined to stand by itself, as Class VIII, at the end, as general science stood by itself, as Class I, at the beginning.

Under ethics (7^4) are put books of metaphysics, so called for the convenience of the consulter. They strictly belong in language, being nothing else than books of the natural history of the mind; but no one would be likely to look for them there. Some may object that metaphysics has not been named the first division of the class of spiritual sciences. If the word had not been perverted from its best and widest meaning, and reduced to the denomination of a specialty, the mere classification of the faculties of the mind, it might have stood in that position. But even then the symmetry of the arrangement would have been lost—the opening of language, the advance through poetry, sculpture, music, and logic, to ethics and the works of virtue, to metaphysical understanding, to Christian faith and hope, and to the personal intercourse with God in praise and prayer.

Under education (7^5), the instruction of the deaf and dumb, the blind, and the idiotic, are placed with that of other kinds; but the treatment of the hopelessly insane could not reasonably be placed anywhere but with medicine, in 4^4. This is one of the cases of forcible divorce of classes of books commonly kept together.

Prison discipline, also, (which some might expect to find in the social class, VI,) and, in fact, philanthropy in general, is grouped with education (7^5). Others might insist upon making a distinct division; but any one who handles the literatures of both classes will find it practically impossible to separate them more widely.

After this analytical statement of its genesis, it is only necessary to reproduce the whole scheme before the reader's eye and leave him to make what use of it he can. It is no just objection to any good arrangement of a library that it requires study to be used. It would be unskilful, indeed, if it did not. Students of books must learn the contents of each book by studying the author's arrangement. How much more needful to learn the contents of a library by a careful analysis of its departments! It is the librarian's duty to save a large part of this labor to the consulters of the library, by a more complete and conscientious analysis than any they can find time to make. The least that they can do is to become accustomed to this analysis when made. Libraries are commonly arranged without, or previous to, any analysis, and in obedience to local accidents or temporary expediency; and, in the case of those which crowds of readers throng, it is not to be so much wondered at. But in quiet libraries it is always possible to collect books of one subject into one place, that the reader may have the entire treasury of that theme before his eyes.

The eight classes of our books are thus collected into eight suits of bookcases, as their titles on cards are arranged in eight drawers. To facilitate the handling of the books, they are also spotted on the back with paper patches of eight different colors, corresponding to the eight suits of bookcases; and each different drawer of the card catalogue is filled with cards of a corresponding color. It is not easy, therefore, for either a card or a book to get astray. The convenience might be extended to the printed catalogue by tinting the pages devoted to each class division with its appropriate color. In the choice of colors there was nothing arbitrary. White being, of course, the color for the first class, general science, the colors of the other seven followed in the order of the solar spectrum:

For mathematics, &c	red	II.
For chemistry, &c	orange	III.
For natural history, &c	yellow	IV.
For chronology, &c	green	V.
For sociology, &c	blue	VI.
For language, &c	indigo	VII.
For biography, &c	violet	VIII.

Under the principal analytical law of arrangement of the library, rule two others—the one a law of space, the other a law of time.

Wherever a *geographical* arrangement could be made out, it was adopted. Such was the case with learned societies and their publications, and the catalogues of libraries; with astronomical observatories and their observations; with books of geography, and voyages and travels; with whole ranges of books in the various physical sciences; with books on ethnology, local history, local manufactures, local laws, and legislation; with books on language, belles-lettres, &c. And the geographical sequence proceeds, like that of history, *from the east westward.*

In all other cases, and in all the sub-sections throughout the catalogue, a *chronological* arrangement is adopted for rapid reference; and, to take the utmost advantage of it, the dates of books are arranged in column on the right side of the page. This column should properly be on the left side of the page, but some allowance was considered due to tradition and the custom of the reader's eye. In consulting a catalogue for a book, perhaps the most natural reference first made is to the time of its appearance. Every catalogue might properly make its statements in the following order: "In this year ( ) a work appeared of this size (°), and in this ( ) number of volumes, entitled thus ( ); its author was M. ——, and he published in such a place; the work is in (vellum, calf, paper, &c.,) and is to be found in the bookcase with this number on its back ( )."

This *chronological* arrangement is practical and handy; and the books of all the principal divisions of the library are so arranged on the book-shelves—the oldest at the left-hand end of the uppermost shelf, and the rest in order of their dates. The reader, who has forgotten both title and author's name, can find, with little search, his book, if he only knows the subject of it and about the date of its appearance. Another most important advantage is obtained: there is no need of renumbering the books of a library when thus arranged. By writing the dates upon the spots of colored paper on the back, the end of numbering the books is gained without the annoyance of posting two sets of numbers, one of which means nothing. Interpolation of new books is also easy and natural. The inconvenience of having half a dozen books of the same date is too slight to notice.

The more serious inconvenience encountered in the case of serials, especially in the case of interrupted series of proceedings, transactions, acts, memoirs, and magazines, may be readily overcome by numbering all of a series of one date, viz: the date of the first volume, or, better yet, by placing all the living serials of a division at the end of a suit of cases of that division, where they will not interfere with the chronological arrangement of the books, and where, also, they can be increased by periodical additions.

Of course, in all this, it is understood that the size of books is disregarded, except where a lower shelf or shelves are given to quartos and folios. This may be a fatal objection to the whole plan, in the view of those who are more disposed to please the eye in regarding than to assist the brain in handling a library. But working scholars are soon cured of undue æstheticism in externals, and a little extra height allowed to each shelf space admits even the smaller folios into their chronological places.

It will be noticed that the column of volumes and pamphlets which stands next to the titles on each page of the catalogue, omits any statement of a book or pamphlet, except in that class and division in which the book or pamphlet is actually to be found in the library; otherwise, in summing up the number of books in the library, the same book would be counted as many times as its title happened to need duplication in different parts of the catalogue, and the search for it among the books of the library would also give that much additional trouble.

ACCOUNT OF HUMAN REMAINS FROM PATAGONIA
IN THE SMITHSONIAN INSTITUTION.

PRESENTED BY DR. AQ. RIED.*

The accompanying female mummy was found about two months ago on the west coast of Patagonia, in latitude 44° south, near a point marked on the charts "Refujio bay."

A considerable number of human skeletons and detached human bones were discovered, occupying a species of cavern on the face of the rocks that bind the coast, at an elevation of about one hundred feet above high-water mark, and at no great distance from the beach. Some of the skeletons retained part of the hair, integuments, and soft tissues, in various stages of decomposition; the body under consideration was, however, the only one in a state approaching preservation. Few similar specimens have hitherto been procured—two are in the national museum at Santiago; a third was sent about ten years ago to the museum at Ratisbon, in Bavaria, by the writer of these remarks; and the fourth is the one herewith presented to the Institution.

That *few* human bodies should be met with in these regions, even so imperfectly preserved as the one in question, is not to be wondered at, if we take into account the *climate*, so peculiarly unfavorable to the preservation of animal fibre, on account of the quantity of moisture with which the atmosphere is impregnated.

We possess no reliable observations on the temperature of the district, but navigators and hunters agree in stating that *ice* is a rare occurrence, and that snow never remains long on the ground. The winter consists of a scarcely interrupted series of gales, with heavy rains, and lasts for upwards of six months of the year. Although sheltered from the direct action of the snow and rain, the bodies lay exposed to the indirect influence of atmospheric changes—the caverns being of no great depth, and the bodies completely uncovered.

The question naturally presents itself, From what cause have these bodies resisted the decomposing action of putrefactive fermentation? Are there any local natural causes to explain a phenomenon which appears in contradiction with what might be expected under ordinary circumstances, but particularly under those mentioned? I am not aware that any notice has been taken of this fact by any writer on natural history; and yet it appears sufficiently interesting to deserve attention.

From the northern border of Patagonia up to the southern termination of the great desert of Atacama, the human body, after death, goes through the usual process of decay. No doubt in Patagonia the same result takes place, but there are evidently numerous exceptions to the rule, as many skeletons are met with on which the soft parts are only exsiccated, tendons and muscles adhering to the bone, in a state of semi-preservation.

To suppose that the bodies had been made to undergo some preparation designed to preserve them, would be to assume the existence of a state of civilization which no collateral evidence warrants us in doing. Besides, the bodies themselves present no traces of the employment of any artificial means for such a purpose.

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* See page 87 of this Report.

The *sitting* posture in which these bodies are found, and which is peculiar to the tribes that inhabited the countries comprised in the ancient Inca empire, indicates that they were not intended to be buried under ground, but to be deposited in some situation where they might be accessible to their friends. Some implement of domestic or warlike use is generally found in the immediate neighborhood of the body, as, in the present case, the rude attempt at a *cutting instrument*, fashioned out of stone, and the *dish*, consisting of the outer shell of a kind of calabash.

There are not wanting those who, determined on adapting all things to their favorite theories, choose to discover analogies between the mummies of Egypt and South America, and to deduce therefrom the direct connexion of these tribes with, if not their descent from, the inhabitants of the Old World. Nothing can be more vague or void of foundation. The one is the result of an artificial refined religious superstition, the other has been forced upon man as a consequence of peculiar local circumstances. The only analogy between them is that they are both intended as an homage to the dead. The feeling that led the Pharaohs to build pyramids, and the Moguls to erect mausoleums, is the same that induced our rude savage to lay these harmless utensils at the feet of his departed friend.

In the rainless regions of the west coast, nearly all of which are contained within the Inca empire, many local circumstances combined to direct our attention to this otherwise anomalous method of treating the dead. The atmosphere is excessively dry, the soil impregnated with alkaline matter, nitre, and soda, in varied combinations, almost everywhere in abundance, thus modifying the process of putrid fermentation, so as to render it exceedingly slow, or to suspend it entirely. Animals may be seen lying unchanged in many parts of the country for years after their death, and what more natural than a desire to preserve the dead when it could be done so easily? In Chili proper, on the other hand, and in Araucania, which intervene between Peru and Patagonia, tradition and actual observation proves that the custom of thus preserving the dead has never prevailed. Indeed, without the employment of artificial means it would be impracticable, and the aborigines of these districts buried their dead in the manner as practiced by the majority of nations.

On these premises how is it to be explained that the mummies recur in the isolated locality where the one before us, and several others, have been found, separated as they are from the mummy races by a large intervening space and a people that differ entirely from them in this important social feature? And how can we account for the connexion which this peculiar custom would lead us to suspect between the Inca Indian and the remote Patagonian?

The *dimensions* of the bones of our mummy are considerably above the average of those of the surrounding tribes, and even of the majority of the present inhabitants of Patagonia. The skeleton measures, even in its present shrivelled condition, fully five feet five and a half inches English, which, allowing for the disappearance of the vertebral cartilages, would give, during life, a height of something like five feet eight inches, thus almost justifying the somewhat poetical epithet of "gigantic," as applied to the Patagonians in general. The fair proportions of the lower extremities are particularly striking, as contrasted with the generally abnormal shortness of these members amongst the Araucanian Indians. The entire individual gives the impression of having belonged to a race superior, in bone and muscle, to its neighbors as well as descendants.

The existence of such a race, distinguished by so striking a physical organization, in an isolated corner of the continent, under circumstances certainly not favorable to growth, having to struggle with every kind of privation and to subsist on the poorest of aliments, is a phenomenon which has not attracted the attention of scientific men in the degree it merits. The Inca Indians, (or the mummy races,) to a connexion with whom the Patagonian mummies would point, were rather under middle size, and weakly, in comparison to the latter, although inhabiting a fertile country, with a favorable climate. At the same time, the inhabitants of Tíerra del Fuego, separated from Patagonia only by the narrow straits of Magellan, present an appearance of almost decrepitude. The reconcilement of these apparent contradictions would be well worth the study of ethnologists.

The contact of these races with what is called civilization has not tended as yet to exalt their sense of morality. Some years ago the attention of the Chilian government was directed towards these territories. A German officer of engineers, Bernhard Philippi, explored the region, and having, amongst other things of commercial importance, discovered coal, the government determined on establishing there a penal colony. In 1851 the military commander, Cambiazo, taking advantage of a revolution then raging in the country, amongst other brutalities, committed the one of hanging some six or eight unfortunate Indians, who had been guilty of no other crime than that of being in the colony at the moment. Having quelled the revolution, the government sent Mr. Philippi to organize the colony. He found the bodies of the murdered men still hanging, and at once having buried them, commenced a series of negotiations with the Patagonians tending to appease their fears and their anger. His apparent success was such that trading was resumed, and he was invited by the chiefs to visit the interior. Two years after he had started, it was ascertained that both he and his secretary had been murdered a few days after their departure from the colony, in retribution for the lives sacrificed by Cambiazo.

Although unable to distinguish between the guilty and the innocent, in the first instance, they must have still possessed the sense of right and wrong to a certain extent, and their conscience made such cowards of them that for several years none approached the settlement, and for a long time they could not believe that retaliation would not be practiced on them.

Although furnished with great physical powers, the Patagonians have never been a warlike race; their collisions with the neighboring Indians, and their intestine broils, never assuming anything of a general character. Of their mechanical skill the accompanying adze-shaped stone may convey an idea; their weapons of offence are the rudest imaginable, the principal one being the "bolas," of the Spaniards, or the "lakhi," of the Araucanians.

Of the four skulls which I have the honor to present to the Institution, the one marked No. 3 belonged to an Indian of the "Pampa," the northeastern frontier of Patagonia, who has been killed by the above-mentioned weapon. The "lakhi" consists of two, or sometimes three round stones of the size of a small orange, covered with raw hide, and connected by pieces of thong of the same material. In war they are used of *lead*, and it is evident that the blow, in the present case, has been given by a weapon of the latter description, as the force necessary to fracture the skull by a stone would have caused a much more extensive gap in the bone; whereas the blow of the lead, being conveyed with greater energy, would produce a more circumscribed wound. The individual had been killed in an incursion of the Pampa Indians towards the south, and the skull was procured by the surgeon of the colony. It is difficult and dangerous to collect such remains, as the Indian will sooner forgive you for killing his companion than for abstracting any part of a dead body. I plead this in excuse of the defective state in which the specimens are presented. The skull, marked No. IV, was found about eighteen leagues (fifty-four miles) from the Chilian settlement towards the northeast, and presents a striking difference from the former one. Neither does it resemble many others found in the same region; and the excessive flatness of the superior anterior portion, the great breadth of the posterior inferior region, and the position of the foramen magnum, would lead to the supposition that it belonged to an inhabitant of "Tierra del

Fuego," one of the lowest branches yet discovered of the human family. This is neither impossible nor improbable, as feuds between the tribes are constantly occurring, and the individual may have been dragged into the interior as a prisoner of war. The large size of the temporal muscles points to carnivorous habits.

The skulls, Nos. I and II, are those of two Araucanian Indians, who were killed in the late collision of these tribes with the Chili troops. The number and nature of the sabre cuts and fractures testify to the barbarism of the contest, as well as to the clumsiness of the combatants. An anecdote is connected with them which is rather characteristic. On being exhibited in the custom-house in Valparaiso, the authorities, who had no idea of any scientific object being attachable to the skull, inquired of me what "*martyrs*" they had belonged to. Although both skulls in question belonged to pure Araucanians, still, I do not consider them as specimens of an *unmixed* race. The upper and anterior portion of the brain is but poorly developed, the parietal diameter exceedingly small, and the cerebellar portion preponderating. Yet, the difference between them and the mummy—the one marked No. III—and even the Pampa Indian, is such as to warrant the presumption that the race had been crossed with a superior one, although in a slight degree. The supraorbital processes are, as found in men of violent passions, endowed with a large amount of animal life, and the position of the foramen magnum approaches more to that of the European skull than the purer Indian. The centuries of intercourse with the Spaniards and their descendants, and the consequent introduction of squatters, deserters, and prisoners, besides the not unfrequent abduction of white women, sufficiently explains this intermingling of race. But it will not suffice to explain all that is told to us about these wandering tribes by the Spanish historians and poets, who attribute to them elevated ideas about the immortality of the soul, systems of religion and politics, and noble qualities such as distinguish the most elevated representatives of the human race. On view of these stubborn skulls, however, and even admitting that the crossing with the superior race has not improved them, we must take these assertions "cum grano salis," and that a very considerable one. The two writers on whom the sins of the subsequent compilers may be charged are the poet Ercilla and the Jesuit Molina. The one, a maker of long verses, in which he describes his own deeds somewhat in the vein of "Ancient Pistol," endeavors to exalt his own prowess by praising his enemy; the other, a simple-minded priest, narrates, with an enviable credulity, historical facts in a poetical manner, and without the critical acumen which ought to distinguish the historian. The fables which the credulity of the one and the exaggeration of the other of these two celebrities, let loose upon the world, have been repeated by their compiling successors down even to "Monsieur Gay," who, although paid by the Chili government for writing, amongst other things, a *critical* history of the country and its inhabitants, repeats the received traditions without much inquiry into their soundness. As the history of these races has been thus rendered obscure by the barbarism, ignorance, and superstition of the first invaders, and as their architectural and industrial relics are too few to guide us securely in our investigations, the mummies and osseous remains which are found in great abundance and in varied situations, form a valuable cue to the observer, and may enable him to solve much of what without them would remain unexplained. Hence, I venture to submit the subject to the attention of the lovers of ethnological inquiry.

VALPARAISO, *June* 4, 1862.

PRIZE QUESTIONS OF SCIENTIFIC SOCIETIES.

INSTITUTION OF CIVIL ENGINEERS, LONDON.

SUBJECTS FOR PREMIUMS, SESSION, 1863–'64.

The Council of the Institution of Civil Engineers invite communications on the subjects comprised in the following list, as well as upon others; such as, 1st. Authentic details of the progress of any work in civil engineering, as far as absolutely executed (Smeaton's account of the Edystone light-house may be taken as an example;) 2d. Descriptions of engines and machines of various kinds; or 3d. Practical essays on subjects connected with engineering, as, for instance, metallurgy. For approved original communications on these, or other subjects, the council will be prepared to award the premiums arising out of special funds devoted for the purpose.

1. On the decay of materials in tropical climates, and the methods employed for arresting and preventing it.

2. On the theory of metal and timber arches.

3. On the theory and details of construction of wrought-iron girder bridges.

4. On land-slips, with the best means of preventing, or arresting them, with examples.

5. On the pressure of earth on tunnels, and the conditions which limit its amount.

6. On the theory and practice of artesian well-boring, and of sinking large shafts, as now practiced on the continent.

7. On the results of contrivances for facilitating the driving of tunnels, or drifts in rocks.

8. On the principles to be observed in laying out lines of railway through mountainous countries, with examples of their application in the Alps, the Pyrenees, the Indian ghauts, the Rocky mountains of America, and similar cases.

9. On the best means of preserving railways in Alpine countries from interruptions from snow.

10. On the results of recent experience in iron permanent way.

11. On the principles to be observed in the designing and arrangement of terminal and other railway stations, repairing shops, engine-sheds, &c., with reference to the traffic and the rolling stock.

12. On railway ferries, or the transmission of ilway trains entire across rivers, estuaries, &c.

13. On locomotive engines for ascending steep inclines, especially when in combination with sharp curves, on railways.

14. On the working of locomotive engines in long tunnels, with frequent stations.

15. On the results of the application of Giffard's injector to the boilers of locomotive and other engines.

16. On the working expenses of railways, and the influence on these of the original design and construction.

17. On the results of a series of observations on the flow of water from the ground, in any large district, with accurately-recorded rain-gauge registries, in the same locality, for a period of not less than twelve months.

18. On the construction of catch-water reservoirs in mountain districts, for the supply of towns, or for manufacturing purposes.

19. Accounts of existing water-works, including the source of supply, a description of the different modes of collecting and filtering, the distribution throughout the streets of towns, and the general practical results.

20. On the best means of improving the water supply of the metropolis.

21. On the structural details, and the results in use, of apparatus for the filtration of large volumes of water.

22. On the drainage and sewerage of large towns, exemplified by accounts of the systems at present pursued with regard to the level and position of the outfall, the form, dimensions, and material of the sewers, the prevention of emanations from them, the arrangements for connecting the house drains with the public sewers, the best means of limiting the contamination of rivers from the sewage discharged into them, and the disposal of the sewage whether in a liquid form, as irrigation, or in a solid form, after deodorization.

23. On the results of the employment of steam-power on canals, and of other measures for the improvement of canals as a means of conveyance for heavy traffic.

24. On iron paving, and a comparison of the results attained by it, and by stone block paving, &c.

25. A history of any fresh water channel, tidal river, or estuary, accompanied by plans and longitudinal and cross sections, including notices of any works which may have been executed upon it, and of the effects of the works, particularly of the relative value of tidal and fresh water, and of the effect of enclosures from the tidal area upon the general regime of sluicing where applied to the improvement of the entrance or the removal of a bar, and of groynes, or parallel training walls; also of dredging, with a description of the machinery employed, and the cost of raising and depositing the material.

26. On the results of a series of observations, illustrative of the modifications which the tidal wave undergoes in its passage up and down a river, or estuary.

27. On the construction of tidal, or other dams, in a constant, or variable depth of water, and on the use of wrought iron in their construction.

28. A history of any harbor, or dock, including the reasons for selecting the site, the mode of construction adopted, and the subsidiary works for the convenience of shipping, and for commercial purposes, with the cost, &c.

29. On graving docks and mechanical arrangements having a similar object, with the conditions determining their relative applicability in particular cases, as dependent on the rise of tide, the depth of water, and other circumstances.

30. On the arrangement and construction of floating landing-stages, for passenger and other traffic, with existing examples.

31. On the different systems of swing, lifting, and other opening bridges, with existing examples.

32. On the construction of light-houses, their machinery and lighting apparatus, with notices of the methods in use for distinguishing the different lights.

33. On the measure of resistance to steam vessels at high velocities.

34. On the results of the use of tubular boilers, and of steam at an increased pressure, with or without superheating, for marine engines, noticing particularly the difference in weight and in speed, in proportion to the horse-power and the tonnage.

35. On the relative advantages of the principle of expansion, as applied in the single long-stroke cylinder engine, in the double cylinder engine, and in the three-cylinder engine, and on the adaptation of the two latter to marine purposes.

36. On the principles and varieties of construction of blast engines, with British and foreign examples.

37. On the best description of steam fire-engines, and their power and efficiency, as compared with ordinary hand fire-engines.

38. On the construction of, and the comparative duty performed by modern pumping engines for raising water for the supply of towns, or for the drainage of mines, noticing in the latter case the depth and length of the underground workings, the height of the surface above the sea, the geological formation, the contiguity of streams, &c.

39. On turbines and other water motors of a similar character, and their construction and performance, in comparison with water-wheels.

40. On the present systems of smelting iron ores, and on the conversion of cast iron into the malleable state, and of the manufacture of iron generally, comprising the distribution and management of iron works.

41. On the manufacture of iron for rails and wheel tyres, having special reference to the increased capability of resisting lamination and abrasion, and accounts of the machinery required for rolling heavy rails, shafts, and bars of iron of large sectional area.

42. On the manufacture of large masses of iron for the purposes of warfare, as armor plates, &c.

43. On the construction of rifled and breech-loading artillery, and on the initial velocity, range and penetration of rifled projectiles, and the influence of atmospheric resistance.

44. On the use of steel bars and plates in engine work and machinery, for boilers and for ship-building as well as for bridges.

45. On the use of steel in the construction of locomotive engines, especially with reference to durability and the cost of repairs, in tyres and cranked axles, as compared with iron of acknowledged good quality.

46. On the Bessemer and other processes of steel-making, on the present state of the steel manufacture on the continent of Europe, and on the employment of castings in steel for railway wheels and other objects.

47. On the safe working strength of iron and steel, including the results of experiments on the elastic limit of long bars of iron, and on the rate of decay by rusting, &c., and under prolonged strains.

48. On the transmission of electrical signals through submarine cables.

49. On the present relative position of English and continental engineering manufactories, especially with reference to their comparative positions in respect of the cost and the character of the work produced.

50. Memoirs and accounts of the works and inventions of any of the following engineers: Sir Hugh Middleton, Arthur Woolf, Jonathan Hornblower, Richard Trevithick, William Murdoch (of Soho,) Alexander Nimmo, and John Rennie.

Original papers, reports, or designs of these or other eminent individuals are particularly valuable for the library of the institution.

The competition for premiums is not confined to members or associates of the institution, but is equally open to all persons, whether natives or foreigners.

The council will not consider themselves bound to award any premium should the communication not be of adequate merit, but they will award more than one premium should there be several communications on the same subject deserving this mark of distinction.

The communications must be forwarded, on or before the 1st of January, 1864, to the house of the Institution, No. 25 Great George street, Westminster, S. W., where copies of this paper, and any further information, may be obtained.

CHARLES MANBY, *Honorary Secretary.*
JAMES FORREST, *Secretary.*

25 GREAT GEORGE STREET,
Westminster, S. W., August, 1863.

Extracts from the minutes of council, February 23, 1835.

The principal subjects for which premiums will be given are:

1. Descriptions, accompanied by plans and explanatory drawings, of any work in civil engineering, as far as absolutely executed; and which shall contain authentic details of the progress of the work. (Smeaton's account of the Edystone light-house may be taken as an example.)
2. Models or drawings, with descriptions of useful engines and machines; plans of harbors, bridges, roads, rivers, canals, mines, etc.; surveys and sections of districts of country.
3. Practical essays on subjects connected with civil engineering, such as geology, mineralogy, chemistry, physics, mechanic arts, statistics, agriculture, etc., together with models, drawings, or descriptions of any new and useful apparatus, or instruments applicable to the purposes of engineering or surveying.

Excerpt by-laws, section XIV, clause 3.

Every paper, map, plan, drawing, or model presented to the institution shall be considered the property thereof, unless there shall have been some previous arrangement to the contrary, and the council may publish the same in any way and at any time they may think proper. But should the council refuse or delay the publication of such paper beyond a reasonable time, the author thereof shall have a right to copy the same, and to publish it as he may think fit, having previously given notice, in writing, to the secretary, of his intention. No person shall publish, or give his consent for the publication of any communication presented and belonging to the institution, without the previous consent of the council.

Instructions for preparing communications.

The communications should be written in the impersonal pronoun, and be legibly transcribed on foolscap paper, about thirteen inches by eight inches, the lines being three-quarters of an inch apart, on the one side only, leaving a margin of one inch and a half in width on the left side, in order that the sheets may be bound.

The drawings should be on mounted paper, and with as many details as may be necessary to illustrate the subject. Enlarged diagrams, to such a scale that they may be clearly visible, when suspended on the walls of the theatre of the institution, at the time of reading the communication, should be sent for the illustration of any particular portions.

Papers which have been read at the meetings of other scientific societies, or have been published in any form, cannot be read at a meeting of the institution. nor be admitted to competition for the premiums.

PROGRAMME

OF THE

PROVINCIAL SOCIETY OF ARTS AND SCIENCES OF UTRECHT.

1862-'63.

The society has awarded no medal for the memoirs which have been offered since its general session in 1861. The decisions which have been pronounced on the four memoirs presented during that period by learned strangers are here summarily given:

1. A memoir in the German language *on the heat of plants.*

Device: *Etenim experimentorum, etc.*

The author of this memoir has not placed himself at the point to which the question had been carried by researches previous to his own. He has, moreover, employed for his experiments a method which, in the present state of science, seems insufficient for the profound study of the question.

2. A memoir in Latin *on the veracity of Cæsar.*

Device: *Quanta majora erant, etc.*

This memoir has furnished but a slight sketch of the subject—a sketch which is neither recommended by the order nor logic of its discussions. The results of researches respecting the opinion of Pollio are not subjected to a judicious and scientific appreciation. There is even no mention made of the recent attempts which have contributed to elucidate this difficult question. Besides, the Latin style of the author has given occasion to no slight animadversion.

3. A memoir in German *on ventilation.*

Device: *Luft mehr luft.*

It was acknowledged that this memoir is not without merit. Yet it is deficient in a critical discussion of the most important elements of the question: the manner in which the enclosed air is altered, and the influence which the materials of constructions exert upon the working of the ventilating apparatus.

4. A memoir in French on the same subject with No. 3.

Device: *Il faut savoir une fois pourtontes, etc.*

This memoir treats only of the means of preserving the air in its state of purity, while the question propounded on the nature of the alteration which the air undergoes in dwellings is passed by without discussion.

The new questions proposed for competition, the subject of which may be of interest to learned foreigners, are the following:

1. An exposition of the principles of sound policy which, dating from the nineteenth century, have prevailed in the relations of Holland with its East Indian possessions, especially the effect of securing an efficacious protection against every sort of oppression, as well to the colonists as to the native population. A comparison in this respect of English and French laws and ordinances with those of the colonial system of Holland.

2. History of the coinage of money among the Greeks.

3. A memoir on the *inhibitory nerves*, (nerfs inhibitoires.) It is requisite that the author should not limit himself to a critical review of the opinions already delivered on this subject, but that he should illustrate it by new experiments.

4. A critical exposition of the principles and results of the method of which Niebuhr has availed himself in explaining the Roman history, and of the influence which the example of this illustrious savant has exercised on historical studies generally.

5. A series of researches on the heat of plants.

6. Researches on the development of one or more species of animals pertaining to the class of Mollusks, to that of Annelidæ, or to that of the Crustaceæ, whose development has not yet been described, accompanied by explanatory figures.

The prize which will be awarded to each satisfactory response will consist of a gold medal of the value of three hundred florins of Holland, (about 600 francs,) or of the same amount in money. This prize will be doubled for question No. 2. The replies must be written in French, Dutch, German, (Roman characters,) English, or Latin, and be addressed, post-paid, before November 30, 1863, to the secretary of the society, M. Gunning, at Utrecht. For question No. 2, competition will remain open till the 30th of November, 1865. The memoirs must be accompanied by a sealed note enclosing the name and address of the author. Accepted replies will be published in the memoirs of the society. Refer for fuller information to the secretary, M. Gunning.

PRIZE QUESTIONS

PROPOSED IN 1862 BY THE ROYAL DANISH SOCIETY OF SCIENCES.

MATHEMATICAL CLASS.

The meridian observations of small stars from the seventh to the tenth magnitude, made by the royal astronomer, Nevel Maskelyne, during a succession of years, but chiefly in 1767–'68, would seem not a little calculated, if vigorously reduced, to extend the catalogues of stars; and as it cannot be doubted that, from their antiquity and the scrupulous care in making them, these observations might be of no small utility to a knowledge of the fixed stars, the society offers its gold medal to any one who shall accurately reduce them, so that the mean places thence resulting, for any particular period, may be catalogued and compared with the positions since assigned by Lalande, Bessel, and Argelander, or with those derived from any other accessible source.

PHYSICAL CLASS.

With the re-agents which we now habitually employ, it is often not practicable to detect with sufficient accuracy and certainty sugar, dextrine, gum, and starch, especially when a small portion of one or more of these bodies is mixed with other organic substances. But as it is in many cases of no little consequence to have subtle and suitable re-agents for distinguishing the bodies above named, the society propounds the question: By what method can it be certainly decided whether or not sugar, dextrine and starch exist in the fluids and tissues of animals?

HISTORICAL CLASS.

Although the ten books on architecture, usually ascribed to Marcus Vitruvious Pollio, are commonly referred, upon external testimony, to the age of the Emperor Augustus, yet, whether we regard delicacy of art and dexterity of construction, or knowledge of letters and style of composition, they seem not to belong to that golden era. Since the objections advanced on this subject have never been satisfactorily examined, nor is the authority which should be conceded to the laws and rules of building prescribed in these books altogether exempt from doubt, the society desires that accurate inquiry should be made, respecting the age of this writer and his sources of information.

In the discussion of the above questions the Latin, French, English, German, Swedish, or Danish language may be used at pleasure. Communications must not be signed with the name of the author, but denoted by some token, and accompanied by a sealed note containing the same token, and indicating the writer's name, style, and place of residence. Competition is not open to the associates of the society inhabiting the Danish dominions. As a prize, the gold medal of the society, equal in value to fifty Danish ducats, will be awarded to the candidate who shall satisfactorily answer any of the questions, except in cases where some other premium is designated.

Answers must be consigned, before the end of October, 1863, to George Forchhammer, corresponding secretary of the society.

PROGRAMME

OF THE

ACADEMY OF SCIENCES OF THE INSTITUTE OF BOLOGNA

IN REFERENCE TO THE

ALDINI PRIZE FOR RESEARCHES IN GALVANISM, FOR THE YEAR 1865.

No memoir having been presented for competition in 1862, the same subject is again proposed; and in view of its great importance, and the no slight difficulties attending it, the prize is increased and its conditions modified as follows:

The muscles and nerves of the frog are seats of electrical currents, which have given occasion to two dissertations crowned by this academy, and elaborated by the chemical professors, Grimelli and Cima, to answer two inquiries proposed for competition for the Aldini prize. On the authority, chiefly, of a very recent publication by M. Budge, professor in the University of Griefswald, the skin is also asserted to be the seat of an electrical current in the frog. The academy, which has always sought to know clearly and accurately how much has been ascertained in point of electricity with regard to that animal from which galvanism had its origin, cannot but desire also to know how much has been since discovered respecting the same, and of course how much should be referred to the last-mentioned current. It proposes therefore the following

INQUIRY.

1st. To examine and explain whatever of consequence has been ascertained by physicists and physiologists, since the above-mentioned dissertations of Professors Grimelli and Cima, respecting the muscular and nervous currents, as well as those of contraction in the frog; and above all, the real importance of the electro-tonic state of the nerves, by no means inconsiderable according to the careful researches of M. Pfluger, but almost nothing in the opinion of the above-named M. Budge. And

2d. To investigate by precise and conclusive experiments whether an electric current really manifests itself in the skin of the frog, and, if the result be affirmative, what are the laws of this current; should it be regarded or not as a physiological phenomena; and has it any dependence on the other currents?

The academy wishes that the analogous facts observed in other animals should not be dissociated from those relative to the frog, but that the former should be referred to and discussed, thus reuniting in one whatever is well-known about the animal economy in relation to the object of this discussion, and within the limit assigned to this competition.

A prize of *two thousand Italian lires* (nearly $300) will be paid to the author of the paper which, under the above-mentioned terms and conditions, shall present, in the judgment of this academy, the best solution of the proposed inquiry.

Memoirs intended for competition must reach Bologna, *postage paid*, within the month of December, 1865, plainly addressed to the secretary of the Academy of Sciences of the Institute of Bologna. This limit is imperative, and therefore memoirs will not be received for competition which arrive after the last day of the month mentioned. The memoirs must be written in Italian, Latin or French, in characters easily legible. The academy requests the greatest exactness in quotations from printed works, and the highest authenticity in the written documents, to which the authors may have recourse for the proof of corroboration of their assertions. Each competitor must countersign his memoir with some epigraph, and accompany it with a sealed note enclosing his name, style and address, the aforesaid epigraph being repeated on the outside. Competitors must use every precaution not to make themselves known, since those who, by some expression of the memoir, or in any other manner, shall divulge their identity, will be excluded from the competition. At the expiration of the above-mentioned term, and judgment having been pronounced according to the regulations of the academy, only that note will be opened which accompanies the accepted memoir, and thereupon the name of the successful candidate will be published.

Prof. GUISEPPE BERTOLINI,
President.
Dr. DOMENICO PIANI,
Secretary.

BOLOGNA, RESIDENZA DEL L'INSTITUTO, 26 *Feb.*, 1863.

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