39TH CONGRESS, 2d Session. | HOUSE OF REPRESENTATIVES. | MIS. DOC. No. 83.

ANNUAL REPORT

OF

THE BOARD OF REGENTS

OF THE

SMITHSONIAN INSTITUTION,

SHOWING THE

OPERATIONS, EXPENDITURES, AND CONDITION OF THE INSTITUTION FOR THE YEAR 1866.

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1867.

IN THE SENATE OF THE UNITED STATES.

MARCH 1, 1867.

Resolved, That five thousand addititional copies of the report of the Smithsonian Institution for the year eighteen hundred and sixty-six, be printed; two thousand for the use of the Institution and three thousand for the use of the Senate; and that said report be stereotyped; *Provided*, That the aggregate number of pages contained in said report shall not exceed four hundred and fifty pages, without wood cuts or plates, except those furnished by the Institution.

IN THE HOUSE OF REPRESENTATIVES.

FEBRUARY, 28, 1867.

Resolved, That five thousand extra copies of the last report of the Smithsonian Institution be printed; two thousand for the Institution and three thousand for the use of the members of this House, and that the same be stereotyped.

LETTER

OF THE

SECRETARY OF THE SMITHSONIAN INSTITUTION,

COMMUNICATING

THE ANNUAL REPORT OF THE OPERATIONS, EXPENDITURES, AND CONDITION OF THE INSTITUTION FOR THE YEAR 1866.

SMITHSONIAN INSTITUTION,
Washington, February 26, 1867.

In behalf of the Board of Regents, I have the honor to submit to the Congress of the United States the annual report of the operations, expenditures, and condition of the Smithsonian Institution for the year 1866.

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

JOSEPH HENRY,
Secretary Smithsonian Institution.

Hon. L. F. S. FOSTER,
President of the Senate.

Hon. S. COLFAX,
Speaker of the House of Representatives.

ANNUAL REPORT OF THE BOARD OF REGENTS

OF THE

SMITHSONIAN INSTITUTION,

SHOWING

THE OPERATIONS, EXPENDITURES, AND CONDITION OF THE INSTITUTION UP TO JANUARY, 1867, AND THE PROCEEDINGS OF THE BOARD UP TO MARCH 1, 1867.

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 1866.

2. Report of the Executive Committee, giving a general statement of the Smithsonian fund, and also an account of the expenditures for the year 1866.

3. Proceedings of the Board of Regents up to March, 1867.

4. Appendix.

Respectfully submitted:

S. P. CHASE, *Chancellor.*
JOSEPH HENRY, *Secretary.*

OFFICERS OF THE SMITHSONIAN INSTITUTION.

FEBRUARY, 1867.

ANDREW JOHNSON, *ex officio* Presiding Officer of the Institution.

SALMON P. CHASE, Chancellor of the Institution.

JOSEPH HENRY, Secretary of the Institution.

SPENCER F. BAIRD, Assistant Secretary.

WILLIAM J. RHEES, Chief Clerk.

RICHARD WALLACH, } Executive Committee.
RICHARD DELAFIELD, }

REGENTS OF THE INSTITUTION.

L. F. S. FOSTER, Vice-President of the United States.

S. P. CHASE, Chief Justice of the United States.

R. WALLACH, Mayor of the City of Washington.

L. TRUMBULL, member of the Senate of the United States.

GARRETT DAVIS, member of the Senate of the United States.

W. P. FESSENDEN, member of the Senate of the United States.

J. A. GARFIELD, member of the House of Representatives.

J. W. PATTERSON, member of the House of Representatives.

J. F. FARNSWORTH, member of the House of Representatives.

W. B. ASTOR, citizen of New York.

T. D. WOOLSEY, citizen of Connecticut.

L. AGASSIZ, citizen of Massachusetts.

RICHARD DELAFIELD, citizen of Washington.

MEMBERS EX OFFICIO OF THE INSTITUTION.

ANDREW JOHNSON, President of the United States.
L. F. S. FOSTER, Vice-President of the United States.
W. H. SEWARD, Secretary of State.
H. McCULLOCH, Secretary of the Treasury.
E. M. STANTON, Secretary of War.
G. WELLES, Secretary of the Navy.
A. W. RANDALL, Postmaster General.
H. STANBERY, Attorney General.
S. P. CHASE, Chief Justice of the United States.
T. C. THEAKER, Commissioner of Patents.
RICHARD WALLACH, Mayor of the City of Washington.

HONORARY MEMBER.

O. H. BROWNING, 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 the organization, and not the converse.

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

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

SECTION I.

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

To INCREASE KNOWLEDGE. It is proposed—

1. To stimulate men of talent to make original researches, by offering suitable rewards for memoirs containing new truths; and,
2. To appropriate annually a portion of the income for particular researches, under the direction of suitable persons.

To DIFFUSE KNOWLEDGE. It is proposed—

1. To publish a series of periodical reports on the progress of the different branches of knowledge; and,
2. To publish occasionally separate treatises on subjects of general interest.

DETAILS OF THE PLAN TO INCREASE KNOWLEDGE.

I. *By stimulating researches.*

1. Facilities afforded for the production of original memoirs on all branches of knowledge.
2. The memoirs thus obtained to be published in a series of volumes, in a quarto form, and entitled Smithsonian Contributions to Knowledge.
3. No memoir on subjects of physical science to be accepted for publication which does not furnish a positive addition to human

knowledge, resting on original research; and all unverified speculations to be rejected.

4. Each memoir presented to the Institution to be submitted for examination to a commission of persons of reputation for learning in the branch to which the memoir pertains; and to be accepted for publication only in case the report of this commission is favorable.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

DETAILS OF THE PLAN FOR DIFFUSING KNOWLEDGE.

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

1. These reports will diffuse a kind of knowledge generally interesting, but which, at present, is inaccessible to the public. Some of the reports may be published annually, others at longer intervals, as the income of the Institution or the changes in the branches of knowledge may indicate.

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

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

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

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

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

I. PHYSICAL CLASS.

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

II. MORAL AND POLITICAL CLASS.

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

III. LITERATURE AND THE FINE ARTS.

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

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

1. These treatises may occasionally consist of valuable memoirs translated from foreign languages, or of articles prepared under the

* This part of the plan has been but partially carried out.

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 with one another.

4. To carry out the plan before described, a library will be required, consisting, 1st, of a complete collection of the transactions and proceedings of all the learned societies in the world; 2d, of the more important current periodical publications, and other works necessary in preparing the periodical reports.

5. The Institution should make special collections, particularly of objects to illustrate and verify its own publications.

6. Also, a collection of instruments of research in all branches of experimental science.

7. With reference to the collection of books, other than those mentioned above, catalogues of all the different libraries in the United States should be procured, in order that the valuable books first purchased may be such as are not to be found in the United States.

8. Also, catalogues of memoirs, and of books and other materials, should be collected for rendering the Institution a centre of bibliographical knowledge, whence the student may be directed to any work which he may require.

9. It is believed that the collections in natural history will increase by donation as rapidly as the income of the Institution can make provision for their reception, and, therefore, it will seldom be necessary to purchase articles of this kind.

10. Attempts should be made to procure for the gallery of art casts of the most celebrated articles of ancient and modern sculpture.

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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

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.

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, PROFESSOR HENRY, FOR 1866.

GENTLEMEN: It again becomes my duty to present to your honorable board the history of another year of the Institution founded on the bequest of James Smithson, and intrusted by the Congress of the United States to your care. In presenting the following account of the events of the year 1866 I am gratified in being able to state that the fire which occurred at the beginning of the previous year, and which, at the time, appeared so disastrous, has, in reality, led to the adoption of measures for the increase and better security of the funds, and of changes of importance to the future efficiency of the means of carrying out the intentions of the benevolent testator.

It will be seen by the report of the executive committee that, by judicious investments and the sale of coin received from England as the residuary legacy of Smithson, as well as that of the annual interest from the United States, not only have the operations of the Institution been maintained, and the reconstruction of the building carried on without any aid from government, but the finances have been improved and are now in a better condition than at any former period. If the petition to Congress to permit additions to be made to the principal on the same terms as those on which the original bequest was received into the treasury of the United States be granted,* then the extra fund, at the present market value of the stocks in which it is invested, will be sufficient to increase the endowment from $515,169 to $650,000, and still leave enough to complete the general restoration of the building, provided the cost of the restoration be limited to $150,000. For these results the Institution is much indebted to the personal exertions and influence of the Chancellor, Chief Justice Chase, to whom constant reference has been had during the past year in the management of the finances of the establishment.

Although the present condition of the fund is a matter of congratulation, yet there is another fact belonging to the history of 1866 of

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* Congress passed an act February 8, 1867, in accordance with the above, allowing the Regents to increase the capital by savings, donations, and otherwise, to a million dollars.

equal if not greater importance, in regard to the future efficiency of the Institution. I allude to the transfer of the Smithson library for safe-keeping and support to the library of Congress. To those who have not fully considered the subject, it might, at first sight, appear that this transfer of a large number of rare and valuable books from the building of the Institution would be attended with serious inconveniences, and be a virtual relinquishment of the control of property procured at the expense of the Smithsonian fund. But it will be evident, on a statement of the facts, that the advantages accruing to the Institution and the public from the transfer far outweigh any inconvenience which may arise on account of it; and that it will tend to increase the efficiency of the funds, while it adds to the security and even facilitates the general use of the library.

Although the removal of the books renders them somewhat less accessible to those engaged in study and researches at the Institution, yet all the current transactions of the scientific Societies of the world, which constitute the principal part of the library, will be still received at the Institution. An opportunity will thus be afforded of noting their contents before they are sent to the Capitol, and such books will permanently be retained as are of most constant reference. In addition to this, the Institution is to have, at all times, the free use of the books of the congressional library as well as those of its own, and for this purpose, and the better accommodation of the public, the library of Congress is to be open throughout the year, with the exception of a month for cleaning and examination. Hence the general student, instead of being debarred any important privilege, will find increased facility for research in having access, in the same building, to both libraries. Again, the east wing of the Smithsonian building, in which the books were deposited, is not fire-proof, and is liable to destruction by accident or the torch of the incendiary, while the rooms of the Capitol are of incombustible materials. This wing was, moreover, filled to overflowing, and a more extended and secure depository could not be obtained, except by another large draught on the accumulated funds intended to form part of the permanent capital. Besides this, by the terms of the transfer, the cost of the care, binding, and cataloguing of the Smithsonian books is provided for by government, and an important part of the annual income of the Institution is thus saved for other purposes.

But the advantages of the transfer are not confined to the Institution. The library of Congress has secured such an addition as cannot be obtained by purchase, since many of the books are presents from the duplicates of the old libraries of Europe, consisting of transactions and other publications of the learned societies of the world, forming a special collection, not only ranking as first in this country, but one of the best anywhere in existence. Neither is it alone the value of the books that have actually been transferred which is to be considered, but also the means which are offered, through our system of exchange, for the perpetual increase of the several series of works which contain the record of the actual progress of the world in all that essentially pertains to the mental and physical development of the human family. The transfer of the Smithson library has furthermore tended to awaken an interest in the library of Congress, which cannot fail, under the energetic superintendence of the present librarian, Mr. Spofford, in a few years to render it worthy of the national Capital. An appropriation of $100,000 has been recommended* by the Joint Committee of the two houses of Congress for the purchase of the library of Mr. Peter Force, consisting of books relative to America. With these additions the library of Congress will be the largest in the United States, and the necessity of a separate building is even already foreshadowed.

But above all, the act authorizing the reception, care, and support of the Smithson library may be considered as an approval by Congress of the general policy adopted and the course pursued by the Institution. It will be recollected that at the commencement a number of influential literary men warmly advocated the expenditure of the larger portion of the income of the Smithson fund in the establishment and support of a library. This plan was opposed on the ground that the Institution by the terms of the bequest should be of a cosmopolitan character, and produce results, the benefits of which would not be confined to one city, nor even one country, and which could not be accomplished by a library. The act of organization, however, was so expressed as to give color to the idea that a large library was intended by Congress, and much complaint was at one time made because more of the income was not expended in the purchase of books. The late legislation of Congress will leave no ground for further cavil in regard to this point, and while the Institution will be relieved from a large expenditure on account of the library, a national collection of books will be formed far more extensive than could possibly be supported by the Smithson fund.

Moreover, while thus relieving the Institution from a charge which has borne so heavily on its resources, Congress has afforded most encouraging evidence of an important advance in public opinion regard-

* An act has been passed in accordance with this recommendation, and the library of Mr. Force is now the property of the government.

ing the right interpretation of the terms of the Smithson bequest. It is substantially a recognition by the national legislature of the fact that the Smithson fund ought not to be burdened with the support of objects which, while they absorb the income, are locally restricted in their influence, and neither essentially connected with the design, nor authorized by the language of the trust. Since Congress has eventually thought proper to assume the care of the library we may cherish the hope that in due time it will also make provision for the separate maintenance of a collection of objects of "nature and art," not unworthy of the National Capital; and that the proceeds of a fund, now generally recognized as having been intended by the testator for objects of a higher order than those confined to local or even national benefit, will be entirely devoted to the system of operations which an experience of nearly twenty years has abundantly shown to be the best and most practical means of realizing the design of the testator.

It is much to be regretted that views of this kind did not prevail at the time of the organization of the Institution, for had this been the case no small amount of the funds would have been saved, and unpleasant discusssion been averted, the tendency of which could only be to obstruct and retard the full development of a system which now constitutes the distinctive and approved policy of the establishment. It is, however, scarcely a matter of surprise that Congress was not, at the time mentioned, prepared to appreciate the significance of the terms of the Smithson bequest, since the world generally has failed to recognize the importance of abstract scientific truths. Although these truths constitute the most important elements of modern civilization, since they give man power and control over the inherent forces of nature, and enable him to render these the obedient slaves of his will; yet, there is even at this time no country, however intelligent it may appear in other respects, that has made adequate provision for the discovery and development of these important principles. Our own legislators can give no better evidence of enlightened views and wise policy than a free acknowledgement of the claims of science, and a liberal provision for its encouragement and support.

The propriety of making provision for the separate maintenance of a national museum, and of relieving the Smithson fund from the burden imposed upon it in this way, will be evident when the fact is recalled that the plan of the present expensive building was adopted to accommodate the museum of the Wilkes' exploring expedition, the care of which had been devolved upon the Institution by the law of Congress. But the Board of Regents, after the building was completed, decided that the keeping of this museum ought not to be a charge on the Smithson fund, and before assuming the care of it asked an appropriation, at least equal to the cost of keeping the articles while in the Patent Office. Congress recognized the propriety of this position, complied with the request, and has appropropriated since the transfer, annually, $4,000. This sum, however, has not sufficed to defray the cost of attendance, even during the period of low prices previous to the war, without allowing for any increase in the number of articles, or for the interest of the money expended on the building.

It is highly gratifying to observe that a more liberal spirit at present exists. Congress has recently made provision, on an ample scale, for the accommodation of an Army Anatomical Museum, and for a Museum connected with the Agricultural Department, and it can scarcely be doubted, that on a proper presentation of the subject, the same enlightened consideration which prompted the appropriations for these kindred objects, will also induce Congress either to appropriate the Smithsonian building to the use of a National museum, under a separate organization and endowment, or else to make an annual grant of money, which, expended under the direction of the Board of Regents, shall be sufficient properly to support an establishment of this kind.

The reconstruction of the building has been carried on during the past year as rapidly as the funds at command and a due regard to the character of the work would allow. The considerations which have governed the building committee have been to render the work entirely stable in regard to materials and construction and thoroughly fire-proof, first completing such parts as were necessary for the safety of the structure, and next those most wanted for use in the operations of the establishment.

One of the most important points to be determined during the year was the kind of roof to be adopted for the main building, and, after full inquiry, the preference was given to one consisting of an iron frame with slate covering. The contract for the iron work was made with the Phœnix Iron Company of Philadelphia, but owing to unexpected delays the frame was not received in time before the setting in of frost to complete the covering with slate. The walls of the large south tower were found in a worse condition than was at first supposed, thirty feet of the height having to be removed and entirely rebuilt, the facing of cut stone being of new material. To secure the front towers and furnish supports for the iron beams and brick arches for the floors, a lining of brick, laid in cement, has been constructed from the bottom to the top. The inner lining of the walls of the upper story of the main building, which was exposed to the fire, has also been entirely renewed with similar materials. The masonry of the whole building is now completed, and it is expected during the next season to finish all the rooms except the main hall over the museum. The future use of this room, which is 200 feet long by 50 feet wide, has not yet been settled. It will not be wanted for the uses of the Institution, and unless devoted to a National Museum or to some other purpose, it will probably remain for an indefinite time, unfinished. The reconstruction of the building, exclusive of this room, will cost about $150,000, and if to this be added the sum previously expended on the erection of the building, we shall have a total of $475,000, of which, at least, $400,000 might have been added to the principal, or expended in the promotion of knowledge.

The superintendence of the work is still under the direction of the architect, Mr. Adolf Cluss, and every part of the construction and all the plans have been critically examined and discussed by General Delafield, chairman of the building committee, who has taken great interest, since his appointment, not only in the restoration of the building, but in all the operations of the Institution.

Publications.—The amount expended for printing during 1866 has been fully equal to that in any previous year, although the number of *new* works which have been issued is less. This has been occasioned by the high price of paper and printing, and by the necessity of issuing new editions of works for which the demand was more pressing.

The following is a list of the works in *quarto*, forming part of the series entitled "Smithsonian Contributions to Knowledge," published during the year 1866:

1. An Investigation of the Orbit of Neptune, with general tables of its motion, by Professor Simon Newcomb, U. S. navy, pp. 110. (Published January, 1866.)

2. Geological Researches in China, Mongolia, and Japan during the years 1862 to 1865, by Raphael Pumpelly, esq., pp. 161, 9 plates, 18 wood-cuts. (Published August, 1866.)

3. On the Fresh-water Glacial Drift of the Northwestern States, by Charles Whittlesey, esq., pp. 32, 2 maps, 11 wood-cuts. (Published December, 1866.)

Of other quarto works in press the following are nearly ready for distribution:

1. Astronomical, Magnetic, Tidal, and Meteorological Observations within the Arctic Circle, by Isaac I. Hayes, M. D.

2. Meteorological Observations made at Brunswick, Maine, for fifty-two years, by Professor P. Cleaveland, from 1807 to 1859.

The memoirs of Newcomb, Pumpelly, and Hayes were fully described in the report for 1865. It therefore remains to give the account of that of Mr. Whittlesey on the Fresh-water Glacial Drift of the Northern States, which will be found at the end of this report.

The following works in *octavo* were also published in 1866:

1. Catalogue of publications of societies and of periodical works belonging to the Smithsonian Institution, (January,) 596 pp.

2. Monograph of American Corbiculadæ, recent and fossil, by Temple Prime, (January,) 91 pp.

3. List of works published by the Smithsonian Institution, (January,) 11 pp.

4. New species of North American Coleoptera, by Jno. L. LeConte, pages 87 to 177, of part I, (April,) 90 pp.

5. List of the Coleoptera of North America, by Jno. L. LeConte, pages 50 to 78, (April,) 30 pp.

6. Check-list of the Invertebrate Fossils of North America, Eocene and Oligocene, by T. A. Conrad, (May,) 45 pp.

7. Review of American birds in the collection of the Smithsonian Institution. By Prof. S. F. Baird. Pp. 321–450 ; (May and June,) 130 pp.

The memoirs actually completed and issued in the year therefore embraced 303 quarto pages and 993 octavo pages, which, with the annual report, 496 pages, makes an aggregate of 1,792 pages printed within the year.

New editions of the following works have been issued during the same period: Allen's Bats; Binney's Land and Fresh-water Shells, parts II and III; Instructions for collecting specimens of Natural History; circular relative to nests and eggs; check-list of shells; comparative vocabulary; ethnological instructions; catalogue of minerals; check-list of fossils ; Draper on the Telescope.

The most expensive octavo publication during the past year has been the catalogue of the transactions of learned Societies and other public bodies, as well as of encyclopedias and other serial works in the library of the Institution. The printing of this work was begun in 1863 and completed in 1866. During its passage through the press proof-sheets were sent to the principal Societies abroad, for the purpose, first, of securing accuracy in the titles; second, of showing the deficiencies in our series, and thus enabling the Societies, as far as possible, to supply them. This work forms a large octavo volume of 596 pages. It has been stereotyped, and copies have been distributed to the principal libraries in this country and Europe, with a view mainly to obtain from them such duplicates as they may possess which may serve to render more complete the Smithson collection of transactions. The catalogue of the works thus obtained will be printed in an appendix, accompanied by proper indexes, when the whole will be issued and more generally distributed as one of the series of volumes of Smithsonian miscellaneous collections. From the stereotype plates an edition of this work has been struck off at the government printing office for the use of the library of Congress. This catalogue is considered the most complete work on the bibliography of publications of learned societies which has yet appeared in the English language.

Another article belonging to the octavo series is a catalogue of the eocene fossils belonging to the series of check-lists of invertebrate fossils of North America, prepared at the request of the Institution by Mr. T. A. Conrad, of Philadelphia. It contains an enumeration of all the species described from the eocene formation up to the date of publication, and is intended to facilitate the labelling of collections and the distribution of duplicate specimens.

The publication of two other parts of this series, prepared by Mr. F. B. Meek, namely, on the miocene, and on the cretaceous and jurassic species, were mentioned in a previous report.

The different series of check-lists published by the Institution have fully answered the purpose intended, in supplying a want long felt by students of natural history; and successive editions from the stereotype plates are required, the cost of which has formed a large item of the expenditure for printing during the past year.

Besides the foregoing articles 92 pages of an addition to the description of new species, and 30 pages of an addition to the list of coleoptera of North America, by Dr. John LeConte, have been printed during the year. These additions bring the works mentioned to the same point in regard to completeness as the first part of the classification of coleoptera by the same author, published by the Institution several years ago.

Another article, which will form a part of the miscellaneous collections, and printed during the year, is a list of all the works published by the Institution up to the beginning of the year 1866, enumerating 203 distinct titles, arranged under the following heads:

		Vols.
General series—Smithsonian Contributions		15
	Smithsonian Miscellaneous Collections	5
	Smithsonian Reports	14

	No. of papers.
Mathematics	1
Astronomy	21
Meteorology	17
Terrestrial Magnetism	11
Physics	3
General Physics of the Globe	2
Chemistry and Technology	3
Ethnology and Philology	16
Microscopical Science	4
General Natural History	3
Physiology	5
Zoology	37
Botany	9
Palæontology	9
Geology and Physical Geography	4
Bibliography	8
Architecture	1
Miscellaneous	1

Of many of the earlier of the separate publications the edition has been exhausted. Of those that remain copies can be procured, at the prices mentioned in the list, by institutions and individuals who do not come within the following rules of gratuitous distribution:

1. They are presented to all learned Societies of the first class which publish transactions, and give copies of these, in exchange, to the Institution.

2. To all foreign libraries of the first class, provided they give in exchange their catalogues and other publications, or an equivalent, from their duplicate volumes.

3. To permanently endowed colleges in actual operation in this country, provided they furnish in return meteorological observations, catalogues of their libraries and of their students, and all other publications issued by them relative to their organization and history.

4. To all States and Territories, provided they give in return copies of all documents published under their authority.

5. To all incorporated public libraries in this country, not included in any of the foregoing classes, now containing 10,000 volumes; and

to smaller libraries, where a whole State or large district would be otherwise unsupplied.

Institutions devoted exclusively to the promotion of particular branches of knowledge receive such articles published by the Institution as relate to their objects. Portions of the series are also given to institutions of lesser grade not entitled, under the above rules, to the full series; and also to the meteorological correspondents of the Institution.

The Reports are of a more popular character, and are presented—

1. To all the meteorological observers and other collaborators of the Institution.

2. To donors to its library or museum.

3. To colleges and other educational establishments.

4. To public libraries and literary and scientific societies.

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

In consideration of the growing expenses of the Institution, and the increasing demand for its publications, it may become necessary to restrict the gratuitous distribution, and to take measures to secure a larger sale by the usual methods adopted by publishers.

Reports.—The usual number of extra copies of the Report for 1865 was ordered by Congress, and, as has been the case for several years past, was stereotyped, so that future editions may be printed if required.

This volume contains, in addition to the report of the Secretary and the proceedings of the Board, the following articles:

A eulogy on General Joseph G. Totten, late Chief Engineer of the United States army, and a Regent of the Smithsonian Institution from its organization, delivered by Major General J. G. Barnard before the National Academy of Sciences; a Memoir of De Blainville, by Flourens, translated from memoirs of the the French Academy of Sciences; a report of the transactions of the Society of Physics and Natural History of Geneva for 1863 and 1864; an original article on the aurora borealis, with illustrations, by Professor E. Loomis; a translation from the German of an article on the senses of feeling and smell; a translation of a course of lectures by Professor Matteuci on electro-physiology; an essay on the palafittes or lacustrian constructions of the Lake of Neuchatel, by Professor E. Desor, of Switzerland, with notes and illustrations furnished by the author; a continuation of Plateau's researches on the figures of equilibrium of a liquid mass withdrawn from the action of gravity; an outline by Professor W. Lilljeborg, of Upsala, of the latest views of ornithologists in reference to the classification and arrangement of the higher divisions of birds, and such as has been adopted in the museum of the Smithsonian Institution; prize questions proposed by various scientific societies in Europe; and an article on the metric system of weights and measures, with tables intended especially for the use of teachers and authors of arithmetics, prepared for the Institution by Professor H. A. Newton.

The translations are from French, German, and Italian scientific publications, not generally accessible to readers in this country, and most of them have been made by C. A. Alexander, esq., of this city, who has devoted much of his time, almost gratuitously, foı a number of years, to this part of the literary work of the Institution.

We have found from experience that to obtain proper scientific translations is a very difficult matter. It is rare to find an individual having so critical a knowledge of languages, united with other requisite qualifications, as to render with fidelity and spirit such discursive popular explanations as are conveyed in the articles in the appendix to the Report for 1866 on the Senses. In this respect Mr. Alexander has evinced much ability. The meaning of the original is not only rendered with great accuracy, but with reference to peculiarities of expression on the part of the authors, and with a critical regard to the precision and correctness of the English.

It is proper to mention in this connection that we are indebted to the Superintendent of Public Printing for greater facilities than usual in the publication of the report, which, on account of the character of the articles, and their translation from foreign languages, requires more than ordinary time and care in revision.

Meteorology.—The system of meteorological observations inaugurated by the Institution, which was much interrupted by the war, is gradually being re-established, and as soon as the expenses for the restoration of the building are diminished we hope to carry it on more efficiently on an improved basis. The meteorological system under the direction of the Surgeon General is also in process of reorganization, and as soon as the military posts are permanently established it will be recommenced with improved instruments, and in harmonious relations with that of the Institution.

The following is an exhibit of the number of observers during 1866, as derived from the list given in the appendix: British America, 6; Mexico, 1; Central America, 3; Bermuda, 1; Alabama, 5; Arkansas, 2; California, 4; Colorado, 1; Connecticut, 5; Delaware, 1; Florida, 4; Georgia, 1; Illinois, 32; Indiana, 12; Iowa, 28; Kansas, 9; Kentucky, 5; Maine, 11; Maryland, 6; Massachusetts, 17; Michigan, 13; Minnesota, 9 ; Mississippi, 5 ; Missouri, 9 ; Montana, 1 ; Nebraska, 3 ; New Hampshire, 7 ; New Jersey, 14 ; New York, 35 ; North Carolina, 4 ; Ohio, 32 ; Oregon, 2; Pennsylvania, 23; Rhode Island, 2; South Carolina, 1 ; Tennessee, 3 ; Texas, 3 ; Utah, 5 ; Vermont, 7 ; Virginia, 2 ; Washington, 1; West Virginia, 2; Wisconsin, 15.

The whole number of observers is 352 ; of these 67 are furnished with complete sets of instruments, including barometer, thermometer, psychrometer, and rain gauge ; 331 with thermometers, 137 with barometers, 72 with psychrometers, and 227 with rain gauges, while ten report the face of the sky, direction of the wind, and casual phenomena without instruments.

The income of the Institution has not been sufficient to furnish instruments to observers, and hence from many of the stations the results are not strictly comparable with one another. There are, however, in almost every part of the country, one or more stations furnished with standard instruments, constructed by James Green, of New York, the observa tions with which may serve to determine the absolute variations of temperature, pressure and moisture at different localities, while the observations from the remainder give results comparable with themselves, and serve to determine the relative character of different seasons, as well as the data necessary for tracing the transmission of waves, as it were, of atmospheric disturbance with regard to wind, pressure and temperature.

The Institution has endeavored to collect the records of all meteorological observations which have been made on this continent, and has succeeded in obtaining a large amount of material, which will serve as the basis of an isothermal map of the country, as well as that of the peculiarities of climate of different sections. A portion of this, that, namely, which relates to the mean temperatures of years and seasons, was to have been published as the second part of the second volume of "Meteorological Results," issued some years ago by order of Congress, but the pressure of business on the public printing office in consequence of the war has as yet prevented the completion of the work. It will probably be published during the next year, either by Congress or the Institution.

Another portion of the material relates to long series of observations made at particular places. Of these we may mention the following : Those made by Prof. Caswell, at Providence, Rhode Island, for twenty-eight and a half years ; Dr. N. D. Smith, at Washington, Arkansas, for twenty years ; Prof. Cleaveland, at Brunswick, Maine, for fifty-two years ; Dr. S. P. Hildreth, at Marietta, Ohio, for forty-one years; Prof. Z. Thompson, at Burlington, Vermont, for twenty-six years; Samuel Rodman, of New Bedford, Massachusetts, for thirty-six years.

Of the series, those of Caswell and Smith have been published in full, and those of Cleaveland and Hildreth are now in the hands of the printer, and will form a part of the sixteenth volume of the Contributions to Knowledge.

The "Monthly Bulletin of the Agricultural Department" still continues to contain a large amount of meteorological information derived from the reports of the Smithsonian observers. In preparing the materials for this a part of the time of Mr. Force, the assistant in charge of meteorology, has been occupied every month. This publication still continues to be of value to the farmer, and of interest to our observers, since it gives them a ready means of comparing their observations with those made in other parts of the country.

The anemometer, which for many years recorded the direction and intensity of the wind on the top of the high tower of the Institution, was destroyed with other instruments in the fire. As soon as the reconstruction of this tower is completed, one of Osler's anemometers will be erected in the same place. The use of one of these instruments, belonging to the National Observatory, has been kindly granted to us by the Secretary of the Navy. A standard barometer, by Newman, belonging to the Institution, now in the possession of Professor Guyot, will also be placed, with other instruments, in the same tower, and the full series of observations resumed.

The break in our series, however, is not of so much importance as at first sight it might appear, since a similar series has been kept up continually at the Observatory, which is a mile west of the Institution.

Exchanges.—The extensive system of international, scientific and literary exchanges, so long carried on by the Institution, has been fully maintained during the year. Eighty-three boxes, containing 1,170 packages, were sent to our foreign agents in 1866, and over 8,000 parcels received from them. These packages, as in former years, contain the publications of the Institution, public documents, transactions of societies and scientific works by individuals, besides specimens of natural history.

Acknowledgments are again due to the various steamship, railroad and express companies mentioned in the last report who have for a number of years contributed materially to the advance of science by the free freights and important privileges granted by them to the Institution in carrying on its system of exchange.

The agents of the Institution still continue to be Dr. FELIX FLUGEL, Leipsic, GUSTAVE BOSSANGE, Paris, WM. WESLEY, London, FREDERIC MULLER, Amsterdam.

Besides these gentlemen, Mr. JAMES SWAIM, now residing in Paris, has been appointed a special scientific agent for the purchase of philosophical apparatus and the transaction of other business, the duties of which he discharges gratuitously and to the great advantage of the Institution. We regret that notice of this fact was inadvertently omitted in the last annual report.

Explorations and collections.—As has been stated in previous reports, it is an important part of the operations of the Institution to encourage, assist and organize explorations for such portions of North and South America as have not been thoroughly investigated in regard to physical geography, climate and natural history. During the past year this part of the operations has been prosecuted with unabated energy, principally under the immediate supervision of Prof. Baird.

Those which have been made directly or indirectly under the guidance of the Institution, and more or less at its expense, are as follows:

British America.—Explorations and collections of specimens by officers of the Hudson's Bay Company in continuation of those of former years, especially those of Mr. Robert MacFarlane on the Anderson river, and of Mr. W. Brass, at Fort Halkett; Jas. Flett, at La Pierre's House; C. P. Gaudet, at Fort Good Hope; William L. Hardisty, at Fort Simpson; Strachan Jones, at Fort Rae; Jas. Lockhart, at Resolution; John Reed, at Big Island; Jas. Sibbiston and Rev. Jas. MacDonald, at Fort Yukon, and Donald Gunn, west of Lake Winnipeg; also in Labrador by Messrs. Henry Connolly and Donald A. Smith.

Russian America.—The explorations of the Collins extension of the Western Union Telegraph Company under Col. Charles L. Bulkley have been continued, and a large amount of interesting matter in regard to the ethnology, topography, and natural history of the country, collected principally by the following gentlemen has been received:

Colonel Bulkley, Captain Scammon, Dr. Fisher, Captain Sands, R. Kennicott, W. H. Dall, H. M. Bannister, J. T. Rothrock, Charles Pease, F. Bischoff, Lt. Davison, &c. Some of the collections in natural history were made by the members of this company in Kamtschatka and the western side of Behring straits.

Pacific Coast of the United States.—The explorations of J. G. Swan at Neeah bay, Puget sound, have also been continued with interesting results, as also those of Dr. C. A. Canfield at Monterey. Other explorations have been those of W. F. Schwartz, San Luis Obispo, Dr. Elliot Coues, San Pedro, and W. H. Dall, at Monterey.

Rocky Mountain Region of the United States.—The explorations of Dr. Coues and Dr. E. Palmer, in Arizona, mentioned in the last report, have been continued and completed. Dr. J. R. McKee, in New Mexico, Dr. Wernigk, in Colorado, have also made collections.

Eastern United States.—The explorations by Dr. H. B. Butcher on the lower Rio Grande, at Laredo, Texas, and by Dr. Redfield Sharp, at San Antonio; J. T. Cleu, at the Louisiana Salt Mines of Petit Anse island; in Illinois, by Joel Reeves, Robert Ridgway and Professor Henry Shimer, have all furnished interesting results.

West Indies.—The explorations here have been those of Charles Wright, in Cuba; A. E. Younglove, in Hayti; Mr. Geo. Latimer and Dr. Henry Bryant, in Porto Rico; Dr. Bryant, in the Bahamas; W. T. March, in Jamaica, and Mr. Allen, in Bermuda.

Mexico.—The explorations in this country have been those of Col. A. J. Grayson about Mazatlan and on the west coast; Dr. Charles Sartorius and Mr. Floretin Sartorius, at Mirador; Professor Sumichrast and M. Botteri, at Orizaba; Dr. Strebel, at Vera Cruz.

Yucatan.—The explorations in Yucatan of Governor Salazar, under direction of Dr. Arthur Schott, have been continued and completed.

Explorations have been made in Honduras by Dr. H. Berendt and D. B. Parsons; in Guatemala, by Mr. Henry Hague, about San Geronimo; in Nicaragua, by Dr. Earl Flint; in Costa Rica, by Dr. A. Von Frantzius, Hon. C. N. Riotte, Julian Carmiol, Juan Cooper and A. Zeledon; in Panama, by Captain J. M. Dow, J. and T. Rhoads, Dr. Totten and Dr. J. P. Kluge; in the Sandwich Islands, by Mr. Valdimar Knudsen. In South America, explorations have been made and collections transmitted by Mr. W. H. Hudson, in Buenos Ayres; Mr. A. De Lacerda, at Bahia, and Hon. A. A. Burton, at Bogota.

It should be stated in this connection that the expenses of the exploration in Honduras of Dr. Berendt have been principally met by contribution from the Chicago Academy of Sciences, Philadelphia Academy of Sciences, Dr. Henry Bryant, and Messrs. Geo. W. Riggs, W. A. Haines, H. Van Nostrand, C. M. Wheatly, R. L. Stuart, Prof. B. Silliman, Robt. L. Swift, J. H. Redfield and Thos. Bland.

For a more particular account of the nature of the contributions in natural history and ethnology reference may be made to the lists and tables furnished by Prof. Baird in the Appendix. From these it appears that the number of entries during the year in the catalogues of specimens received is 7,254, which is about the usual annual average. Of these, nearly 4,500 were of birds.

As has frequently been stated in previous reports, the collections which are the result of the explorations made by the Institution or by the voluntary contributions of individuals, are not, primarily, intended to enlarge its museum, but for distribution to all the principal museums in the world and to furnish the materials for special researches. In accordance with this policy a distribution of over one hundred thousand labelled specimens, including 53,166 species, has been made during the year 1866 to museums and colleges where they were required for comparative investigation or as materials for instruction. Besides these distributions a large number of specimens have been intrusted to individuals for special study, viz: the insects to Baron Ostensacken, Dr. G. H. Horn, Edw. Norton, esq., W. H. Edwards, esq., and H. Ulke, esq.; the shells to P. P. Carpenter, esq., Thomas Bland, esq., Isaac Lea, esq., and George W. Tryon, esq.; the minerals to Prof. T. Egleston; the plants to Dr. John Torry; the bats to Dr. H. Allen; infusorial earths to S. A. Bailey, esq.; birds to Jno. Cassin, esq., Geo. N. Lawrence, esq., and D. G. Elliot, esq.; nests and eggs to Dr. T. M. Brewer; reptiles to Prof. E. D. Cope; fossils to Dr. Jos. Leidy and F. B. Meek, esq.

In all cases in which specimens are presented to institutions or to individuals for investigation, full credit is required to be given to the name of Smithson for the benefits thus conferred.

Library.—During the past year the library has received from exchange and donation—

Volumes:		
Octavo	922	
Quarto	226	
Folio	95	
		1,243
Parts of volumes and pamphlets:		
Octavo	3,123	
Quarto	1,103	
Folio	283	
		4,509
Maps and charts		121
Total		5,873

Showing a large increase over the receipts during the previous year.

The following are some of the larger donations received in 1866:

From the Genevese National Institute, Geneva, 17 volumes, completing the "Memoirs" and "Bulletin;"

Royal Statistical Bureau, Munich, "Contributions to the Statistics of the Kingdom of Bavaria," vols. 2–15;

Society of Emulation for the study of the History and Antiquities of Flanders, Bruges—23 volumes of their publications;

Numismatic Society, London, "The Numismatic Chronicle," 21 volumes;

Ministry of Public Instruction, Paris, "Description of Egypt, published by order of Napoleon the Great," 21 volumes folio, and several other valuable works;

Library of the Parliament, Melbourne, 51 official documents, 1st volume of "Plants Indigenous to Victoria, Australian Mosses, No. 1," and other works;

Society of Agriculture, Sciences, Arts, and Belles-Lettres, of the Department of Indre and Loire, "Annals," 12 volumes;

Friesland Society of History, Antiquities, and Philology, Leuwarden, 10 volumes and 25 pamphlets;

Government of the Netherlands, 34 volumes and 45 pamphlets;

Belgian Entomological Society, Brussels, "Annals," vols. 1–8;

University of Chili, Santiago, 10 volumes;

Charles Kessler, Reading, Pa., "The Naturalist," vols. 1–22, 1774–1787;

Dr. G. J. Fisher, Sing Sing, N. Y., medical journals and other medical books, 77 volumes;

Dr. K. Koch, Berlin, 5 volumes and 336 pamphlets;

Professor C. J. Tornberg, Lund, Sweden, 11 volumes and 8 pamphlets.

From the Royal Library of Dresden we have received a series of 232 original discourses or theses and tracts, most of which were written by Luther, and the remainder by his contemporaries, and nearly all of which were published during the lifetime of the great reformer.

These tracts were referred to Rev. Dr. Morris, librarian of the Peabody Institute of Baltimore, for a translation of the catalogue, to whom we are indebted for the following remarks in regard to them:

"The majority of these brief treatises can be found in almost any entire edition of Luther's works, and are therefore not rare; but what renders this collection interesting to the bibliographer is, that they are all *first impressions*, and not reprints. The proofs were doubtless revised and corrected by his own hand, as most of them were printed at places where he resided. They present specimens of paper and printing which are very creditable to the artisans of that day, ranging as they do from 1518, the year after the Reformation began, to 1546, the year of Luther's death. These writings have come to us in the same type and paper in which they were distributed by thousands over the land at the dawn of the Reformation. While the language in which they are written, both German and Latin, is not as refined as that employed by scholars of the present day, and while the pictorial illustrations are coarse, yet these productions show the extraordinary progress which the typographic art had already made in the early part of the 16th century. Many of them have the title pages ornamented with a broad margin of wood-cut figures, most of them mythological and grotesque, and all curious. They are specimens of the engraving of that day, exceedingly interesting to the student of the history of art, for these are undoubted originals, which collectors of ancient prints prize so highly. A few of them are unskilfully illuminated, probably executed by some incipient artist, who tried his hand on these coarse and cheap wood-cuts. The subjects of the pamphlets are diverse and curious, and the titles of many of those which are controversial, as was the general custom of that day, are expressed in language more forcible than refined."

Under the act of Congress mentioned in the last report, previously alluded to, the Smithsonian books have been transferred to one of the fire-proof rooms in the extension of the library of Congress, where they will be catalogued under the direction of Mr. Spofford, the librarian, and the large number of volumes in sheets will be bound at the government bindery. The continued new accessions to the library from foreign exchanges and otherwise are received and recorded at the Institution as formerly. The accessions from abroad do not alone consist of transactions of societies, but also, as has been shown in this and previous reports, of some of the most costly publications of foreign governments, and rare duplicates in the older libraries of Europe.

Correspondence.—A large amount of labor connected with the operations of the Institution is devoted to correspondence. Besides those relating to official business, hundreds of letters are received in the course of a year, containing inquiries relative to the various subjects on which the writers desire information. If these cannot be forthwith answered, without much research in the Smithsonian library, they are referred to our collaborators, who are experts in the different branches of knowledge, and who can readily supply trustworthy information in regard to the subjects within the range of their special studies.

During the past year we have made references of this kind to the following gentlemen, viz: Dr. Torrey, of New York; Prof. Agassiz, Dr. Gray, and Prof. Wyman, of Cambridge; Dr. Leidy and Mr. Isaac Lea, of Philadelphia; Professors Dana, Whitney, Brush, and Newton, of New Haven; Drs. Woodward and Craig, of the Surgeon General's office; Prof. Schaffer and Mr. W. B. Taylor, of the Patent office; Admiral Davis and Prof. S. Newcomb of the Naval Observatory: and Mr. George Gibbs, and Mr. J. H. Lane of Washington.

Investigations.—During the past year a considerable portion of my own time has been devoted to investigations connected with the revenue service, light-house board, and other branches of the government. These services, as well as those of a similar character in former years, have been entirely gratuitous, and may be considered in some degree as having tended to repay the care bestowed on the Smithson trust by the government in its character of guardian and trustee.

Lectures.—Since the burning of the upper story of the building in which the lecture room was situated, the public lectures have been discontinued. I have given, however, a course on electricity to the telegraphic association of this city, and also accepted, last autumn, an invitation to deliver the opening course of lectures at the Peabody Institute, in Baltimore. I was induced to accept this invitation contrary to my general habit, in order to present, from the experience gained in the management of the Smithsonian Institution, some suggestions which might be of importance in organizing the interesting establishment so liberally endowed by our distinguished countryman. The endowment of the Peabody Institute is $1,000,000, nearly double the original Smithson fund, and sufficient, if the trust be wisely administered, to render the favored city celebrated throughout the world as a centre of literature and science. But I trust I shall be pardoned for saying, that in the case of the organization of a new institution, of which the purposes are not clearly defined, and which is entrusted to a board of trustees, it can scarcely be possible that the best course can be adopted at once. There will always be honest differences of opinion, and these will lead to compromises incompatible with a unity of plan, giving rise to difficulties in the future. The wisest course, therefore, in such cases, is to proceed cautiously, and on no account to adopt a plan which cannot be modified, or which shall call for any considerable expenditure of the principal of the endowment. I may, perhaps be allowed to say, that the publication of the remarks in full would probably be of importance in diffusing information derived from the experience of the Smithsonian Institution, especially wanted at this time, when so many new institutions are about to be organized; but they would be out of place in this report, though they may be given in the appendix or elsewhere.

Whittlesey on the Drift.—The following is an account of the paper on the drift before mentioned which has not yet been described.

The term drift, as employed in geology, includes the collection of gravel, sand, clay, and stones occurring over portions of the continent without stratification or order of arrangement, and which have evidently been transported and distributed without the agency of rivers. For the study of this formation in the territory north of the Ohio river and east of the Mississippi to the national boundary, Mr. Whittlesey claims to have had special opportunities during the past twenty-five years. The length north and south of this area is about eleven degrees of latitude, from the 38° to the 49°, its breadth being quite irregular. Its eastern boundary is a line passing through the middle of the system of North American lakes, and to the west it extends at least to the Lake of the Woods. Over this space he has found what he considers but one formation, belonging to the post-tertiary, wholly of fresh-water origin, no remains as yet having been found of a salt water character, while to the eastward of Lake Erie, in the valleys of Lakes Ontario and Champlain, and the River St. Lawrence, the shells which occur are wholly marine. Further examination he supposes will show that the fresh-water formation overlaps the marine, and is consequently more recent. The thickness of the fresh-water drift is variable, in some places from 600 to 1,000 feet, though it seldom exceeds 200 or 300 feet. The author uses the term glacial drift to describe this formation as expressing what he conceives to be its origin in accordance with the theory originally proposed by Professor Agassiz, viz., an immense sheet of ice, at first principally confined to the pole of greatest cold, but gradually increasing its area by accumulations of snow in winter on its central part, and by the thawing in summer of the surface, the subsequent freezing and expansion of the infiltered water producing a slow but almost resistless motion outward in every direction from the centre. This sheet of ice, like the modern glacier, having fragments of hard stone imbedded in its lower surface, would furrow and grind the rocks underneath—would bear on its upper surface, and within its interior, boulders and fragments of rocks which fell on it in its passage through mountain gorges, or which it tore from their sides, and, in addition to these, would carry with it the materials eroded in its passage; and finally, on melting, would leave the whole in a condition identical with that in which the drift formation is found. This formation, according to the author, may be divided into three members, in the order of superposition, from the surface downward, composed as follows: The first of coarse sand, gravel, loam, and hard-pan, with large boulders from northern rocks, exhibiting little stratification. The second of sand and gravel, less coarse than the preceding, with irregular bands of clay somewhat laminated, and smaller boulders. The third of fine laminated sand, of marly clay of great thickness, of various colors, with few boulders and little gravel. Wherever a sufficient thickness of drift occurs these divisions can be readily traced, and always in the same order. The first occupies the highest points of land, and frequently lies upon the rock formation without the intervention of the other beds, and in it occur moraine hillocks and depressions intermingled with the underlying strata at different points, indicating the movement of ice, which, while it brought materials from a distance, ploughed up and pulverized the previously existing surface.

This upper member of the drift contains the coarsest material and the largest and most numerous boulders, a fact inexplicable on any other theory excepting that of their deposit by the melting of glaciers carrying on their surface the heavier masses. The surface is pitted with cavities extending below the general level ten, fifteen, and even a hundred feet, their outline being rudely circular, and their sides as steep as is compatible with the stability of the soil. In travelling through this region the explorer frequently finds these hollows so near together that he no sooner rises out of one than he is obliged to descend into another. They seldom contain water, but boulders are found at the bottom, on the sides, and on the surface around. In the prairie regions of southern Wisconsin, timber grows within the cavities, as well as on the adjacent surface, in clumps, known as oak orchards. The formation of such a system of depressions of so uniform a character, and over so large an extent of country, while the rocks beneath, wherever uncovered, are found polished and grooved, also indicates the movements and subsequent melting of large masses of ice. In various places there occur patches of boulders from among which the finer materials have been washed away. Near Twin Falls, on the Menomonee river, in Michigan, on the northern slope of a mountain, such a collection occurs nearly a mile across, covering the surface like a pavement with large masses smoothed and polished by attrition. Along the heights of land collections of boulders also frequently occur. After passing northward, above Lake Winnebago, the portion of sand increases, and also the size and number of the boulders, which are mostly of igneous origin.

The second member of the glacier drift is not as readily made out as the other two. In general it is thin, passing into the first member above, and into the third below. Its characteristics are the finer condition of the material and more distinct stratification in the alternate layers of clay and sand. It is seen in its greatest development at the Grand Sable of Lake Superior, east of Grand island, where the coarse sand forms a stratum of from three to four hundred feet in thickness, overlying a thin stratum of clay. The prominent mountains and dunes along the eastern shore of Lake Michigan belong to this member of the drift formation.

The third member consists of the ash-colored, the red, and the blue laminated clays, the difference of color being probably caused by varying portions of oxide of iron. These strata are not, strictly speaking, formed of clay, but of finely comminuted sand, marl, and oxide of iron, with alumina enough to cause adhesion. On the summit of the land whence the streams flow northwardly into Hudson's bay, southerly into the gulf of Mexico, and southeasterly into Lake Superior, the three members of the drift period are conspicuously exhibited—the third occupying the lower level. On Lake Superior the lower member attains its greatest thickness. The rocks on which the glacial drift rests, wherever uncovered, exhibit markings and indications of scouring and striæ with variable distinctness, depending upon the capability of the rock to retain the impressions. In some cases the movement was parallel, or nearly so, to the strike of the strata, and in such instances sandstone beds have been carried away to a considerable depth, leaving long, narrow ridges of material better able to resist the grinding action. The general movement, as indicated by the striæ, was from the northeast to the southwest, the same as that observed in the northern part of Europe. In New England, markings of this kind on the rocks have been observed at 3,000 feet elevation, in Ohio at from 1,300 to 1,400 feet, and at Point Keewenaw, in Lake Superior, at from 1,400 to 1,600 feet. These elevations indicate the great thickness which the glacial ice must have attained. In various parts of the drift, boulders and nuggets of copper occur, which, in some cases, have been transported a long distance southward. Those found near Lake Superior, which have not been carried far from their origin, are of great size, and not as much rounded by attrition as those found at a greater distance. One of the former, from the clay on the west fork of the Ontonagon river, now in the museum of the Smithsonian Institution, weighs 3,000 pounds, presenting angular points.

One of the characteristics which denotes the unity of the drift is the presence of wood, leaves and other vegetable matter. The imbedded timber is the same throughout the formation and is similar to that of the present growth of a more northern latitude, consisting of pine, spruce, willow, and white cedar, the latter being most abundant. At Cleveland, Ohio, the entire trunk of a white cedar, twenty feet in length, filled with proto-sulphide of iron, was found imbedded in the drift eighteen feet below the surface, and many wells in that city are rendered unfit for use by the layer of vegetation through which the water passes.

Animal remains, such as those of the elephant, mastodon, and horse are found in the same formation, in addition to various varieties of fresh-water shells.

Through the western country there are bluffs and terraces composed of solid rock, but besides these there are others composed of boulders, gravel, hard-pan, clay, or sand. The former were the result of geological causes more ancient than the drift, while the latter are contemporaneous with this formation, and are probably due to parallel currents. What are commonly known as lake ridges, according to the author, are not ancient beaches, but the result of lateral currents distributing the drift. All the rivers and streams cut through these ridges and exhibit the drift clay frequently down to the rock below, while the beaches evidently belong to the alluvial period. Ancient lake ridges must not, therefore, be confounded with lake beaches. The distinctive feature of the latter is that they are narrow and steepest on the lake side, resembling terraces.

In the prairie region of Illinois, Wisconsin, Iowa, and Missouri the general surface is very uniform, but little elevated above the northern lakes. A rise of only twenty-six feet in Lake Michigan would turn its surplus waters across the summit into the Illinois river. Over a territory embracing Ohio, Indiana, Illinois, Iowa and part of Kentucky, Tennessee, Missouri, Wisconsin, Michigan, and Canada West, there are no considerable elevations. This space is an extended basin with a rolling surface, in which glacial action, unobstructed in its movement, would tend to produce but little change in the general topography, and therefore the position of valleys, rivers, and lakes is about the same at present as it was before the glacial period. The motion of the ice, however, must have removed a large portion of the broken fragments of the northern rocks to positions further south. The appearance would indicate that, as the glacial period drew towards its close, the transporting material changed from one in which ice predominated to a modified one of ice and water.

Remarks are also made in this paper on the encroachment of the water upon the land, as indicating the slow but constant changes to which the surface of our earth is subjected. The clay bluffs of the lakes are continually worn away by the action of springs, rains, winds, and waves. They are attacked at the base, partially undermined, and finally precipitated into the water with the trees and whatever may be resting upon them. In due time they are spread over the bottom of the lake, forming new strata to be, perhaps, investigated by future geologists. Lakes Erie and Michigan, the shores of which are composed of drift clay, are filling up more rapidly than Lakes Huron and Superior, the coasts of which are more rocky.

Another phenomenon noticed is that of lines of boulders within and parallel to the shores of shallow lakes, presenting, in some cases, the appearance of a rude wall or fence of large stones. For example, in Mille lake, Minnesota, there are lines of this kind rising five and six feet above the water level, and formed of stones far too heavy to have been moved by the action of the waves. More than fifty years ago a similar appearance was described by President Dwight, of Yale College, as observed at Salisbury, Connecticut. The movement of the stones towards the shore was in this case clearly indicated by the troughs left in the bottom of the lake, visible in clear water. The phenomenon is now satisfactorily referred to the expansion of ice in which the boulders are imbedded in winter. This takes place in every direction from the centre of the lake outward, and is exerted in the case of ice of considerable thickness with almost irresistible energy. The stones are carried forward, and on the thawing of the ice are left in their new position.

The last section of the paper refers to examples of erosions, also referable to the movement of the ice of the glacial period. These are particularly exhibited in the great projection into Lake Superior from the southern shore called Point Keweenaw, in which are scooped out depressions, forming the basins of lakes, inlets, and harbors, exhibiting evidence of the direction and intensity of the transforming force, and it is probable that the shore line, and form of the bottom of the great lakes must have been modified by the action of the same cause. This work is illustrated by a map of North America, a profile of the deposits from Lake Erie to the Lake of the Woods, and eleven wood cuts.

Respectfully submitted,

JOSEPH HENRY,

Secretary Smithsonian Institution.

APPENDIX

TO THE

REPORT OF THE SECRETARY.

REPORT OF THE ASSISTANT SECRETARY, SPENCER F. BAIRD, RELATIVE TO EXCHANGES, COLLECTIONS OF NATURAL HISTORY, ETC.

During the past year 763 principal packages have been received at the Institution by the various channels of transportation, and 768 sent out from it—being a considerable increase over the numbers of last year.

The principal statistics of the exchanges will be found in the following tables marked A, B, C, and D:

A.

Receipts of books, &c., by exchange in 1866.

Volumes:		
Octavo	922	
Quarto	226	
Folio	95	
		1, 243
Parts of volumes and pamphlets:		
Octavo	3, 123	
Quarto	1, 103	
Folio	283	
		4, 509
Maps and charts		121
Total		5, 873

The corresponding receipts for last year were 4,206, showing an increase of 1,667.

B.

Table showing the statistics of the exchanges of the Smithsonian Institution in 1866.

Agent and country.	Number of addresses.	Number of packages.	Number of boxes.	Bulk of boxes in cubic feet.	Weight of boxes in pounds.
DR. FELIX FLÜGEL, *Leipsic*—					
Sweden	11	21			
Norway	5	10			
Denmark	11	12			
Iceland	1	2			
Russia	44	49			
Germany	311	332			
Switzerland	33	38			
Special sending in May		120			
Total	416	584	29	203	7,550
FREDERICK MÜLLER, *Amsterdam*—					
Holland	45	53			
Belgium	11	14			
Total	56	67	5	35	1,250
GUSTAVE BOSSANGE & CO., *Paris*—					
France	101	104			
Italy	59	61			
Spain	7	8			
Portugal	3	4			
Special sending in May		40			
Total	80	217	15	105	3,750
W. WESLEY, *London*—					
Great Britain and Ireland	186	207			
Australia	19	19			
Total	205	226	12	96	3,000
Rest of the world	70	76	22	132	2,800
Grand total	827	1,170	83	571	18,050

C.

Addressed packages received by the Smithsonian Institution from parties in America for foreign distribution in 1866.

	Number of packages.
Albany, N. Y.—	
Dudley Observatory	125
New York State Agricultural Society	72
New York State Homœopathic Society	10
Prof. James Hall	46
Ann Arbor, Mich.—	
Prof. A. Winchell	13

Boston, Mass.—	
American Academy of Arts and Sciences	280
Board of State Charities	10
Boston Society of Natural History	231
J. J. Bowditch	1
Dr. B. A. Gould	46
Dr. S. G. Howe	1
Theodore Lyman	20
Buffalo, N. Y.—	
G. W. Clinton	1
Cambridge, Mass.—	
Cambridge Observatory	101
Harvard College	20
Museum of Comparative Zoology	913
Nautical Almanac Office	52
Prof. H. J. Clark	25
Prof. Asa Gray	12
Chicago, Ill.—	
Chicago Academy of Sciences	102
Columbus, Ohio—	
Ohio State Board of Agriculture	88
W. S. Sullivant	1
Dorchester, Mass.—	
Dr. Edw. Jarvis	53
Hillsboro', N. C.—	
Dr. M. A. Curtis	1
Iowa City, Iowa—	
State University of Iowa	12
Janesville, Wis.—	
Institution for the Blind	125
Lowell, Mass.—	
J. R. Francis	17
New Haven, Conn.—	
American Journal of Science	27
American Oriental Society	5
Prof. J. D. Dana	48
Prof. E. Loomis	53
New York—	
American Ethnological Society	6
New York Lyceum of Natural History	114
United States Sanitary Commission	400
Thomas Bland	25
A. A. Julien	9
Baron Ostensacken	1
Philadelphia, Penn.—	
Academy of Natural Sciences	184
American Pharmaceutical Society	31
Dr. John L. LeConte	9
G. W. Tryon	53

Providence, R. I.—	
State of Rhode Island	6
Rock Island, Ill.—	
B. D. Walsh	1
San Francisco, Cal.—	
Prof. W. P. Blake	15
Dr. Newcomb	50
St. Louis, Mo.—	
Academy of Sciences	165
Dr. George Engelmann	30
Dr. B. F. Shumard	9
Dr. A. Wislizenus	90
Toronto, Canada—	
Canadian Institute	5
Dr. C. J. Bethune	5
Washington, D. C.—	
Census Bureau	4
Surgeon General United States army	392
Prof. Newcomb	85
Dr. Wetherill	25
Wilmington, Del.—	
W. M. Canby	2
	4, 137

The preceding tables show a large increase over the numbers of 1865, there being a difference of 1,574.

D.

Addressed packages received by the Smithsonian Institution from Europe in 1866 for distribution in America.

	No. of packages.
ALBANY, NEW YORK.	
Albany Institute	3
Dudley Observatory	10
New York State Agricultural Society	19
New York State Library	28
New York State Medical Society	2
New York State University	2
State Cabinet of Natural History	1
AMHERST, MASSACHUSETTS.	
Amherst College	6
ANNAPOLIS, MARYLAND.	
United States Naval Academy	1
ANN ARBOR, MICHIGAN.	
Observatory	3
AUSTIN, TEXAS.	
State Library	15
BALTIMORE, MARYLAND.	
Maryland Historical Society	2
State Library	12
BLACKWELL'S ISLAND, NEW YORK.	
New York City Lunatic Asylum	1
BOSTON, MASSACHUSETTS.	
American Academy of Arts and Sciences	98
Christian Examiner	2
American Statistical Association	11
American Unitarian Association	3
Boston Christian Register	3
Boston Society of Natural History	197
Bowditch Library	1
Geological Survey of Massachusetts	1
Historical Society of Massachusetts	2
New England Historico-Genealogical Society	2
North American Review	2
Prison Discipline Society	2
Public Library	14
State Library	6
Technological Institute	1
BRATTLEBORO', VERMONT.	
State Lunatic Asylum	1
BRUNSWICK, MAINE.	
Bowdoin College	8
BURLINGTON, VERMONT.	
University of Vermont	1
CAMBRIDGE, MASSACHUSETTS.	
American Association for Advancement of Science	27
Astronomical Journal	2
Harvard College	17
Museum of Comparative Zoology	8
National Academy of Sciences	22
Observatory of Harvard College	14
Perkins' Institution for the Blind	1
CHARLESTON, SOUTH CAROLINA.	
Astronomical Observatory	4
Elliott Society of Natural History	103
Society Library	13
South Carolina Historical Society	5
CHARLOTTESVILLE, VIRGINIA.	
University of Virginia	19
CHICAGO, ILLINOIS.	
Chicago Academy of Sciences	21
CINCINNATI, OHIO.	
Astronomical Society	2
Dental Register	1
Mercantile Library Association	1
Observatory	4
COLUMBIA, MISSOURI.	
Geological Survey of Missouri	9
COLUMBIA, SOUTH CAROLINA.	
South Carolina College	10
State Library	10
COLUMBUS, OHIO.	
Ohio State Board of Agriculture	58
CONCORD, NEW HAMPSHIRE.	
New Hampshire Historical Society	2
State Lunatic Asylum	1

D.—*Addressed packages received by the Smithsonian Institution, &c.*—Contin'd.

	No. of packages.
DES MOINES, IOWA.	
State Library	23
DETROIT, MICHIGAN.	
Michigan State Agricultural Society	10
FRANKFORT, KENTUCKY.	
Geological Survey of Kentucky	7
GAMBIER, OHIO.	
Kenyon College	1
GEORGETOWN, D. C.	
Georgetown College	5
HALIFAX, NOVA SCOTIA.	
Nova Scotian Institute of Natural Sciences	1
HAMPDEN SYDNEY, VIRGINIA.	
Hampden Sydney College	1
HANOVER, NEW HAMPSHIRE.	
Dartmouth College	9
HARRISBURG, PENNSYLVANIA.	
State Library	1
State Lunatic Hospital	1
HARTFORD, CONNECTICUT.	
Trinity College	4
Young Men's Institute	1
HUDSON, OHIO.	
Western Reserve College	1
IOWA CITY, IOWA.	
Iowa State University	27
JACKSON, MISSISSIPPI.	
State Library	15
JACKSONVILLE, ILLINOIS.	
Illinois State Lunatic Hospital	1
Institution for Blind	2
JANESVILLE, WISCONSIN.	
Institution for Blind	1
JEFFERSON CITY, MISSOURI.	
Historical Society of Missouri	1
KINGSTON, JAMAICA.	
Jamaica Society of Arts	1
LEXINGTON, KENTUCKY.	
Eastern Lunatic Asylum	1
LITTLE ROCK, ARKANSAS.	
State Library	167
State University	5
LOUISVILLE, KENTUCKY.	
University	5
MILLEDGEVILLE, GEORGIA.	
Oglethorpe University	19
State Library	13
MONTGOMERY, ALABAMA.	
State Library	15
MONTPELIER, VERMONT.	
State Library	8
MONTREAL, CANADA.	
Geological Survey of Canada	1
Natural History Society	7
NASHVILLE, TENNESSEE.	
State Library	13
State Lunatic Asylum	1
NEW BRUNSWICK, NEW JERSEY.	
Geological Survey of New Brunswick	9
NEW HAVEN, CONNECTICUT.	
American Journal of Science and Arts	46
American Oriental Society	30
Connecticut Academy of Sciences	2
Yale College	16

D.—*Addressed packages received by the Smithsonian Institution, &c.*—Contin'd.

	No. of packages.		No. of packages.
NEW ORLEANS, LOUISIANA.		PROVIDENCE, RHODE ISLAND.	
New Orleans Academy of Sciences	23	Brown University	6
NEW YORK, NEW YORK.		Secretary of State	1
American Ethnological Society	5	QUEBEC, CANADA.	
American Geographical and Statistical Society	25	Literary and Historical Society of Quebec	2
American Institute	8	RALEIGH, NORTH CAROLINA.	
Astor Library	7	State Library	13
Austrian Consulate	1	RICHMOND, VIRGINIA.	
Columbia College	4	Historical Society of Virginia	6
Mercantile Library Association	2	State Library	2
New York Academy of Medicine	3	SACRAMENTO, CALIFORNIA.	
New York Christian Inquirer	2	State Library	1
New York Lyceum of Natural History	52	ST. JOHN, NEW BRUNSWICK.	
United States Sanitary Commission	1	Natural History Society	1
University	3	ST. LOUIS, MISSOURI.	
OLYMPIA, WASHINGTON TERRITORY.		Deutsches Institut zur Beförderung der Wissenschaften	2
State Library	1	St. Louis Academy of Sciences	92
OMAHA, NEBRASKA.		University	3
State Library	1	SALEM, MASSACHUSETTS.	
OXFORD, MISSISSIPPI.		Essex Institute	11
University of Mississippi	1	SAN FRANCISCO, CALIFORNIA.	
PENFIELD, GEORGIA.		California Academy of Natural Sciences	45
Mercer University	1	Geological Survey of California	1
PHILADELPHIA, PENNSYLVANIA.		SAVANNAH, GEORGIA.	
Academy of Natural Sciences	131	Georgia Historical Society	4
American Journal of Conchology	1	SPRINGFIELD, ILLINOIS.	
American Pharmaceutical Association	9	Illinois State Agricultural Society	1
American Philosophical Society	59	STAUNTON, VIRGINIA.	
Central High School	1	Western Lunatic Asylum	1
Entomological Society	8	STOCKTON, CALIFORNIA.	
Franklin Institute	16	California State Lunatic Hospital	1
Girard College	1		
Historical Society of Pennsylvania	4		
Hospital for Insane	1		
Philadelphia Library Company	1		
Pennsylvania Horticultural Society	7		
Pennsylvania Institution for Blind	1		
Wagner Free Institute	2		
PORTLAND, MAINE.			
Portland Society of Natural History	18		
PRINCETON, NEW JERSEY.			
Princeton College	5		

D.—*Addressed packages received by the Smithsonian Institution, &c.*—Contin'd.

	No. of packages.		No. of packages.
TAUNTON, MASSACHUSETTS.		WASHINGTON, D. C.—Continued.	
State Lunatic Asylum	1	Ordnance Bureau	2
TORONTO, CANADA.		Secretary of the Interior	1
Canadian Institute	11	Secretary of the Treasury	1
Observatory	1	Secretary of War	2
University College	2	Topographical Bureau	1
TUSCALOOSA, ALABAMA.		Treasury Department	8
University of Alabama	1	United States Coast Survey	16
UTICA, NEW YORK.		United States Naval Observatory	72
American Journal of Insanity	2	United States Patent Office	136
New York State Lunatic Asylum	1	WATERVILLE, MAINE.	
WASHINGTON, D. C.		Waterville College	2
Bureau of Ordnance and Hydrography	2	WILLIAMSBURG, VIRGINIA.	
Census Bureau	3	Eastern Lunatic Asylum	5
Department of Agriculture	3	WINDSOR, NOVA SCOTIA.	
Library of Congress	3	King's College	4
Medical Department	32	WORCESTER, MASSACHUSETTS.	
Navy Department	2	American Antiquarian Society	6

Total addresses of institutions	169	
Total addresses of individuals	160	
		329
Total number of parcels to institutions	2,205	
Total number of parcels to individuals	498	
		2,703

ADDITIONS TO THE COLLECTIONS OF THE INSTITUTION IN 1866.

The total number of distinct donations in 1866 amounts to 220; the number of donors being 168. This exhibits a considerable increase over the figures of 1865, which are 155 and 102, respectively. The number of different packages received was 318, as compared with 257 of 1865.

The precise character of the collections received will be best gathered from the list of donations given further on. As will be seen they come from many different localities throughout the continent of America; the most important, however, being those from the officers of the Hudson's Bay Company, in Arctic America.

WORK DONE IN MUSEUM AND COLLECTIONS IN 1866.

Much labor has been expended during the year in cleaning the specimens on exhibition, and otherwise repairing the damage of the fire of 1865. The difficulty of preventing the rain and melted snow from coming through the temporary roof has caused great trouble from mould, requiring energetic measures of relief. No mounted specimens have been added to the collection.

As usual the collections received have been regularly entered and catalogued. The following table shows what has been done in this respect:

Table showing the entries in the record books of the Smithsonian Institution in 1865 and 1866.

Class.	1865.	1866.
Skeletons and skulls	6,609	7,100
Mammals	8,416	8,685
Birds	40,554	45,000
Reptiles	6,544	6,582
Fishes	5,588	5,591
Eggs of birds	9,939	10,400
Crustaceans	1,287	1,287
Mollusks	18,103	18,500
Radiates	2,725	2,725
Annelides	110	110
Fossils	5,907	5,920
Minerals	4,940	4,941
Ethnological specimens	1,125	2,260
Total	111,847	119,101

The comparison of the two columns shows that the number of entries during the year in the catalogues of specimens amounts to 7,254, about the usual annual average. Of birds alone the entries were nearly 4,500.

The following table exhibits the distribution of duplicate specimens in 1866, as compared with previous years:

Approximate table of distribution of duplicate specimens by the Smithsonian Institution to the end of 1866.

Class.	In 1865.		Total to end of 1865.		1866.		Total to end of 1866.	
	Species.	Specimens.	Species.	Specimens.	Species.	Specimens.	Species.	Specimens.
Osteology	1	1	64	64	40	40	104	104
Mammals	21	33	770	1,543	24	31	794	1,574
Birds	233	1,038	7,497	11,530	582	756	8,079	12,286
Reptiles	74	126	1,631	2,591	10	18	1,641	2,609
Fishes	750	1,200	2,393	5,149	1	1	2,394	5,150
Eggs of birds	893	2,421	3,603	9,162	96	217	3,699	9,379
Shells*	5,780	11,086	18,212	59,663	49,200	102,551	67,412	162,214
Radiates	......	......	551	727	......	......	551	727
Crustaceans	......	......	1,013	2,516	......	......	1,013	2,516
Marine invertebrates generally	200	550	1,800	5,060	38	92	1,838	5,152
Plants	10,000	12,975	10,000	12,975	3,058	5,328	13,058	18,303
Fossils	2,224	5,319	2,971	7,557	......	......	2,971	7,557
Minerals and rocks	250	600	1,321	5,554	25	25	1,346	5,579
Ethnology	......	......	58	58	92	92	150	150
Total distribution	20,426	35,349	51,884	124,149	53,166	109,151	105,050	233,300

* Of shells 390 sets were made up, and distributed to over 100 different institutions.

From the table it will be seen that the distribution of specimens in 1866 has amounted to nearly as much as in all previous years put together, the number of species being, indeed, greater. This is due to the extensive and final distribution of shells of the United States exploring expedition, &c.

The following assignment of Smithsonian materials for investigation or conservation has been made during the year 1866:

Dr. H. Allen, Philadelphia.—Collections of cheiroptera, especially those gathered by Dr. Coues in Arizona.

S. A. Bailey, Utica—Specimens of infusorial earths.

Thomas Bland, New York.—Collections of land shells, made by Mr. Charles Wright in Cuba, Mr. Strebel in Mexico, Mr. Stearns and Dr. Flint in Nicaragua.

Dr. T. M. Brewer, Boston.—Nests and eggs of American slender-billed oscine birds.

Dr. P. P. Carpenter, Montreal.—Collections of shells, made by Mr. John Xantus at Manzanillo, Messrs. W. H. Dall and Dr. C. A. Canfield at Monterey, California, James G. Swan in Puget sound, and various other west coast series.

John Cassin, Philadelphia.—Icteridæ of the general Smithsonian collection.

Prof. E. D. Cope, Philadelphia.—Reptiles collected by Mr. D. B. Parsons at Belize, Dr. A. Schott in Yucatan, Mr. Kennicott and others in Nicaragua, Prof. Sumichrast in Mexico, and Mr. A. E. Younglove in Hayti.

W. H. Edwards, Newburgh.—Lepidoptera, collected in Russian America, by the Russian telegraph expedition, by Drs. Coues and Palmer in Arizona, and Mr. Botteri and Prof. Sumichrast in Mexico.

Prof. Thomas Egleston—All the unlabelled boxes of minerals.

D. G. Elliot, New York.—Various species of North American birds, to be figured in his work.

Dr. G. H. Horn, Philadelphia.—Various exotic coleoptera.

Isaac Lea, Philadelphia.—Mexican and other unionidæ.

R. Ostensacken, New York.—Mexican diptera, collected by Mr. Botteri.

Edward Norton, Farmington, Conn.—Hymenoptera, collected in Mexico by Prof. Sumichrast.

Dr. Joseph Leidy, Philadelphia.—Various specimens of fossil mammal remains.

George N. Lawrence, New York.—Costa Rican and other birds.

Dr. John Torrey, New York.—Plants of western America, collected by Mr. Brydges, and other botanical specimens.

George W. Tryon, Philadelphia.—Melaniadæ of the United States and Mexico.

H. Ulke, Washington.—Coleoptera, collected by the Russian telegraph expedition.

ADDITIONS TO THE COLLECTIONS OF THE SMITHSONIAN INSTITUTION IN 1866.

Akhurst, John.—Skin of mouse, Brooklyn.

Aldrich, Mr.—Rattlesnake in alcohol, from Maryland.

Allen, C. M.—Series of birds breeding in Bermuda.

Ayres, Dr. W. O.—Types of California fishes.

Bannister, H.—(See *Bulkley*.)

Beadle, Rev. E. R.—Fossil echinoderms from desert of Sinai, and recent shells.

Bell, J. G.—Skins of South American birds.

Berendt, Dr H.—Two boxes zoological collections from Belize.

Bland, Thomas—Fifty species West Indian land shells.

Boardman, G. A.—Box of birds from New Brunswick.

Bode, Mrs.—Skins of South American birds.

Botteri, M.—Birds and mammals of Mexico.

Bradford, U. S. A., Dr. G. H.—Encrinite from New Mexico.

Brass, W.—Zoological collections from Fort Halkett.

Brevoort, J. C.—Specimen of *Tragulus* from Java; died in captivity.

Brigham & Mann, Messrs.—Series of Hawaiian plants.

Bryan, O. N.—Albino blackbird, and fossils from Prince George county, Md.
Bryant, Dr. Henry—Birds of Belize and Bahia
Brydges, Mrs.—Box of plants collected in Chile and Bolivia by Mr. Brydges.
Bulkley, Colonel C. S.—Collections of Russian telegraph expedition, in various departments of natural history, made by Robert Kennicott, Dr. Fisher, Captain Sands, Captain Scammon, W. H. Dall, Henry Bannister, and J. F. Rothrock.
Bulkley, Colonel C. S.—Tusk of mammoth from Behring straits.
Butcher, Dr. H. B.—Collection of birds, &c., of the Lower Rio Grande, Texas.
Canfield, Dr. C. A.—Collection of birds, eggs, and marine invertebrates of California.
Carmiol, J.—Birds and butterflies of Costa Rica.
Clarke, O.—Silicious cinder from burning of wheat bran.
Cleu, J. F.—Large masses of rock salt, portions of tusks and teeth of fossil elephant, and pieces of cane-matting found below the elephant remains, at a depth of thirteen feet, and just above the salt. From Petit Anse island, Vermillion bay, Louisiana.
Chicago Academy of Sciences.—Specimens of *Amblystoma luridum.*
Chute, R.—Infusorial earth, St. Anthony's Falls.
Collier, D. C.—Chalk from a bluff seventy-five feet high, on the Smoky Hill river, Colorado.
Collins, Colonel W. O.—Skull and horns of *Cervus macrotis*, and dress of Indian chief, from Fort Laramie.
Connolly, Henry.—Skins and eggs of birds from Labrador.
Cooper, Dr. J. G.—*Hesperomys californicus*, and skin of hammer head shark, from California; skull of Indian from Santa Catalina.
Cooper, Juan.—Skins of birds of Costa Rica.
Copenhagen, Royal Zoological Museum.—Nine specimens of skins of North European seals.
Corse, Mr.—Two bottles reptiles, Fort Tejon.
Coues, U. S. A., Dr. S. E.—Four hundred skins of birds, and other collections, from Arizona and California.
Creamer, David.—Lignite from near Baltimore.
Curtis, Dr. M. A.—Skins of *Sorex* and *Hesperomys.*
Dall, W. H.—Shells collected at Monterey, and types of *Helix chersonella.*
Dall, W. H.—(See *Bulkley.*)
Darby, Dr. J. E.—*Melaniadæ* from Tennessee river.
Davis, Henry—Fresh-water shells from Iowa.
Dean, Henry C.—Gray fox, (*Vulpes virginianus,*) in flesh.
Dolbech, James.—Gold-bearing quartz from Great Falls of Potomac.
Dorman, O. M.—Shells, Michigan.
Dorr, R. R.—Nest of *Geothlypis trichas* and insects.
Dow, Captain J. M.—Zoological specimens from Panama.
Dow, Captain J. M.—(See *Totten.*)
Dresden, Royal Mineralogical Museum of.—(Through Professor H. B. Geinitz.) two boxes of fossils and minerals.
Ellsworth, Captain E. D.—Piece of oak from the hull of Congress Galley, sunk in Lake Champlain, 1776, near town Panton, Vermont.
Engelmann, Dr. Geo.—Head of surf duck: *Pelionetta perspicillata.*
Fisher, Dr.—(See *Bulkley.*)
Flett, James—Zoological collection from Fort Good Hope, Arctic America.
Flint, Dr. Earl.—Birds, shells, insects, and plants from Nicaragua.
Foreman, Dr. Ed.—Salamander from Maryland.
Foster, Hon. L. F. S.—Scalp of Navajo chief Ganado Blanco, from Fort Sumner, New Mexico.
Frantzius, Dr. A. Von.—Skin of birds of Costa Rica.

Gatchell, Dr. H. T. F.—Dried toad from Kenosha, Wisconsin.
Gaudet, Charles P.—Zoological collection from Peel's river, Arctic America.
Geinitz, Professor H. B.—(See *Dresden.*)
Gilpin, Dr.—Skin of Sorex; Halifax.
Glover, Professor T.—Two mounted birds.
Goss, B F.—Skins of *Reithroden* and *Arvicola*, Kansas.
Grayson, Colonel A. J.—Box of birds of Mazatlan.
Grier, Colonel.—Scale of fish from Loess, near Vicksburg.
Gruber, Ferd.—Box of birds of California, and egg of "Mexican wren."
Gundlach, Dr. J.—*Caereba* and *Myiadestes* in alcohol, from Cuba.
Gunn, Donald.—Zoological collections from lakes west of Lake Winnipeg.
Haldeman, Professor S. S.—Three boxes of fossils invertebrata, and types of American fresh-water shells.
Hardesty, W. L.—Ethnological and other collections from Fort Simpson.
Harper, Professor L.—*Ceratites americanus*, from Alabama.
Hawon, F.—Box of grasshoppers, Fort Leavenworth.
Hayes, Dr. I. I.—Skin of walrus in salt.
Hague, Henry.—Box of birds, San Geronemo, Guatemala.
Heade, M. J.—Head of Savannah blackbird (*Crotophaga sulcirostris,*) Virgin bay.
Hepburn, Jas.—Birds of Nicaragua. Box of birds and eggs, California.
Hill, M. S—Specimens of 17-year locust, Ohio.
Hubbard, Dr.—Specimens of *Helix blandii*, from Cherokee nation.
Hubbard, Samuel.—Keg of alcoholic specimens, and nest of *Mygale*, from California.
Ilges and Sauter.—Skins of South American birds.
Jewett, Colonel E.—Ancient pottery, from Chiriqui.
Jones, Strachan.—Zoological collection from Fort Yukon and Fort Rae.
Kelsey, S. C.—Living spider from Nashville.
Kennicott, Robert.—(See *Bulkley.*)
Kluge, Dr.—Jar of crustacea from Aspinwall.
Knudson, Valdemar.—Collection of birds from the Sandwich Islands.
Krider, John.—Collection of mounted hawks.
Latimer, George.—Pair of living *Loxigilla portoricensis* from Porto Rico.
Lewis, M.—Pachnolite from Greenland.
Little, Major T. E.—Skull and vertebral column of mink, (*Putorius vison.*)
Lockhart, James.—Zoological collection from Great Slave lake.
London, Royal College of Surgeons.—Two mounted sterna of birds.
MacDonald, R.—Zoological collections from the Rocky mountains west of the Lower Mackenzie.
McDowell, U S. A., Major General.—(See *Wallen.*)
MacFarlane, Robert.—Thirty-five boxes of zoological, ethnological and other collections from the Arctic coast of America, between the mouths of the Mackenzie and Coppermine rivers.
McKee, U. S. A. Dr. J. R.—Skin of *Meleagris mexicana* from New Mexico.
Magruder, Miss.—Large moth from Washington.
Mann.—(See *Brigham* and *Mann.*)
March, W. T.—Collection of birds and shells of Jamaica.
Mayberry, Dr. E.—*Arvicola* and osteological specimens from Martha's Vineyard.
Merrick, Mr.—Stone arrowheads from Long Island.
Michener, Dr. E.—Sterna of birds.
Neufchâtel Zoological Museum.—Twenty-seven species of birds, types of Tschudi, Fauna Peruane.
Palmer, Dr. S.—Insects, plants, and fossils of Arizona.
Parsons, D. B.—Series of turtles from Belize.

Parsons, J. H.—Fossil shell from New York.
Pease, U. S. A., Captain W. B.—Two specimens of *Sesia*, Texas.
Philadelphia Academy of Natural Sciences.—Six mounted birds in exchange.
Portland Natural History Society.—Series fresh-water and land shells of Maine.
Potts, John.—Large living moth from Washington.
Prentiss, Dr. D. W.—Skin and skeletons of birds.
Pumpelly, R.—Two boxes of shells from China and Japan.
Putnam, F. W.—*Gasterosteus Wheatlandii*, from Salem.
Randall, F. A.—Five living *Menopoma Allegheniensis.*
Reeves, Mr. and Mrs. Joel.—Eggs of birds from Calumet marshes, Illinois.
Regenhofer, Dr.—(See *Vienna.*)
Rhoads, J.—Collection of birds of Panama.
Ridgway, A. E.—Monstrous chicken.
Ridgway, Robert.—Box of nest and eggs of birds, Illinois.
Ried, John.—Zoological collection from Great Slave lake.
Riotte, C. N.—Antiquities from Costa Rica.
Roome, J. H.—Porpoise skull and bird skins.
Rothrock, J. T.—(See *Bulkley.*)
Royall, Jos.—Marl from Vicksburg.
Salazar, Governor.—Two boxes of zoological collections made by Dr. A. Shcott in Yucatan.
Salem, Essex Institute.—Head of *Diomedea culminata.*
Samuels, E. A.—Goshawk, (*Astur atricapillus,*) in flesh.
Sands, Captain.—(See *Bulkley.*)
Sauter.—(See *Ilges.*)
Sartorius, Dr. Charles.—Zoological collections from the vicinity of Merador, Mexico.
Sartorius, Florentin.—Collections of birds from near Merador, Mexico.
Scammon, Captain.—(See *Bulkley.*)
Schaufuss, L. W.—*Ætherea* from New Grenada, and four types of new coleoptera.
Schee, Dr. H. M.—Box of shells, Iowa.
Schott, Dr. A.—(See *Salazar.*)
Schwartz, W. F.—Birds and eggs, California.
Sclater, Dr. P. L.—Skin of *Peucæa boucardi.*
Shaffer, D. H.—Box of living ferns.
Sharp, U. S. A., Dr. Redford.—Specimens of reptiles, insects, &c., in alcohol, from Texas.
Sibbiston, James.—Zoological collections from Fort Youkon.
Siler, A. L.—Skin of *Centia rubrizona*, Utah.
Skidmore, M.—Heads of ducks.
Slagle, M.—Box of eggs of birds.
Smith, Donald A.—Five skins of Labrador birds.
Smith, W.—Living horned owl, (*Bubo virginianus.*)
Stearns, R. E. C.—Shells from Nicaragua, California, and South America.
Steindachner. Dr. F.—(See *Vienna.*)
Stickney, Mr.—Starch made in Florida from seeds of coontee, (*Zamia integrifolia.*)
Strebel, Dr. M.—Box of shells, Vera Cruz.
Sumichrast, Professor F.—Birds, mammals, and alcoholic specimens of Mexico.
Swan, J. G.—Indian curiosities and zoological specimens from Puget sound.
Tiffany, Rev. C. C.—Bottle reptiles, Arabian desert.
Totten, Dr. G. F.—Skeleton of *Procyon cancrivora*, Aspinwall.
Totten, Dr. G. F., through Captain J. M. Dow.—Skeleton of *Elasmognathus Bairdii* from Panama railroad.

Vienna Zoological Museum., through Dr. Regenhofer.—Series of Austrian Lepidoptera.

Vienna Zoological Museum, through Dr. F. Steindachner.—Series of European fishes in alcohol.

Vuille, W.—Skins and eggs of birds of California.

Wallen, U. S. A., Colonel H. D.—Skin of *Heloderma horridum*, Arizona; received through Major General Irvin McDowell.

Wallis, J. Willie—Skin of albino swallow, from Bladensburg.

Warren, General G. K.—Mounted head of Elk.

Watt, D. A.—Fishes from St. Lawrence river.

Weeds, U. S. A., Dr. James F.—Living horned frog (*Phrynosoma orbiculare,*) New Mexico.

Wernigk, Dr.—Zoological collections, Colorado and Montana.

Woltz, T. N—Eggs of pigeons.

Woodward, R. B.—Mounted birds of California.

Wright, Charles.—Shells and insects of Cuba

Young, John F.—*Remora*, or sucking fish, from Potomac river.

Younglove, A. E.—Box of birds and reptiles, Hayti.

Zeledon, A.—Skins of birds of Costa Rica.

LIST

OF

METEOROLOGICAL STATIONS AND OBSERVERS OF THE SMITHSONIAN INSTITUTION FOR THE YEAR 1866.

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	80	A	11
Connolly, Henry	Winowkupa, Labrador				B. T	6
Magnetic Observatory	Toronto, Canada West	43 39	79 21	†108	A	6
Murdock, G	St. John, New Brunswick	45 16	66 03	135	A	12
O'Donoghue, John	St. Anne, Canada East	47 24	70 05	175	B. P. T	2
Rankin, Colin	Michipicoton, Canada West	47 56	85 06	660	B. T	5

MEXICO.

torius, Dr. Charles	Mirador, Vera Cruz	19 15	96 25	3,600	A	[illegible]

CENTRAL AMERICA.

xius, Dr. A	San José, Costa Rica	9 54	84 06	3,772	A	[illegible]
e, C. N	San José, Costa Rica	9 54	84 06	3,772	T. R	[illegible]
, J. P., M. D	Aspinwall	9 23	79 53	6	A	[illegible]

BERMUDA.

Royal Engineers, (in the Royal Gazette.)	Centre Signal Station, St. George's				A	12

* 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.

List of meteorological stations and observers, &c.—Continued.

ALABAMA.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° ′	° ′	*Feet.*		
Cornette, A., S. J.	Spring Hill	Mobile	30 41	88 09	157	T. R	1
Harris, Andrew J.	Moulton	Lawrence	34 32	87 25	643	A	5
Peters, Thomas M.	Moulton	Lawrence	34 36	87 25	643	B. T. R.	4
Vankirk, W. J.	Bon Secour.	Baldwin				T	3
Tutwiler, H	Havana	Greene	32 50	87 46	500	T. R	7

ARKANSAS.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Russell, O. F	Helena	Phillipps	34 33	90 10		T. R	12
Springer, Rev. Francis	Fort Smith					T	1

CALIFORNIA.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Ayres, W. O., M. D.	San Francisco	San Francisco	37 48	122 27	30	A	12
Canfield, Colb't A., M. D.	Monterey	Monterey	36 36	121 52	40	A	12
Logan, Thomas M., M. D.	Sacramento	Sacramento	38 32	121 30	65	A	12
Smith, Mrs. M. D	Meadow Valley	Plumas	40 20	120 15	3,700	B. T. R.	9

COLORADO.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Merriam, Arthur H.	Fountaine	El Paso				N	2

CONNECTICUT.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Dewhurst, Rev. E.	Groton	New London	41 21	72 12	20	B. T. R.	12
Hunt, Rev. Daniel	Pomfret	Windham	41 52	72 10	587	A	12
Johnston, Prof. John	Middletown	Middlesex	41 33	72 39	175	A	12
Rockwell, Charlotte	Colebrook	Litchfield	42 00	73 03		T	12
Yeomans, William H.	Columbia	Tolland	41 40	72 42		T	12

DELAWARE.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Vankekle, L.	Delaware City	New Castle	39 35	75 34		T	4

FLORIDA.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Baldwin, A. S., M. D.	Jacksonville	Duval	30 15	82 00	20	A	4
Garvin, P. C., M. D.	Gordon	Alachua	29 45	82 30		T	4
Ives, Edward R.	Lake City	Columbia	30 12	82 40	185	T. R	3
Scott, H. B	Gordon	Alachua	29 45	82 30		T	2

GEORGIA.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Deckner, Fredrick	Atlanta	Fulton	33 45	84 31	1,050	T. R	11

List of meteorological stations and observers, &c.—Continued.

ILLINOIS.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° ′	° ′	Feet.		
Adams, W. H	Elmore	Peoria	40 56	90 04	612	R	11
Aldrich, Verry	Tiskilwa	Bureau	41 15	89 16	550	T	12
Babcock, E	Riley	McHenry	42 11	88 33	760	T. R	11
Bowman, E. H., M. D	Andalusia	Rock Island	41 30			B. T	11
Ballou, N. E., M. D	Sandwich	DeKalb	41 31	88 30	665	T. R	12
Brendel, Frederick, M. D	Peoria	Peoria	40 43	89 30	460	A	12
Blanchard. O. A	Elmira	Stark	41 12	90 15		T. R	12
Brinkerhoff, George M	Springfield	Sangamon	39 48	89 33		T	12
Brookes, Samuel	Chicago	Cook	42 00	87 30	600	T	12
Carey, Daniel	Alto	Lee	41 45	89 00		T	5
Dudley, Timothy	Waverly	Morgan	39 40	90 00	680	T. R	12
Duncan, Rev. Alexander	Mount Sterling	Brown	40 00	91 15		T	12
Eldredge, William V	Golconda	Pope	37 41	88 47		T	11
Grant, John; Grant, Charles W	Manchester	Scott	39 31	90 34	683	A	11
Huse. Fred. J	Evanston	Cook	42 03	87 38	614	B. T	1
Livingston, Prof. William	Galesburg	Knox	40 55	87 11	795	A	12
Marcy, Prof. Oliver	Evanston	Cook	42 03	87 38	614	B. T. R	1
Marsh, O. J	Hoylton	Washington	38 30	89 00		T	3
Mead, S. B	Augusta	Hancock	40 10	91 00		T. P. R	12
Merwin, Mrs. Emily H	Ottawa	La Salle	41 20	88 47	500	T. R	11
Moore, C. H	Clinton	De Witt	40 09	88 58	430	B. T	4
Parker, John D	De Kalb	De Kalb				B	2
Phelps, E. S; Phelps, Miss Lelia E	Wyanet	Bureau	41 30	89 45		T. R	12
Pool, Isaac A	Chicago	Cook				T	1
Rogers, O. P. and J. S	Marengo	McHenry	42 14	88 38	842	B. T. R	7
Smith, Henry K	Magnolia	Putnam	41 15	89 15		T. R	2
Spencer, Wm. C	Dubois	Washington	38 14	89 16		T. R	12
Spaulding, Abiram	Aurora	Kane	41 48	88 23		A	12
Spaulding, S. C	South Pass	Union				T	2
Tolman, James W	Winnebago	Winnebago	42 17	89 12	[illegible]	A	12

INDIANA.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Boerner, Charles G	Vevay	Switzerland	38 46	84 59		T. R	12
Butterfield, W. W. & Mrs	Indianapolis	Marion	39 45	86 20	698	T	8
Chappellsmith, John	New Harmony	Posey	38 08	87 50	350	A	12
Collins, Rev. Samuel	Madison	Jefferson	38 45	85 40	400	B. T. R	5
Dawson, William	Spiceland	Henry	39 48	85 18	1, 025	B. T. R	12
Griest, Miriam	Balbec	Jay	41 00	85 00	1, 000	B. T	4
Holmes, Thomas	Merom	Sullivan	39 10	87 40		T. R	4
Kemper, G. W. H., M. D	Muncie	Delaware	40 12	85 16		T. R	5
McCoy, Dr. F; McCoy, Miss Lizzie	Columbia	Whitney	41 10	85 30		T. R	12
Sutton, George, M. D	Aurora	Dearborn	39 04	84 54	*80	B. T. R	12
Valentine, John	Richmond	Wayne	39 52	84 39	850	A	12

IOWA.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Bryant, A. F	Fontanelle	Adair	41 30	95 35	1, 500	T. R	8
Bush, Rev. Alva	Osage	Mitchell	43 20	83 00		T	9
Collin, Prof. Alonzo	Mount Vernon	Linn	42 00	91 00		T	12
Deering, D. S	Independence	Buchanan	42 30	92 16	850	T	12
Dickinson, James P	Guttenburg	Clayton	43 00	90 50	690	T	7
Dorweiler, Philip	Guttenburg	Clayton	43 00	90 50	690	T	3
Dorweiler, Philip	Algona	Kossuth				T. R	4
Farnsworth, P. J., M. D	Clinton	Clinton	40 40	90 10	630	T. R	12
Hagensick, John Mathias	Ceres	Clayton	42 45	91 11	825	T	12
Horr, Asa, M. D	Dubuque	Dubuque	42 30	90 40	666	A	12
Hudson, A. T	Lyons	Clinton	40 42	90 10	630	T. R	7
Kridelbaugh, S. H., M. D	Clarinda	Page				T. R	1
Love, Louisa P	Burlington	Des Moines	40 53	91 10	530	T	2
McCready, Daniel	Fort Madison	Lee	40 37	91 28		T. R	12

* Above low water in Ohio river.

List of meteorological stations and observers, &c.—Continued.

IOWA—Continued.

Name of observer.	Station.	County.	North latitude. ° ′	West longitude. ° ′	Height. Feet.	Instruments.	No. of months received.
Massman, Dr. J	Lyons	Clinton	40 42	90 10		T. R	3
Mead, Allen	Manchester	Delaware	42 30	91 30	925	T. R	12
Mead, Chauncey	Monticello	Jones	42 15	91 15	880	T. R	6
Moulton, M. M	Monticello	Jones	42 15	91 15	800	T. R	6
Nash, Rev. J. A	Des Moines	Polk	41 35	93 36		T. R	12
Parvin, Prof. Theodore S.	Iowa City	Johnson	41 37		621	A	12
Pratt, George B. / Smith, Sydney	Davenport	Scott	41 30	90 40	737	A	12
Steed, T	Waterloo	Black Hawk	42 30	92 30	670	T	12
Stern, Jacob T	Harris Grove	Harrison	41 00	95 00	909	T	6
Taylor, Prof. K. M., M. D.	Keokuk	Lee	40 27	91 36		A	2
Townsend, Nathan	Iowa Falls	Hardin	42 32	93 20		T. R	11
Walton, Josiah P	Muscatine	Muscatine	41 25	92 02	582	A	6
Wheaton, Alex. Camp	Independence	Buchanan	42 29	91 50		T. R	2
Wheaton, Mrs. Daniel D	Independence	Buchanan	42 29	91 50		T. R	10
KANSAS.							
Agricultural College	Manhattan	Riley	39 12	96 45	1,000	T. R	8
Bacon, David G	Lecompton	Douglass				R	4
Beckwith, W	Olatha	Johnson	38 50	94 30		T. R	12
Crocker, Allen	Avon	Coffey	38 08	95 35	825	T. R	3
Horn, Dr. H. B., and Miss Clotilde.	Atchison	Atchison	39 42	95 00		T	7
Shaffer, Joseph M. / Camp, Essex P	Fort Riley	Davis	39 00	96 30	1,300	T	2
Stayman, Dr. J	Leavenworth	Leavenworth	39 15	94 52		T. R	12
Woodworth, Abner, M. D.	Council Grove		38 42	96 32		T. R	12
KENTUCKY.							
Beatty, O	Danville	Boyle	37 40	84 30	900	B. T. R	12
Doak, W. S	London	Laurel	37 12	84 03		T	3
Martin, Dr. Samuel D	Chilesburg	Fayette	38 04	84 20	900	B. T. R	11
Young, Mrs. Lawrence	Louisville	Jefferson	38 07	85 24	570	A	12
Mathis, H. C	Taylorsville	Spencer	38 00	85 00	600?	T. R	4
MAINE.							
Gardiner, Robert H	Gardiner	Kennebec	44 11	69 46	76	A	12
Guptill, G. W	Cornish	York	43 40	70 44	800	T. R	12
Moore, Asa P	Lisbon	Androscoggin	44 00	70 04	130	T. R	12
Moulton, John P	Standish	Cumberland	43 45	70 30	280	T. R	8
Parker, J. D	Steuben	Washington	44 31	67 57	50	A	11
Pettingill, Waldo	Rumford Point	Oxford	44 30	70 40	600	T. R	3
Pitman, Edwin	Lee	Penobscot				T	3
Pitman, Edwin	Williamsburg	Piscataquis	45 21			T. R	6
Robinson, Almon	Webster	Andrescoggin	44 04	70 04		T	1
Towle, Benjamin H	Lee	Penobscot				T. R	7
West, Silas	Cornish	York	43 40	70 44	784	B. T. R	12
Wilbur, Benjamin F	West Waterville	Kennebec	44 30	69 45	250	T. R	12
MARYLAND.							
Baer, Miss Harriott M	Frederick	Frederick	39 24	77 17		T. R	9
Goodman, William R	Annapolis	Anne Arundel	38 58	76 29	20	A	12
Grape, George S	Catonsville	Baltimore	39 17	76 42	42	T	12
McCormick, James O	Woodlawn	Cecil	39 39	76 04		B. T. R	12
Smith, Eli	Emmittsburg	Frederick				T	6
Stephenson, Rev. James	St. Inigoes	St. Mary's	38 10	76 30	45	A	4

List of meteorological stations and observers, &c.—Continued.

MASSACHUSETTS.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° ′	° ′	Feet.		
Astronomical Observatory.	Williamstown	Berkshire	42 43	73 13	686	A	12
Bacon, William	Richmond	Berkshire	42 13	72 20	1,000	T. R	11
Caldwell, John H	Newbury	Essex	42 45	70 55	25	T	12
Cunningham, George A	Lunenburg	Worcester	42 35	71 43		B. T	5
Davis, Rev. Dr. Emerson	Westfield	Hampden	42 06	72 48	180	A	5
Draper, Joseph	Worcester	Worcester	42 16	71 48	528	A	12
Fallon, John	Lawrence	Essex	42 42	71 11	133	A	12
Fendler, Augustus	Cambridge	Middlesex	42 28	71 11	60	B. T	2
Merriam, Arthur M	Topsfield	Essex	42 38	71 57		A	3
Merriam, Sidney A	Topsfield	Essex	42 38	71 57		A	9
Metcalf, John Geo	Mendon	Worcester	42 06	71 34		B. T. R	12
Nason, Rev. Elias	North Billerica	Middlesex	42 34	71 16		B. T	11
Nelson, Henry M	Georgetown	Essex	42 42	71 00	225	T	8
Newcomb, Guilford S	Kingston	Plymouth	42 00	70 45		T. R	6
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
Tucker, Edward T	New Bedford	Bristol	41 39	70 55	50	T. R	4

MICHIGAN.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Bullard, R	Litchfield	Hillsdale	42 01	84 46	1,040	T. R	7
Chase, Milton, M. D	Kalamazoo	Kalamazoo				T	4
Ellis, Edwin, M. D	Ontonagon	Ontonagon	46 40	90 00	610	T	7
Holmes, E. S	Grand Rapids	Kent	43 00	85 40	752	T	9
Kedzie, Prof. R. C	Lansing	Ingham	42 42	84 34	895	A	12
Mapes, Henry H	Oshtemo	Kalamazoo				N	12
Minick, J. B	Houghton	Houghton	46 40	88 30	600	T. R	1
Paxton, J. W	Alpena	Alpena	45 02	83 05		B. T	6
Smith, Rev. Geo. N	Northport	Leelenau	45 08	85 41	592	T	7
Smith, Harmon M	Kalamazoo	Kalamazoo	42 20	85 40		N	11
Steele, George E	Homestead	Benzie	44 30	86 00		T	11
Strong, L. H	Holland	Ottawa	42 42	86 00		T. R	10
Whelpley, Miss Florence E.	Monroe	Monroe	41 58	83 23	590	T. R	12

MINNESOTA.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Babcock, Dr. B. F	Afton	Washington	44 50	93 00	950	T	9
Cheney, William	Minneapolis	Hennepin	45 00	93 10	856	A	12
Helmstreet, John W	St. Paul	Ramsey	44 57	93 05	800	A	3
Paterson, Rev. A. B., D.D	St. Paul	Ramsey	44 57	93 05	800	T. R	12
Roos, Charles	New Ulm	Brown	44 16	94 26	*821	T. R	12
Smith, Henry L	Forest City	Meeker	45 13	94 28		T. R	5
Stouffer, Andrew	Bowles's Creek	Washington	44 56	92 52	800	T	1
Wieland, C	Beaver Bay	Lake	47 12	91 18	650	T. R	12
Woodbury, C. W	Sibley	Sibley	44 31	94 26	1,600	T. R	11

MISSISSIPPI.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Cleland, Rev. T. H	Fayette	Jefferson				T	2
McCary, Robert	Natchez	Adams	31 34	91 25		B. T. R	7
McCary, William	Natchez	Adams	31 34	91 25	2	B. T. R	5
Moore, Albert	Grenada	Yallobusha	33 45	90 00		T	11
Smith, J. Edwards	Kingston	Adams	31 24	91 16		B. T	4

MISSOURI.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Caldwell, Joseph T	Athens	Clark				T. R	6
Christian, John	Harrisonville	Cass	38 40	94 30		T. R	12
Englemann, George, M. D.	St. Louis	St. Louis	38 37	90 15	481	A	11
Fendler, Augustus	Allenton	St. Louis	38 29	90 45	482	B. T. P	10
Moore, Dr. W	Union	Franklin	38 25	91 09	616	T. R	11
Ray, George P	Canton	Lewis	40 12	91 37		T	7
Sibley, P. B	Easton	Buchanan	39 46	94 22		T. R	7
Stuntebeck, F. H., S. J	St. Louis	St. Louis	38 37	90 15	470	A	12
Vertrees, John E	Edinburg	Grundy	40 00	93 30		T. R	4

* Above Minnesota river.

List of meteorological stations and observers, &c.—Continued.

MONTANA.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° ′	° ′	*Feet.*		
Wheaton, Alex. Camp	Helena City	Edgerton	46 45	111 50	4,150	T	12

NEBRASKA.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Bowen, John S	Elkhorn City	Washington	41 22	96 12	1,350	T	12
Child, A. L., M. D	Glendale	Cass	40 55	96 05	1,010	T	11
Hamilton, Rev. William	Bellevue	Sarpy	41 08	95 50		T. R	12

NEW HAMPSHIRE.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Brown, Branch	Stratford	Coos	44 04	71 07	1,000	T. R	12
Chase, Arthur	Claremont	Sullivan	43 22	72 21	539	B. T. R	12
Hurlin, Rev. Wm	Antrim	Hillsboro'				N	1
Mead, Stephen O	Claremont	Sullivan				T	11
Odell, Fletcher	Shelburne	Coos	44 23	71 06	700	B. T	12
Pitman, Charles H	North Barnstead	Belknap	43 38	71 27		T. R	10
Wheeler, John T	Concord	Merrimack	43 12	71 29	400	B. T. R	9

NEW JERSEY.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Beans, Thomas J	Moorestown	Burlington	39 59	74 54		T. R	12
Brooks, William	Paterson	Passaic	40 55	74 10	60	T. R	12
Cole, Barker	Seaville	Cape May	39 20	74 40	18	T	5
Cook, Ephraim R	Trenton	Mercer				B. T. R.	12
Cook, Prof. George H	New Brunswick	Middlesex	40 30	74 27	80	A	12
Deacon, John C	Burlington	Burlington	40 05	75 10	60	T. R	12
Fleming, John	Readington	Hunterdon	40 33	74 40		T	2
Lippincott, James S	Cole's Landing	Camden	39 54	75 02	50	A	5
Rhees, Morgan J., M. D	Mount Holly	Burlington	40 00	74 47	30	B. T	12
Sheppard, Clarkson; Sheppard, Miss R. C.	Greenwich	Cumberland	39 20	75 25	30	A	12
Shriver, Howard	Dover	Morris				B. P. T.	4
Whitehead, W. A	Newark	Essex	40 45	74 10	35	B. T. R	12
Wood, Samuel	Haddonfield	Camden				A	7

NEW YORK.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Arden, Thomas B	Garrison's	Putnam	41 22	74 02	180	T. R	12
Aubier, Rev. Jno. M., S. J	New York	New York	40 44	73 59	104	B. T	4
Barrows, Storrs	South Trenton	Oneida	43 10	74 56	835	T. R	12
Bartlett, Erastus B	Vermillion	Oswego	43 26	77 26	327	T. R	12
Beauchamp, William M	Skaneateles	Onondaga	43 00	76 30	932	B. T	12
Bowman, John	Baldwinsville	Onondaga	43 04	76 41		T	12
Denning, William H	Fishkill on Huds'n	Dutchess	41 34	74 18	42	B. T. R.	10
Dewey, Prof. Chester	Rochester	Monroe	43 07	77 51	516	B. T. R	12
Edwards, Daniel	Little Genesee	Allegany	42 00	78 36	1,500	B. T. R	11
Fries, George W	Friendship	Allegany	42 15	78 10	1,536	T	2
Gardiner, James H	Newburg	Orange	41 31	74 01	85	B. T. R	12
Gregory, S. O	Theresa	Jefferson	44 12	75 48	365	T. R	11
Haas, Henry	Depauville	Jefferson	44 15		350	T. R	12
Heimstreet, John W	Troy	Rensselaer	42 44	73 40	58	A.	5
Howell, Robert	Nichols	T oga	42 00	76 32		T	12
Ingalsbe, Grenville M	South Hartford	Washington	43 15	73 21	400	T. R	12
Ives, William	Buffalo	Erie				B. T. R.	12
Joy, Prof. Charles A.	New York	New York	40 43	74 05		A.	12
Mack, Rev. Eli T	Flatbush	Kings	40 37	74 02	54	B. T. R	10
McMore, P. A	Fort Ann	Washington	42 03	73 44	1,430	T. R	2
Malcom, Wm. Schuyler	Oswego	Oswego	43 28	76 30	250	B. T. R.	12
Mathews, M. M., M. D	Rochester	Monroe	43 08	77 51	525	A	12
Morris, Miss Elizabeth	Throg's Neck	Westchester	40 49	73 49	43	T	12

List of meteorological stations and observers, &c.—Continued.

NEW YORK—Continued.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° ′	° ′	*Feet.*		
Morris, Prof. Oran W	New York	New York	40 43	74 05	75	A	11
Paine, Horace M., M. D.	Albany	Albany	42 39	73 44	75	P. T. R	4
Roe, Sanford W	Jamestown	Chautauqua	42 06	79 29	1,454	T	3
	Germantown	Columbia				T	9
Rogers, F. M.	Throg's Neck	Westchester				T. R.	2
Russell, Cyrus H	Gouverneur	St. Lawrence	44 19	75 29		B. T. R.	11
Smith, E. A., & daughters.	Moriches	Suffolk	40 49	72 36	13	T. R.	12
Spooner, Stillman, M. D.	Oneida	Madison	43 04	75 50	500	T. R.	12
Trowbridge, David	Hector	Schuyler	42 30	77 00	850	N	12
Willis, Oliver R	White Plains	Westchester	41 05	73 40		T	9
Wilson, Rev. W. D., D. D	Geneva	Ontario	42 53	77 02	567	B. T. R.	12
Wooster, Charles A	North Hammond	St. Lawrence	44 30	75 40		B. T. R.	7
Yale, Walter D	Houseville	Lewis	43 40	75 32		T. R.	6

NORTH CAROLINA.

Adams, E. W.	Wilson	Wilson	35 41	77 47	105	T. R.	11
Allison, Thos. A	Statesville	Iredell				T. R.	7
Brewer, Rev. Fisk P.	Raleigh	Wake				T. R.	5
Mills, John H.	Oxford	Granville	36 23	78 14		T	6

OHIO.

Abell, B. F.	Welshfield	Geauga	41 23	81 12	1,205	T. R.	3
Bambach, Dr. G.	Ripley	Brown	38 47	83 31	*106	A	7
Benner, Josiah F.	New Lisbon	Columbiana	40 45	80 45	961	B. T. R.	12
Clarke, John	Bowling Green	Wood				T. R.	11
Crane, George W	Bethel	Clermont	39 00	84 00	555	T. R.	12
Doyle, J. B.	Steubenville	Jefferson	40 45	80 47		B. T.	12
Fraser, James B	Saybrook	Ashtabula	41 48	80 53		T	4
Hammitt, John W.	College Hill	Hamilton	39 19	84 26	800	T. R.	10
Harper, George W.	Cincinnati	Hamilton	39 06	84 27	*305	A	12
Haywood, Prof. John	Kingston	Ross	39 29	83 00	692	A	12
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 38	683	B. T. R.	12
McMillan, Smith B.	East Fairfield	Columbiana	40 41	80 44	1,152	A	12
Mathews, Joseph McD.	Hillsborough	Highland	39 13			A	12
Myers, John H.	Smithville	Wayne	40 52	81 51	934	T	2
Newton, Rev. Alfred	Norwalk	Huron	41 13	82 43		T. R.	10
Phillips, R. C.	Cincinnati	Hamilton	39 06	84 27	588	B. T. R.	12
Rodgers, A. P.	Gallipolis	Gallia	39 00	82 00	600	T. R.	11
Schauber, Hubert A.	Centralia	Marion				N	1
Smith, C H., M. D.	Kenton	Hardin	40 10	83 54	1,562	T	8
Smurr, T. A., M. D.	Cleveland	Cuyahoga	41 37	81 46		T	3
Thompson, Rev. David	Milnersville	Guernsey	40 10	81 45		T. R.	12
Thompson, Prof. H. A.	Westerville	Franklin	40 04	83 00		A	12
Towson, J. W	Lancaster	Fairfield				T	3
Trembley, J. B., M. D.	Toledo	Lucas	41 39	82 22	604	B. T. R.	12
True, H. A., M. D.	Marion	Marion	40 35	83 08	1,077	T. R.	11
Tuckerman, L. B.	College Hill	Hamilton	39 19	84 26	800	T. R	12
Tweedy, D. H	Smithfield	Jefferson	40 20	80 38	1,000	R	2
Williams, Prof. M. G.	Urbana	Champaign	40 06	83 43	1,015	B. T. R.	12
Winchester, Electus D.	Austinburg	Ashtabula	41 54	80 52	816	B. T.	2
Winger, Martin	Wooster	Wayne	40 49	81 57	872	T	8

OREGON.

Barnard, A. D.	Corvallis	Benton	44 30	118 06		T	8
Hindman, S. M. W.	Albany	Linn	44 22	123 00	600	R.	11

* Above low water in the Ohio river.

List of meteorological stations and observers, &c.—Continued.

PENNSYLVANIA.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° ′	° ′	*Feet.*		
Bentley, E. T	Tioga	Tioga	42 00	77 00	1,000	T. R	12
Bruckart, H. G	Silver Spring	Lancaster	40 05	76 45		T.	8
Brugger, Samuel	Fleming	Centre	40 55	77 53	780	T. R	12
Darlington, Fenelon	Pocopson	Chester	39 40	75 37	218	T. R	12
Day, Theodore	Dyberry	Wayne	41 36	75 19		T	11
Dutton, J Russell	Stevensville	Bradford	41 45	76 35	300	T. R	8
Fenton, Elisha	Grampian Hills	Clearfield	41 00	78 40	1,400	B. T. R.	12
Grathwohl, John	Blooming Grove	Pike	41 30	75 00		T. R	11
Hance. Ebenezer	Fallsington	Bucks	40 12	74 48	30	B. T. R.	12
Heisely, Dr, John	Harrisburg	Dauphin	40 16	76 15		A	12
Hoffer, Dr. Jacob R	Mount Joy	Lancaster	40 08	76 32		B. T. R.	12
James, Prof. C. S	Lewisburg	Union	40 58	76 58		A	12
Kirkpatrick, Prof. Jas. A	Philadelphia	Philadelphia	39 57	75 11	60	A	12
Kohler, Edward	North Whitehall	Lehigh	40 44	75 28	450	T	12
McConnell, E. M	New Castle	Lawrence	41 00	80 12		T	12
Martindale, Isaac C	Byberry	Philadelphia	40 05	75 00	70	N	12
Meehan, Thomas	Germantown	Philadelphia				T	12
Raser, John Heyl	Reading	Berks	40 20	75 57	269	T	6
Ricksecker, Lucius E	Nazareth	Northampton	40 43	75 21	525	T	9
Smith, Wm., D. D	Canonsburg	Washington	40 16	80 10	850	B. T. R.	11
Spencer, Miss Anna	Horsham	Montgomery	40 00	75 11	250	B. T. R.	12
Spera, W. H	Ephrata	Lancaster				T. R	12
Taylor, John	Connellsville	Fayette	40 00	79 36		T	12

RHODE ISLAND.

[illegible] Prof. Alexis	Providence	Providence	41 49	71 25	120	A	5
[illegible], William H	Newport	Newport	41 28	71 21	25	T. R	12

SOUTH CAROLINA.

[illegible]y, Charles	Wilkensville	Union	34 50	81 36		N	2

TENNESSEE.

Doak, S. S. & W. S	Greenville	Green	36 05	82 50		T	3
Stewart, Prof. Wm. M	Clarksville	Montgomery	36 29	87 13	481	A	12
Williams, Edward F	Lookout Mount'n	Hamilton	35 15	85 15	1,626	B. T	9

TEXAS.

Brown, James	Kaufman	Kaufman				T	3
Gantt, W. H., M. D	Chappell Hill	Washington	30 15	96 21	542	T. R	5
Rayel, James T	Kaufman	Kaufman	32 30	96 00		N	10

UTAH.

Bullock, Thomas	Wanship	Summit	40 42	111 20	6,200	T	7
Burgon, George A; Pearce, Harrison	St. George	Washington	37 11	114 00		T. R	2
Phelps, W. W	Salt Lake	Salt Lake	40 45	111 26	4,320	T. R	11
Siler, Andrew L	Rockville	Kane	37 20	113 40		N	4

List of meteorological stations and observers, &c.—Continued.

VERMONT.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° ′	° ′	*Feet.*		
Buckland, Harmon	Brandon	Rutland	43 45	73 00	460	T. R.	9
Cutting, Hiram A.	Lunenburg	Essex	44 28	71 41	1,124	A	10
Eaton, Benjamin F., M.D.	Barnet	Caledonia	44 18	72 05	952	B. T. R.	7
Paddock, James A.	Craftsbury	Orleans	44 40	72 30	1,100	T. R.	12
Paine, Charles L.	Randolph	Orange	43 35	72 36	700	T. R.	12
Perry, Rev. John B.	Wilmington	Windham	42 53	72 47	1,250	B. T.	8
Sheldon, Harmon A.	Middlebury	Addison	43 59	73 10	398	A	12

VIRGINIA.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Meriwether, Charles J.	near Lynchburg	Bedford	37 15	79 10	....	T	[illegible]
Shriver, Howard	Wytheville	Wythe	36 55	81 04	2,400	B. T.	[illegible]

WASHINGTON.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Swan, James G.	Neeah Bay	....	48 22	124 37	17	T. R.	9

WEST VIRGINIA.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
McDowell, W. H.	Romney	Hampshire	....	....	....	T	8
Roffe, Charles L.	Ashland	Cabell	38 30	82 16	600	T. R.	9

WISCONSIN.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
[illegible]eed, E. Everett	Embarass	Waupaca	44 51	88 37	....	T. R.	[illegible]
Curtis, W. W.	Rocky Run	Columbia	43 26	89 19	....	T. R.	[illegible]
Eddy, Levens	Delavan	Walworth	42 39	88 37	957	B. T. R	12
Ellis, Edwin, M. D.	Odanah	Ashland	46 33	91 00	610	T	3
Hicks, John C.	Weyauwega	Waupaca	44 15	88 50	850	T. R.	8
Lapham, Incr'se A., LL.D.	Milwaukee	Milwaukee	43 03	87 56	604	A	12
Lups, Jacob	Manitowoc	Manitowoc	44 07	87 45	658	B. T. R	12
Mathews, Dr. James	Weyauwega	Waupaca	....	....	....	T	1
Mead, H. C.	Waupaca	Waupaca	44 20	89 11	1,000	T	12
Moeller, G.	Plymouth	Sheboygan	43 44	88 07	870	B. T.	12
Porter, Henry D.	Beloit	Rock	42 30	89 04	780	A	12
Waite, M. C.	Baraboo	Sauk	43 27	89 45	920	T R.	12
Ward, Prof. Wm. H.	Ripon	Fond du Lac	43 54	88 59	....	B. T.	8
Whiting, William H.	Geneva	Walworth	42 30	89 41	600	T	8
Winkler, Carl, M. D.	Milwaukee	Milwaukee	43 03	87 57	630	B. T. R.	12

Deaths of observers.

Stephen O. Mead, Claremont, New Hampshire, March 18, 1867.
Rev. Dr. Emerson Davis, Westfield, Massachusetts, June 8, 1866.
William H. Denning, Fishkill, New York, October 31, 1866.
Robert McCary, Natchez, Mississippi, July 25, 1866.
Allen Mead, Manchester, Iowa, December 19, 1866.
Dr. James Mathews, Weyauwega, Wisconsin, September 20, 1866.

Colleges and other institutions from which meteorological registers were received during the year 1866, included in the preceding list.

Nova Scotia	Acadia College	Wolfville.
Canada	Magnetic Observatory	Toronto.
Alabama	Baptist Female Institute	Moulton.
	Greene Springs School	Havana.
	Spring Hill College	Spring Hill.
Arkansas	Normal School	Helena.
Connecticut	Wesleyan University	Middletown.
Illinois	Lombard University	Galesburg.
	Northwestern University	Evanston.
Iowa	Cornell College	Mount Vernon.
	Griswold College	Davenport.
	Iowa State University	Iowa City.
Kansas	Agricultural College	Manhattan.
Maryland	St. Timothy's Hall	Catonsville.
Massachusetts	Amherst College	Amherst.
	State Lunatic Hospital	Worcester.
	Williams' College	Williamstown.
Michigan	State Agricultural College	Lansing.
Missouri	St. Louis University	St. Louis.
	Grand River College	Edinburg.
New Hampshire	St. Paul's School	Concord.
New Jersey	Rutger's College	New Brunswick.
New York	Columbia College	New York.
	Institution for Deaf and Dumb	New York.
	Erasmus Hall Academy	Flatbush.
	St. Francis Xavier's College	New York.
	University of Rochester	Rochester.
Ohio	Farmers' College	College Hill.
	Otterbein University	Westerville.
	Urbana University	Urbana.
	Woodward High School	Cincinnati.
Pennsylvania	Central High School	Philadelphia.
	Jefferson College	Cannonsburg.
	Lewisburg University	Lewisburg.
Tennessee	Stewart College	Clarksville.
	Lookout Mountain Educational Institution	Lookout Mountain.
	Tusculum College	Greenville.
Texas	Institution for Deaf and Dumb	Austin.
Wisconsin	Beloit College	Beloit.
	Ripon College	Ripon.

METEOROLOGICAL MATERIAL CONTRIBUTED IN ADDITION TO THE REGULAR OBSERVATIONS.

Agnew, J. C.—Table showing the amount of rain, also the depth of snow, in each month, from May, 1859, to December, 1866, inclusive, at Edina, Knox county, Missouri.

Anderson, W. H—Monthly mean temperature of a station one mile north of Rockville, Parke county, Indiana, during the years 1862, 1863, 1864, 1865, 1866. Two pages foolscap.

Asiatic Society of Bengal.—Abstract of the results of the hourly meteorological observations taken at the surveyor general's office, Calcutta, in the months of August, September, October, November, and December, 1865.—Meteorological observations taken at Gangaroowa, near Kandy, Ceylon, in the month of May, 1864. [Published in the journal of the Society, part 2, Nos. 1 and 2, 1866.]

Bartlett, Erastus B.—Summary of observations for the year 1866, and comparison with the preceding twelve years. Newspaper slip.

Beauchamp, W. M—Summary of observations for the year 1866, at Skaneateles, New York.

British Museum.—Catalogue of the collection of meteorites exhibited in the mineral department of the British Museum. January 7, 1866. 8 pages, 8vo.

Buckley, S. B.—A preliminary report of the Texas geological survey, by S. B. Buckley. Austin, 1866. 87 pages, 8vo. [Contains remarks on climate of Texas, and tables of rain and temperature.]

Bullock, Thomas.—Observations on temperature and face of the sky, at St. Mary's, Chalk creek, Summit county, Utah Territory, from June to December, 1865. 8 pages foolscap.

Cook, Eugene B.—Report of the meteorologist of the New York Skating Club for the season 1865–'66. 24 pages, 12mo.

Dawson, William.—Summary of observations for the year 1866, at Spiceland, Indiana.

Dijon, Comité Central d'Agriculture.—Journal d'Agriculture de la Côté-d'Or, publié par le Comité Central d'Agriculture de Dijon, sous les auspices de M. le préfet et du conseil général. [Contains monthly tables of meteorological observations made at Dijon, France, by Alexis Perrey.]

Dowell, Greensville, M. D.—The Galveston Medical Journal, a monthly record of medical science. Greensville Dowell, M. D., editor. Galveston, Texas. [Contains tables of meteorological observations at Houston, Texas.]

Fallon, John.—Summary of observations for the year 1866, at Lawrence, Massachusetts. Newspaper.

Geological survey of India.—Catalogue of meteoric stones and meteoric irons in the museum of the geological survey, Calcutta. 4 pages.

Heis, Professor Dr.—Wochenschrift für Astronomie, Meteorologie und Geographie. Redigirt von Professor Dr. Heis, in Münster. 8vo.

Huntington, George C.—Summaries of observations at Kelley's island: Table I. Mean and extreme temperature, amount of rain, force of wind, first appearance of anchor-ice in lake, &c., during the month of December, from 1859 to 1866, inclusive. Table II. Monthly and annual mean temperature from 1860 to 1866, inclusive. Table III. Monthly and annual quantity of rain and melted snow for the years 1860 to 1866, inclusive. Printed slip.

Hyde, Gustavus A.—Summary of observations for the year 1866, and means of eleven years, at Cleveland, Ohio. Printed slip.

Ives, William.—Summary of observations made at Buffalo, New York, during the year 1866. Newspaper.

Jelinek, Dr. K.—Ueber den jährlichen Gang der Temperatur und des Luftdruckes in Oesterreich und an einigen benachbarten Stationen. Von Dr. K. Jelinek, correspondirendem Mitgliede der kaiserlichen Academie der Wissenschaften. Mit 2 Tafeln. Vorgelegt in der Sitzung am 17. November, 1865. Vienna, 1866. 4to, 78 pages.

Kennicott, Robert.—Observations on temperature, rain, snow, winds, and clouds, made at Fort Yukon, Russian America, from January to July, 1861.

Kongelige Danske Videnskabernes-Selskab.—Oversight over det Kongelige danske Videnskabernes Selskabs Forhandlingers og dets Medlemmers Arbeider i Aaret 1864. Af Conferentsraad, Professor G. Forchhammer, Selskabets Secretair. 8vo, 192 pages. [Contains monthly tables of observations at Copenhagen, and other meteorological articles.]

Kongelige Norske Universitet.—Meteorologiske Iagttagelser paa Christiania Observatorium. 1865. Christiania, 1866. 4to, 50 pages.

Koninklijk Nederlandsch Meteorologisch Instituut.—Meteorologisch Jaarboek. Eerste Gedeelte. Waarnemingen in Nederland. Uitgegeven door het Koninklijk Nederlandsch Meteorologisch Instituut. 1865. Utrecht, 1866. Oblong quarto, 198 pages.

Meteorologisch Jaarboek. Tweede Gedeelte. Afwijkingen van Temperatuur en Barometerstand op vele Plaatsen in Europa. Uitgegeven door het Koninklijk Nederlandsh Meteorologisch Instituut. 1865. Utrecht, 1866. Oblong quarto, 192 pages.

Kron, Dr. F. J.—Thermometer, cloud, and wind record kept at Attaway Hill, near Albemarle, Stanly county, North Carolina, from April to October, 1861, inclusive.

Lapham, I. A., LL D.—Table showing the mean amount of evaporation and of rain in each month of the year, deduced from observations made during five years; also the amount of rain in each month of the year 1866, and the monthly mean of twenty-three years; also the mean temperature for each month during the years 1865 and 1866, and the monthly and annual mean of twenty-two years, at Milwaukee, Wisconsin. Newspaper.

Logan, Thomas M., M. D.—Abstract of meteorological observations made during the year 1866, at Sacramento, California, with extended remarks on the climate of Sacramento. Newspaper.

McAdoo, William G.—Catalogue of plants observed at and near Milledgeville, Georgia, with the times of their leafing, flowering, fructification, and fall of leaf in the year 1866 Two sheets foolscap.

Merrill, John C.—Record of the direction of the wind, morning, noon, and evening, from January to June, 1866, inclusive, at Farmingdale, Queen county, New York. Fourteen pages small pass-book.

Mathews, Joseph McD.—Summary of observations for the year 1866, at Hillsboro', Ohio.

Meteorologische Centralanstalt der schweizerischen naturforschenden Gesellschaft.—Schweizerische meteorologische Beobachtungen, herausgegeben von der meteorologischen Centralanstalt der schweizerischen naturforschenden Gesellschaft, unter Direction von Professor Dr. Rudolph Wolf. Zweiter Jahrgang. 1865. Zurich. 4to.

Meteorological Society, (British.)—Proceedings during the year 1866. Edited by James Glaisher, esq., F. R. S., secretary. 8vo.

Naturforschende Gesellschaft in Emden—Die Regenverhältnisse des Königreichs Hannover, nebst ausführlicher Darstellung aller den atmosphärischen Niederschlag und die Verdunstung betreffenden Grössen, welche beim Wasserbau sowie beim rationnellen Betriebe der Landwirthschaft in Betracht kommen. Von Dr. M. A. F. Prestel. Mit einer Regenkarte und zwei lithographirten Tafeln. Emden, 1864. 4to, 56 pages.

Naval Observatory.—Notes on vegetation at the Naval Observatory, Washington, D. C., Captain J. M. Gilliss, superintendent, kept by Mr. James Watt, gardener, during the years 1862 and 1864.

Nieder-Oesterreichische Landes- oder Real-Schule, in St. Pölten.—Zweites Programm der nieder-oesterreichischen Landes- oder Real-Schule in St. Pölten. 1865. 8vo. [It contains "Beiträge zur Meteorologie, St. Pölten, von Karl Swoboda." Twenty-four pages.]

Nova Scotian Institute of Natural Science.—Proceedings of the Nova Scotian Institute of Natural Science, of Halifax, Nova Scotia. Vol. 1, part iv, for 1865–'66. [Contains: Notes on the weather at Halifax, Nova Scotia, during 1865, by Colonel W. J. Myers, F. B. M. S. Also, abstract of observations for the years 1863, 1864, and 1865, at Halifax, by the same.]

Observatorio Magnético y Meteorológico del Real Colegio de Belen de la Compañia de Jesus en la Habana, Francisco Pons, director.—Monthly bulletins of observations. 8vo

Oesterreichische Gesellschaft für Meteorologie.—Zeitschrift der österreichischen Gesellschaft für Meteorologie. Redigirt von Dr. Carl Jelinek und Julius Hann. I. Band. Vienna, 1866, 8vo., 392 pages.

Plantamour, E.—Résumé météorologique de l'année 1865, pour Genève et le Grand Saint-Bernard, par E. Plantamour, professeur. Tiré des Archives des

Sciences de la Bibliothèque Universelle, Août 1866, avec autorisation de la direction. Genève, 1866. 8vo, 142 pages.

Quetelet, Ad.—Etoiles filantes observées à Bruxelles, le 10 Août 1865, par MM. Ad. Quetelet, Ern. Quetelet, et Hooreman.—Sur les orages observés en Belgique pendant le mois d'Août 1865, par M. Ad. Quetelet, secretaire perpétuel de l'Academie. (Extrait des bulletins de l'Academie Royale de Belgique, 2me serie, tome xx, nos. 9 et 10.) 8vo, 15 pages.

Quetelet, Ernest.—Sur l'état de l'atmosphère à Bruxelles, pendant l'année 1865, par M. Ernest Quetelet, membre de l'Academie Royale de Belgique. (Lu à la séance du 3 Février 1866) 8vo, 48 pages.

Reale Osservatorio di Palermo.—Bulletino meteorologico del Reale Osservatorio di Palermo. 4to, 8 pages. Published monthly.

Reichel, Edward H.—Manuscript meteorological journal, kept at Nazareth, Pennsylvania, (sixty miles north of Philadelphia,) from January, 1787, to December, 1792, by Charles Gotthold Reichel, principal of the Nazareth Hall Boarding School for Boys, from 1785 to 1802. [The journal gives the temperature twice a day, the wind, face of the sky, and a part of the time the hygrometer and barometer.]

Royal Society of London.—Results of meteorological and magnetical observations made at Stonyhurst College Observatory, England, during the year 1865. 31 pages, 12mo.

Sartorius, Dr. Charles.—Summary of observations for the year 1866, at Mirador, Mexico.

Scottish Meteorological Society.—Journal of the Society for the year 1866. Published quarterly.

Société de Geographie, Paris.—Observations thermometriques faites à Djeddah dans le courant de l'année 1864, par M. Pellissier de Reynaud, consul de France à Djeddah. (Published in the bulletin of the society, 5th series, vol. 10. July to December, 1865, pp. 197–202.)

Société Météorologique de France.—Annuaire.

Spanish Minister.—Observationes meteorolójicas hecas en el Observatorio Astronómico de Santiago, durante el año de 1865, i en el Faro de Valparaiso hasta Agoste del Mismo, inclusive. Santiago de Chile, 1866. 46 pages. 1866.

State Department, Washington.—The Royal Standard, January 12, 1867, a newspaper published at Grand Turk, Turk's Island, containing the report of President Moir to his government in relation to the hurricane of September 30, 1866.

Tayloe, Edward D.—Meteorological and agricultural notes, from 1831 to 1865, inclusive, at Powhatan Hill, King George county, Virginia. On sixteen sheets Smithsonian blanks.

Trembley, Dr. J. B.—Annual meteorological synopsis for the year 1866. By J. B. Trembley, M. D, Toledo, Ohio. 8vo, 16 pages.

Valentin, Felipe.—Observations (thermometer, psychrometer, state of atmosphere, wind, and rain) made daily at 7 a. m., 2 p. m., and 7 p. m., at Limon, in the province of Cartago, republic of Costa Rica, from the 16th of October, 1865, to August 31, 1866.

Vivenot, jun., Dr. Rudolph Edl. v.—Beiträge der klimatischen Evaporationskraft, und deren Beziehung zu Temperatur, Feuchtigkeit, Luftströmungen und Niederschlägen, von Dr. Rudolph Edl. v. Vivenot, jun. Erlangen, 1866. 8vo, 103 pages.

Walker, David, M. D.—Observations of barometer, thermometer, winds, and weather, taken at Victoria, Vancouver's island, from December, 1863, to June, 1864; also at Fort Steilacoom, Washington Territory, during the same time. 16 pages foolscap.

Whitehead, W. A.—Summary of observations for the year 1866, at Newark, New Jersey, and comparison with the preceding twenty-two years. Newspaper.

Wilbur, B. F.—Abstract of meteorological register kept at West Waterville, Maine, during the year 1866. Newspaper.

Williams, Prof. M. G.—Summary of observations for the year 1866, at Urbana, Ohio.

Unknown—Levant Herald, Constantinople, January 9, 1867, containing a summary of the weather, including observations of the barometer and thermometer, at Constantinople, for each month of the year 1866

REPORT OF THE EXECUTIVE COMMITTEE.

The executive committee respectfully submit the following report in relation to the funds of the Institution, the receipts and expenditures for the year 1866, with estimates for the year 1867.

Statement of the fund.

The original amount received as the bequest of James Smithson deposited in the treasury of the United States as a trust fund, by act of Congress, approved August 10, 1846, section. 2..........	$515, 169 00
The residuary legacy of James Smithson, received in 1865, invested in United States 7.30 bonds..........	26, 210 63
Extra fund, detived from saving of interest, &c., invested in stock, viz:	
United States 7.30 bonds..........	27, 939 37

	Par value.	Cost.	Present value.	Interest unpaid.	
Indiana 5 p. cent.	$75, 000	$63, 000 00	$69, 750	......	
Virginia 6 p. cent.	53, 500	49, 832 50	31, 565	$17, 655	
Tennessee 6 p. cent.	15, 000	11, 167 50	9, 300	......	
Georgia 6 p. cent.	500	500 00	500	165	
Washington 6 p. cent.	100	100 00	100	......	
	144, 100	124, 600 00	111, 215	......	111, 215 00

Total productive and unproductive capital..........	$680, 534 00

Receipts and expenditures during the year 1866.

RECEIPTS.

Balance in hands of the Treasurer, January, 1866..........	$13, 718 63
Interest on the original bequest of Smithson, viz., 6 per cent. on $515,169..........	30, 910 14
Interest on Indiana stock, viz., 5 per cent. on $75, 000..........	3, 750 00
Interest on Tennessee stock, viz., 6 per cent. on $15,000..........	900 00
Premium for sale of coin, &c..........	40, 691 26
Interest on temporary deposit with United States Treasurer..........	380 82
Proceeds of sale of $600, coupons, for interest due on Tennessee stock..........	402 00
Total funds available for the year 1866..........	90, 752 85

EXPENDITURES.

For building and furniture..........	$36, 428 66	
" general expenses..........	11, 577 24	
" publications and researches..........	13, 109 95	
" library, museum, and literary exchanges..........	6, 745 77	
		67, 861 62
Balance in hands of the Treasurer, January, 1867..........		$22, 891 23

Statement in detail of the expenditures during the year 1866.

BUILDING.		
Reconstruction of parts injured or destroyed by fire,	$33,291 81	
Repairs and incidentals to old parts	2,181 54	
Furniture and fixtures	955 31	
		$36,428 66
GENERAL EXPENSES.		
Meetings of the board: travelling expenses and hack hire	127 00	
Lighting and heating	1,555 54	
Postage	452 84	
Stationery	485 14	
Incidentals: hardware, tools, materials for cleaning, &c.	1,213 12	
Salaries: Secretary, chief clerk, bookkeeper, clerks, janitor, messenger, and laborers	7,743 60	
		11,577 24
PUBLICATIONS, RESEARCHES, ETC.		
Smithsonian Contributions to Knowledge	3,776 17	
Smithsonian Miscellaneous Collections	5,702 67	
Smithsonian Annual Report	1,377 80	
General printing	243 27	
Meteorology	1,017 90	
Apparatus for experiments and researches	496 85	
Laboratory, chemicals, &c.	122 98	
Exploration	372 31	
		13,109 95
LIBRARY, MUSEUM, AND LITERARY AND SCIENTIFIC EXCHANGE.		
Cost of books	873 24	
Assistants in library, (January to July)	600 00	
Literary and scientific exchanges	2,009 33	
Assistants in museum, and incidentals for collections,	1,568 04	
Freights on specimens, books, &c., for museum and library	1,695 16	
		6,745 77
Total expenditures during the year 1866		$67,861 62

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 made.

The Commissioner of Agriculture has continued to pay one-half of the salary of the meteorological clerk, who has been engaged during the year in preparing the abstracts from the reports of the observers for the Monthly Bulletin of the Agricultural Department.

The appropriation received for the preservation of the collections of the exploring 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 employés in the museum, and the cost of arranging and preserving the articles. This appropriation is $4,000 annually, but during the last year an additional sum of $2,000 was received, due for the year 1865, which had not been paid on account of the failure of the appropriation bill to pass Congress at the usual time.

The specimens intrusted to the care of the Institution are in a good condition, although on account of the unfinished state of the building much care and labor have been required to preserve them from the effects of dampness.

The committee have examined three hundred and eighty-two vouchers, embracing several thousand items, for payments during the year, amounting collectively to $75,211 35. All these vouchers are for moneys actually expended, and for legitimate purposes of the Institution, and are in the form which has heretofore been in use. The committee, however, would advise that in future the following rules, similar to those in use by government, be adopted:

1. Vouchers to be made out on blanks furnished by the Institution.

2. All receipts for money paid to be signed by a principal, and not by an agent unless legally authorized.

3. Each voucher to be complete in itself; no payments to be made on a running account.

During the year the State of Tennessee made arrangements to issue six per cent. coupon bonds to pay off the arrearage of interest which had accumulated during the war. The claims for interest to be converted into these new bonds were limited to amounts of not less than $1,000. As the interest due the Institution was $3,600, Mr. Riggs was instructed to sell the odd $600 worth of coupons and convert the remaining $3,000 into stock. The sale of these coupons yielded $402, which sum has been credited in the receipts of the Institution for the year, and three bonds of one thousand dollars each, added to the permanent investment.

It is impossible, until it is known what further action the board will take in regard to the disposition of the stocks, to give a definite estimate of the receipts and expenditures during the present year. The following, however, may be considered as an approximation sufficiently exact for the general appropriations for the operations of the Institution, and the continuance of the work upon the building:

Estimates for the year 1867.

RECEIPTS.

Balance in hand January, 1867	$22, 891 23
Interest on original bequest..................................	30, 910 14
Probable premium on coin.....................................	10, 000 00
Interest on $54,150 in United States 7.30 bonds, unpaid coupons from February 15, 1865, to February 15, 1867..............	7, 905 90

Coupons due on same August 15, 1867	$1,976 47
Interest on Indiana 5 per cent. stock	3,750 00
Interest on Tennessee 6 per cent. stock	900 00
Interest on Washington 6 per cent. stock for 1863-'4-'5-'6 and '7	30 00
Total available income	78,363 74

It is proposed to apply this as follows:

1. For the operations and support of the Institution, viz: Publications, researches, literary and scientific exchanges, museum, salaries, and general expenses	$40,000 00
2. For the reconstruction of the building	38,363 74
	78,363 74

The income of $78,363 74 above stated does not include any interest on the Virginia and Georgia stocks. Their value at the present time is about $32,000, being the amount of the unproductive funds of the Institution.

The Indiana stock yields but five per cent. on the par value. It can now be sold and lent to the United States at six per cent., which will increase the annual income from this source at least $300.

The Georgia and Washington stocks can now be sold at par, and the proceeds lent to the United States will increase the annual income from this source at least $30.

The Tennessee stock at present is worth about 62 per cent. on its par value. It pays 6 per cent. on the par value, and about 9.6 per cent. on the market value.

The Virginia stock is worth at the present time about 59 per cent. on the par value, and pays no interest. A large amount of the debt of this State is secured by stocks in the railroads and other internal improvements amounting to many millions, and it is therefore considered most advantageous to retain this stock until it commands a more favorable price.

The committee, in conclusion, have the satisfaction of reporting to the regents the fact that the entire bequest of Smithson remains undiminished in the treasury of the United States; that all the expenditures from the organization of the establishment to the present time, including nearly $400,000 on the building, have been made exclusively from the income derived from the bequest, while at the same time the efficiency of the Institution has been increased by an extra fund of upwards of $100,000.

RICHARD DELAFIELD,
RICHARD WALLACH,
Executive Committee.

NOTE.—Since the presentation and adoption of this report, by authority of the Board, the United States 7.30's, the Indiana, Georgia, and Tennessee stocks have been sold, and the proceeds added to the permanent capital, increasing it from $515,169 to $650,000.

REPORT OF THE BUILDING COMMITTEE.

The restoration of the building has been prosecuted during the last year as rapidly as the funds at the disposal of the committee and the character of the work would permit.

It will be recollected that the damage occasioned by the fire consisted principally in the destruction of the roof and upper story of the main building, the interior of the large north and south central projections, and the towers connected with them.

It will be further recollected that in the reconstruction of the building, as stated in the last report, the committee were governed by the following considerations:

1. To render the work entirely stable, both as to material and mode of construction.

2. To render it thoroughly fire-proof.

3. In view of the great cost at present of material and workmanship, and the means at the disposal of the committee; to do first, such work as would be necessary to preserve the stability of the several parts of the building, and prevent injury by the weather.

Immediately after the fire, a temporary covering was placed over the main building in such a position as not to interfere with the subsequent erection of the permanent roof. To secure the northern towers, a 9-inch lining of brick was laid in cement from the bottom to the top, and firmly united to the original wall. In this and all the other parts of the building reconstructed, wrought-iron girders and brick arches were substituted for wooden beams and floors.

The large south tower was so much injured that thirty feet of the upper portion had to be taken down and rebuilt, the cost of which was much enhanced by the necessity of recutting a large amount of new stone for the facing. This tower has been divided into six stories, affording as many large rooms, the lower for an extension of the museum, an upper one for the meetings of the Regents, and the others for storage, &c. The offices for the accommodation of the Secretary and assistants will be in the northern towers and connecting space.

The principal access to the second story of the main building is by two large iron staircases, one on either side of the northern entrance. These have been completed.

All the towers and connections with the main building have been covered with substantial roofs. After much inquiry and personal investigation it was concluded to adopt the plan for the roof of the main building of wrought-iron framing, and slate covering, the latter secured in place by wire to iron purlines, and pointed underneath by a coating of cement.

A contract was made for the roof in July with the Phœnix Iron Company, of Philadelphia, the proposals from which were the lowest; but owing to unexpected delays the iron frame was not received until too late in the season for putting on the slate, without injury to the cement by frost. The work will, however, be prosecuted as early in the spring as the condition of the weather will permit.

The inside lining of the walls of the second story of the building, which had been much injured by the fire, has been removed and its place supplied by a new 9 inch brick wall laid in cement, securely tied and clamped to the outer stone-work.

The chairman of the committee has given personal attention to the work in its progress, and can state from actual knowledge that the plans, material and workmanship are of a satisfactory character, alike creditable to the talents and careful supervision of Mr. Cluss, the architect.

The following is an exhibit of the whole amount expended up to the end of the year 1866, on the reconstruction of the building:

Detailed statement of payments made on the reconstruction of the Smithsonian building, from January 1, 1865, *to January* 1, 1867.

Iron-work—beams, doors, frames, &c	$12,728 58
Iron-work—staircase north hall	4,043 25
Iron-work—new roof	482 55
Stone, from quarry at Seneca creek, Maryland	1,468 94
Stone cutting and setting	14,165 40
Brick, for walls, floors, &c	7,242 27
Bricklayers	9,371 34
Lumber	2,185 84
Carpenters and laborers	6,571 22
Cement	1,436 52
Sand and lime	558 69
Blacksmiths	544 82
Hardware—nails, tools, clamps, steel, &c	705 32
Rope, blocks and derricks	173 15
Painting	46 00
Tin roof on northeast tower	168 05
Felt and pitch for repairing temporary roof	212 92
Freight and hauling on iron beams and stone	1,027 89
Slate for new roof	182 60
Compensation of architect	1,788 75
	65,104 10

In addition to the above amount which was expended for the reconstruction of the building, under the superintendence of Mr. Adolf Cluss, architect, the following amounts were paid for work done for the preservation of the building after the fire:

Removing debris and clearing up	$1,055 29	
Temporary wooden roof	1,974 25	
Carpenter's work	254 75	
Blacksmith's work	28 25	
Glazier's work	121 95	
Plasterer's work	52 00	
Glass, oil and paint	544 50	
		4,030 99

Making the whole amount expended on the building in consequence of the fire, during 1865 and 1866	$69, 135 09
There was also expended during 1865 and 1866 for general repairs on the parts of the building not injured by the fire, the introduction of additional water-pipes, new plugs, hose, hardware, lumber &c., the sum of	4, 268 97
Whole expenditure on building for 1865 and 1866	$73, 404 06

It is proposed during the present year to complete the roof of the main building and all the rooms in the projections and towers, leaving the large room of the upper story of the main building unfinished until funds may be accumulated for the purpose, and its future use be determined upon.

Respectfully submitted:

RICHARD DELAFIELD,
Chairman Building Committee.

WASHINGTON, *February* 1867.

JOURNAL OF PROCEEDINGS

OF

THE BOARD OF REGENTS.

WASHINGTON, *January* 16, 1867

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

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

WASHINGTON, *January* 28, 1867.

A meeting of the Board of Regents was held at 8 p. m., in the laboratory of the Institution.

Present: Chief Justice Chase, Chancellor of the Institution; Hon. Garret Davis, Hon. J. W. Patterson, Hon. J. A. Garfield, General Richard Delafield, Professor Louis Agassiz, Hon. Richard Wallach, Mr. William B. Astor, and the Secretary, Professor Henry.

The Chancellor took the chair.

Professor Henry presented a general statement of the operations of the Institution during the year, and of the present condition of the funds.

He also stated that, on account of the advanced state of the session of Congress at the time of the passage of the resolution by the Board directing a memorial to be presented in regard to increasing the amount of the Smithsonian capital in the treasury of the United States, and also on account of the apprehension that a larger sum would be required for the completion of the building than was at first expected, he had deferred action until the present session of the Regents, when further instructions could be given in regard to the matter.

After remarks by the Chancellor and other members of the Board relative to the desirableness of increasing the amount in the treasury as the best means of permanently securing the capital of the Institution, and of exhibiting to the world the care and judgment with which the finances have been managed,

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

Resolved, That a committee of three be appointed to present a memorial to Congress in behalf of the Board of Regents, requesting the passage of an act authorizing the Treasurer of the United States to receive into the treasury, on the same terms as the original bequest, the residuary legacy of James Smithson, now in United States bonds in the hands of said Treasurer, namely, \$26,210 63, together with such other sums as the Regents may from time to time see fit to deposit, not exceeding, with the original bequest, the sum of one million dollars; and that the income which has accrued or which may accrue from said residuary legacy be applied in the same manner as the interest on the original bequest.

The Chancellor appointed Messrs. Davis, Patterson, and Garfield as the committee.

General Delafield, from the Building Committee, made a report relative to the work done and the expenditures on the reconstruction of the building during the past year.

On motion of General Garfield, the report was accepted and the committee continued.

Professor Agassiz remarked that he had just read the report of the Librarian of Congress, in which it was stated that the addition of volumes from the Smithsonian Institution had increased the library of Congress one-third, and he wished to call attention to the source of the accumulation of so large and valuable a library as that of the Smithsonian, one of the best of the kind in the world. He attributed it to the policy which had been proposed by the Secretary, and so long sustained by the Regents, of publishing original contributions to science, and sending these, with the greatest liberality, to every part of the world, in return for which so many volumes of the transactions of learned societies had been received, and without which system of publication and exchange the present reputation of the Institution and such a library could not have been acquired.

The Secretary stated that, since the last meeting, the death had occurred of Mr. W. W. Seaton, Treasurer of the Institution and one of the original Regents.

Mr. Seaton was for many years mayor of the city of Washington, and in several of his messages recommended the councils to urge Congress to take measures for the organization of the Smithsonian Institution, and it was, finally, through his personal influence and that of others interested in the cause of science, that the law of 1846, authorizing its establishment, was passed. The mayor of Washington is ex-officio, a Regent, a provision chiefly due to the zealous interest which Mr. Seaton had manifested in the advocacy of the measure. At the first session of the Board of Regents he was appointed Chairman of the Executive Committee, a member of the Building Committee, and Disbursing Officer. At the close of his term as Regent he was elected Treasurer, which office he retained until his death, rendering gratuitous service during the whole period.

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

Resolved, That the Regents of the Smithsonian Institution have learned with deep regret the decease of WILLIAM W. SEATON, late Treasurer of the Institution, who has been connected with it from its organization, and was one of the original members of the Board. His long and gratuitous services to the Institution entitled him to our thanks, and his loss, in common with the citizens of Washington and of the whole country, we deplore.

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

On motion of General Garfield, the Secretary was directed to have these resolutions, and a suitable notice of the late Mr. Seaton, inserted in the next annual report.

The Secretary stated that it would be necessary to make provision for supplying the place of Mr. Seaton, and perhaps some new arrangements in regard to the method of keeping the accounts, paying bills, &c.

The Chancellor suggested that the income of the Institution should be placed in the treasury of the United States, or in a government depository; and,

On motion, it was

Resolved, That a committee be appointed to consider the subject of the proper deposit of the income, and of any change which might be necessary in regard to the system of accounts, and report at the next meeting.

The Chancellor appointed General Delafield, General Garfield, and the Secretary as the committee.

The Board then adjourned to meet on Friday evening, at 7½ o'clock.

WASHINGTON, *February* 1, 1867.

A meeting of the Board of Regents of the Smithsonian Institution was held this day at 8 o'clock p. m. in the laboratory of the Institution.

Present: Chief Justice Chase, Chancellor; Hon. L. F. S. Foster, President of the Senate; Hon. L. Trumbull, Hon. J. W. Patterson, Hon. J. A. Garfield, Hon. J. F. Farnsworth, General R. Delafield, Professor Louis Agassiz, Hon. R. Wallach, and the Secretary, Professor Henry.

The Chancellor took the chair.

The minutes were read and approved.

The Secretary presented a petition from William De Beust, asking for remuneration for losses by the fire at the Institution.

On motion of Mr. Foster, it was

Resolved, That the Secretary be authorized to settle with William De Beust at a fair valuation for his tools lost during the fire at the Institution in January, 1865, in full payment of his claims, the amount not to exceed five hundred dollars.

The Secretary presented the following memorial which had been offered to Congress by the special committee:

MEMORIAL OF THE BOARD OF REGENTS OF THE SMITHSONIAN INSTITUTION.

To the honorable the Senate and House of Representatives in Congress assembled:

The Board of Regents of the Smithsonian Institution have directed the undersigned to transmit to your honorable body the resolution herewith appended, and to solicit the passage of an act in accordance therewith.

It is known to your honorable body that the original sum received into the United States treasury from the bequest of James Smithson, of England, was

$515,169, which was considered a trust fund, the interest alone to be applied to carrying out the purpose of the testator, viz: "The increase and diffusion of knowledge among men."

This, however, was not the whole of the Smithsonian bequest, the sum of £5,015 sterling having been left by Hon. R. Rush, the agent of the United States, as the principal of an annuity to the mother of the nephew of Smithson.

The annuitant having died, the sum of $26,210 63 has been received from this source, and is now in charge of the Secretary of the Treasury of the United States; and no provision having been made in the act of August 10, 1846, establishing the Institution, for the disposition of this remainder of the legacy, your memorialists, in behalf of the Board of Regents, now ask that it be added to the original bequest on the same terms; and that the increase which has arisen from interest or otherwise on the sum before mentioned, also in the hands of the Treasury Department of the United States, be transferred to the Board of Regents for assisting to defray the expense of the reconstruction of the building, and for other objects of the Institution.

And your memorialists would further ask that the Board of Regents be allowed to place in the treasury of the United States, on the same terms as the original bequest, such sums of money as may accrue from savings of income and from other sources, provided the whole amount thus received into the treasury shall not exceed one million dollars.

The sole object of this request is the permanent investment and perpetual security of the entire Smithsonian bequest and such other sums as may be accumulated from savings of accrued interest, legacies, &c.

And your memorialists will ever pray, &c.

S. P. CHASE,
Chancellor.
JOSEPH HENRY,
Secretary Smithsonian Institution.

Resolved, by the Board of Regents of the Smithsonian Institution, That an application be made to Congress for an act authorizing the Treasurer of the United States to receive into the treasury, on the same terms as the original bequest, the residuary legacy of James Smithson, now in United States bonds in the hands of said Treasurer, namely, $26,210 63, together with such other sums as the Regents may from time to time see fit to deposit, not exceeding, with the original bequest, the sum of $1,000,000; and that the income which has accrued or may accrue from said residuary legacy be applied in the same manner as the interest on the original bequest.

Mr. Patterson stated that in behalf of the committee he had presented the memorial to the House of Representatives, with a bill in accordance therewith, which had passed unanimously that day, and been transmitted to the Senate.

Mr. Trumbull stated that this bill had also unanimously passed the Senate, and only awaited the signature of the President to become a law.

The subject of the future policy of the Institution in regard to the museum and the appropriation of the large hall in the second story of the building, was brought before the board by the Secretary, and remarks were made by Professor Agassiz, Mr. Patterson, General Garfield, General Delafield, the Chancellor, and the Secretary.

On motion of Mr. Patterson, it was

Resolved, That a committee be appointed to consider what will be the best use for the large room in the second story of the main building of the Institution.

The Chancellor appointed Messrs. Trumbull, Patterson, Agassiz, and the Secretary as the committee.

General Delafield, from the special committee appointed at the last meeting in regard to the depository of the funds, reported in part, and asked for further time, which was granted.

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

FEBRUARY 22, 1867.

A meeting of the Board of Regents was held at 7 p. m., in the room of the Committee on Foreign Affairs, House of Representatives, United States Capitol.

Present: Chief Justice Chase, Chancellor, Hon. W. P. Fessenden, Hon. J. W. Patterson, Hon. J. A. Garfield, Hon. J. F. Farnsworth, Hon. R. Wallach, General R. Delafield, and Professor Henry, Secretary.

The Chancellor took the chair and the minutes were read and approved.

The Secretary stated that it had become his painful duty at this time to announce the departure from life of another and one of the most important and highly esteemed members of the Board. Since the last meeting Professor Alexander Dallas Bache, head of the United States Coast Survey and Regent of the Smithsonian Institution, had, after a protracted illness, died, on the 17th February, at Newport, Rhode Island, in the sixty-first year of his age; that though this occurrence must be felt as a sad calamity by all familiar with the progress of art, science, and education in this country for the last forty years, and by all who had been favored with a personal acquaintance with the deceased, yet he begged to be permitted to say that none, save his bereaved widow, could feel the loss more deeply than himself. He had been on terms of brotherly association with him for more than thirty years. It had been principally through the influence of Professor Bache that he had been induced to venture to accept the appointment of Secretary of this Institution, and that with the sympathy, counsel and support of the deceased, he had been enabled, through all the eventful changes which had since taken place, to continue the discharge of the responsible duties of the office.

On motion of Mr. Patterson, it was

Resolved, That the highest honor is due to the memory of our respected and beloved associate, Professor ALEXANDER DALLAS BACHE, who, through so many years of active life, has devoted, unselfishly and with untiring energy, great talents, profound acquirements, and undeviating integrity to the advance of art, science, education, and philanthropy.

Resolved, That in the death of our lamented associate, this Institution, of which he was a Regent and one of the executive committee from its first organization to the time of his death, has lost an efficient collaborator, a sagacious counsellor, and zealous supporter.

Resolved, That the members of the Board, in common with the Secretary, lament in his departure the loss of a warm and tried personal friend, and that they will always cherish the memory of his genial and sympathetic disposition, his gentle and prepossessing manners, his refined taste, high moral perceptions, and unswerving advocacy of the right.

Resolved, That a copy of these resolutions be transmitted to the widow of the deceased, and that the Secretary prepare a suitable eulogy for insertion in the next Annual Report.

General Delafield presented the annual report of the Executive Committee, which was read and adopted.

The Secretary presented the following copy of the act of Congress relative to the increase of the trust fund, referred to at the last meeting of the Board, and a statement of what had been done in accordance with it.

[PUBLIC—No 20.]

AN ACT authorizing the Secretary of the Treasury to receive into the treasury the residuary legacy of James Smithson, to authorize the Regents of the Smithsonian Institution to apply the income of the said legacy, and for other purposes.

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That the Secretary of the Treasury be, and he is hereby, authorized and directed to receive into the treasury, on the same terms as the original bequest, the residuary legacy of James Smithson, now in United States bonds in the hands of said Secretary, namely, twenty-six thousand two hundred and ten dollars and sixty-three cents, together with such other sums as the Regents may from time to time see fit to deposit, not exceeding with the original bequest the sum of one million dollars.

SEC. 2 *And be it further enacted,* That the increase which has accrued, or which may hereafter accrue, from said residuary legacy, shall be applied by the Board of Regents of the Smithsonian Institution in the same manner as the interest on the original bequest, in accordance with the provisions of the act of August tenth, eighteen hundred and forty-six, establishing said Institution.

Approved, February 8, 1867.

The Secretary stated that in accordance with the directions of the Board of Regents, and the authority conferred by the above act, he had increased the amount of the Smithsonian fund in the treasury of the United States on the 19th of February, 1867, to $550,000, in the following manner:

The interest, at 7 3-10 per cent., due for two years, to February 15, 1867, on the $54,150 U. S. bonds, was collected, viz:		$7, 905 90
$25,400 of the bonds were taken by the Treasury Department at 6 per cent. premium, yielding Bonds	$25, 400	
Premium.....	1, 524	
		26, 924 00
Interest from 15th February to 19th, four days............		20 32
Amount realized....................................		34, 850 22
Amount placed in the United States treasury, to be added to the original trust fund, $515,169, (making it $550,000).......		34, 831 00
Leaving a balance in cash of......................		$19 22

This balance was deposited with Riggs & Co. to the credit of the Smithsonian account.

After discussion as to the further increase of the permanent capital,

On motion of Mr. Wallach, it was

Resolved, That the Chancellor and Secretary be directed to dispose immediately of the remaining United States 7-30 bonds, and the Indiana, Georgia, and Washington bonds, now held by the Institution, and, if possible, from the proceeds to place an additional hundred thousand dollars in the treasury of the United States.

Mr. Patterson requested further time for the committee on the future use of the large hall in the main building of the Institution, which was granted.

General Delafield presented the following report from the special committee:

REPORT ON ACCOUNTS, ETC.

The committee to whom was referred the subject of the deposit of the income, and any changes which may be advisable in the system of accounts, respectfully report that they have carefully considered the matters referred to them, and recommend as follows:

1. That the income being collected according to law, under the authority of the Chancellor and Secretary, be deposited in a national bank, which is also an authorized government depository, subject to draft, as hereinafter provided.

2. That no money be drawn from the depository except for the payment of accounts which have been examined and approved by the Secretary of the Institution, and which are in accordance with appropriations made by the Regents.

3. That all payments for bills exceeding twenty dollars, be made by separate checks, drawn by the Secretary on the depository, and that for bills of less amount, a check may be drawn for a number, as exhibited in a single statement.

4. That there be rendered quarterly to the Executive Committee, by the Secretary, an account current, accompanied by abstracts of disbursements, supported by receipted vouchers, for the expenditure of all money drawn from the depository. These vouchers to be made, as far as practicable, in accordance with the forms hereto annexed, each voucher being complete in itself, and to contain the facts and information thereon indicated.

5. That a check-book, ledger, and such other books of record as may be necessary, be kept in the office of the Secretary, so as to exhibit at any time the financial condition of the Institution. The check-book shall indicate not only the sums deposited, and drawn, but also the sources whence they were derived, and the appropriation under which they were paid.

6. That there be obtained from the depository, quarterly, an account current of receipts and expenditures, to be compared by the Executive Committee with the accounts of the Secretary.

7. That the Executive Committee make a quarterly examination of the books and accounts of the Institution, and, as usual, an Annual Report of the Board of Regents.

Respectfully submitted:

RICHARD DELAFIELD,
JAMES A. GARFIELD,
JOSEPH HENRY,
Committee

FEBRUARY 22, 1867.

On motion, the report was adopted.

Professor Henry announced the death of two gentlemen who had long been identified with the Institution in connection with explorations and collections in natural history, Robert Kennicott, esq , of Illinois, and Dr. Henry Bryant, of Massachusetts.

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

Resolved, That the Board of Regents have heard with deep regret of the death of Mr. ROBERT KENNICOTT and Dr. HENRY BRYANT, gentlemen so long associated with the Institution in its system of explorations and collections relative to natural history, and that it tenders to the families and friends of the deceased their heartfelt condolence in their loss.

The Secretary presented the usual account of the operations of the Institution for the year 1866, which was accepted, and, on motion of Mr. Wallach, it was

Resolved, That the Secretary communicate the annual report of the Board of Regents to Congress.

The Board then adjourned *sine die.*

A SKETCH OF THE SERVICES OF THE LATE HON. W. W. SEATON IN CONNECTION WITH THE SMITHSONIAN INSTITUTION, AND SOME NOTICES OF HIS LIFE AND PERSONAL CHARACTER.

AMONG the many friends and enlightened advocates of the Smithsonian Institution, as at present established, none has been more constant and more efficient than the distinguished and lamented citizen to whose memory the following brief and imperfect notices are dedicated. These notices are designed, in the first place, to convey some idea of the character and value of his services in connection with the above-named establishment, as well by his co-operation in securing for it as prompt and suitable a commencement as the circumstances of the time permitted, as by sustaining eventually that plan of organization which corresponded most nearly with the terms and spirit of the bequest to which it owes its existence. He is to be remembered also as a benefactor of the Institution by his gratuitous and faithful discharge, for a series of years, of one of its most responsible executive offices.

As is well known, the Smithson fund, paid into the treasury of the United States in 1838, had been, with other moneys, lent by the government to the State of Arkansas, and remained for eight years without appropriation to any object contemplated by the donor. In 1846 Mr. Seaton, being then mayor of Washington, and surpassed by no one in zeal for the public good and in the influence due to his rare social qualities, his known integrity, and peculiarly winning and unaffected eloquence, united with other gentlemen of like feelings in urging upon Congress the organization of an establishment which should at length do justice to the benevolent and far-sighted views which had dictated the bequest. Their labors, after much opposition, were finally crowned with success; the good faith of the country was redeemed by an unconditional assumption of the debt incurred by the improper disposition of the fund, which was now declared to be a permanent deposit in the treasury of the United States for the objects of the trust, while interest also was allowed upon the money from the time of its receipt in this country. The Institution, organized in accordance with these resolutions, was placed under the guardianship of fifteen regents, among whom was included the mayor of the city of Washington, a provision chiefly due to the zealous interest which had been manifested by Mr. Seaton in his enlightened advocacy of the enterprise.

At the first meeting of the Board of Regents he was elected treasurer, and subsequently one of the building committee. The former office he continued to hold until the time of his death, and during the whole of this period, nearly twenty years, discharged its duties without other compensation than the pleasure he derived from an association with the Institution and the laudable pride he felt in contributing to its prosperity and usefulness. It is well known that at the time of the organization of the Institution a wide diversity of opinion existed as to the practical means which would be most suitable for realizing the objects of the legacy. Mr. Seaton, on mature reflection, finally gave his cordial support to the policy which sought to impress upon the Institution a truly cosmopolitan character. He strenuously advocated the plan which the Secretary, then recently elected, had been invited to submit to the Board of Regents, and which looked to the advancement of knowledge chiefly through the encouragement and publication of original researches, a system which, without neglecting other available means for the promotion and diffusion of scientific enlightenment, may be claimed, without undue pretension, to have made the Institution favorably known, and to have exerted a well-recognized influence wherever men occupy themselves with intellectual pursuits.

The relation borne by Mr. Seaton to the city of Washington, the delight with which he watched and aided its progress, a certain native taste also for artistic embellishment, led him to take special interest in the architectural character of the Smithsonian building and the ornamentation of the public grounds around it.

Mr. Seaton was a constant attendant at the meetings of the Board of Regents, and from his familiarity with the early history of the Institution and the state of the funds, as well as from his long experience in public office, was enabled to offer suggestions, always marked by clearness of conception and soundness of judgment. The social attentions which he was accustomed to extend to the regents, especially those who were called from abroad to attend the annual meetings, and to gentlemen invited to lecture before the Institution, were but the expression of his characteristic hospitality; but by thus adding to the pleasure of their sojourn in Washington, he contributed largely to increase the number of its friends and supporters. The columns of the National Intelligencer, under his direction, were always open to the defence of the policy adopted and the course pursued by the Institution, and he rarely failed to soften by the courtesy of his manner and the moderation of his expressions, any irritable feeling which might arise in the discussion of conflicting opinions. It would, indeed, be difficult to say in how many and in what various ways he contributed to the popularity as well as to the true interests of the Institution. The Secretary, who was in the habit of conferring with him on all points requiring mature deliberation, may with justice acknowledge that he never failed to derive important assistance from the wisdom of his counsels.

Of a man so highly honored and, what in his case is a more distinctive phrase, so greatly beloved by his fellow-citizens, the following biographical account, gethered from a communication kindly furnished us by an esteemed correspondent, will prove, we are confident, neither uninteresting nor uninstructive. It will evince that his eminence was won not less by diligence in the pursuit of a useful and laborious profession than by the graces of his personal character; not less by his unwavering adhesion to principle and duty, than by the flexibility with which he knew how to adapt himself to all the classes of men with whom his varied life brought him into contact.

He was a descendant in the direct line of a family of no little note in the annals of Scotland. But however famed for wisdom in council and valor in the field, the Seatons had for centuries been scarcely less distinguished for an unfaltering attachment to the royal house. Hence, when the government of the revolution of 1688 was finally established, Mr. Henry Seaton, with a number of other Scotch gentlemen, despairing of any retrieval of the fallen cause of the Stuarts, was led to cross the ocean and seek a new home and new fortunes on the hospitable soil of colonial Virginia. It was in King William county, Virginia, that William Winston Seaton, a lineal descendant of Henry Seaton, first saw the light, January 11, 1785. His mother bore the name of Winston, a family originally from Yorkshire, in England, but long settled in Virginia, where it has always enjoyed great social consideration and influence. Mr. Seaton's early training was under the roof of his father, Mr. Augustine Seaton, at a time when the state of society may be said to have been peculiarly conducive to the formation of habits of self-reliance, independent thought, and a scrupulous regard for the feelings of others. To the love of study he added the love of the chase, a taste which accompanied him through life, and which, exacting robust exercise and steadiness of aim, seemed to have left its impress on his figure and action when he had reached the age of more than fourscore years. In the days to which we now refer, schools and colleges were not always at hand; books were not strewn broadcast through the country; the library, which existed as an heirloom of some old family, was such as they had brought with them from the Old World, received only rare accessions, and afforded none of those helps to easy knowledge which have perhaps extenuated our mental culture in the same proportion that they have extended it. If any special source of the love of letters, the refined taste, and varied acquirements which so highly distinguished Mr. Seaton might be pointed out, perhaps it would be found in the influence exerted on his opening mind by a Scotch gentleman then living as a refugee in Virginia, the well known Ogilvie, earl of Finlater, of whom few persons living at that day had not something to tell, as well respecting his eccentricities as his diversified accomplishments. But the mind on which these various influences acted was in itself well disposed for vigorous and independent exertion. At the early age of seventeen young Seaton was already prepared to enter on the business of life, and having adopted political journalism as his future pursuit, he gave it his constant services to the close; with how high and just a reputation for editorial equity, skilful management, and fulness of information, is well known to all who have been observers of the public events of our time.

After acting for some time as assistant editor of a Richmond paper, he passed, not without the prestige of early developed talent and force of character, to the sole management of the Petersburg Republican, and subsequently to that of the North Carolina Journal, of Halifax. Both Virginia and "the Old North State" were then peculiarly agitated by the passions and turbulence of political partisanship; the position of editor was not only environed with difficulties, but attended with danger. Mr. Seaton, with a diffidence which was always characteristic, had at first hesitated to accept a post on which so much depended for his party, but every one who knew him will believe that when he had consented to lead the attack on the stronghold of federalism at Halifax, he brought to the service precisely those qualities which were requisite for its success—firmness of purpose, consistency of principle, courtesy to opponents, fairness as well as force in discussion. It is claimed for him in fact that mainly through his well directed exertions the reign of federalism was subverted in that part of the State where he labored, and the ascendency fully transferred to those rules of constitutional construction which were then known alike by the name of *democratic* and *republican.*

We next find Mr. Seaton, scarcely twenty years of age, established at Raleigh, and associated with Joseph Gales, senior, in the editorship of the Register, the most influential journal of the State. It was in the family of Mr. Gales that two incidents bearing with the most important results on the future career and welfare of Mr. Seaton occurred. It was here that he met with the late Joseph Gales, junior, son of the former, with whom he was destined to maintain in the sequel an editorial connection of nearly fifty years, a connection which has inseparably associated their names, and whose fruits, as embodied in the columns of the National Intelligencer, will ever constitute an invaluable monument of the history and policy of their times. It was in the bosom of the same family also that the crowning happiness of his life was realized, in his union with Miss Sarah Gales, daughter of his editorial chief, and sister, therefore, of his future associate. "To refrain on this occasion from drawing aside for a moment the veil which covers the sanctity of domestic life, would be to omit the most interesting and graceful chapter of Mr. Seaton's personal history. His union with the honored partner of his life was marked by a mutual tenderness so seldom paralleled, by a devotion so chivalrous on the one part, a reliance so trustful and unhesitating on the other, that it must ever be referred to as the crown and complement of his earthly existence. The loveliness and good report of this conjugal example were treasured, it may be said, as a personal pride and possession by the community in which, for fifty-four years, the virtues, the talents, the ineffable grace of true womanhood, as exhibited in the person of Mrs. Seaton, sustained and cheered the toils of her husband in his arduous career."

In the mean time, Joseph Gales the younger had been forming himself, under the skilful guidance of his father, for the duties of a profession in which he was destined to attain an eminence that few have approached, and eventually became the proprietor of the National Intelligencer, then established at Washington. In this enterprise he was subsequently joined by his brother-in-law, Mr. Seaton, nor at the time of his accession did their united talents want for occupation in the exasperated state of party feeling and the imperilled condition of the country. It was in 1812, and hostilities had already been declared against Great Britain. Without entering into the questions which then convulsed the public mind, it is sufficient to say that the Intelligencer gave its earnest and able support to the party which regarded the declaration of war necessary to maintain the power and secure the rights of the United States. In this state of things it was not likely that either of our editors would confine his efforts in behalf of the cause he had espoused to the labors of the pen and press; they were both members of volunteer corps, and shared in the expeditions which were organized, from time to time, to repress the predatory incursions of the enemy. But the labors of the pen and press were, in their case, felt to be of too much national importance to be dispensed with, and a furlough granted alternately to one and the other editor, provided for the uninterrupted appearance of the sheet to which the public chiefly looked for authentic information and the vindication of governmental measures. Mr. Seaton was at the editorial post on the morning of the memorable 24th of August, 1814, when the report of the distant gun told too surely that the enemy was advancing in force on the ill-prepared metropolis. Hastily despatching his workmen to their respective corps, Mr. Seaton himself hurried to the front and arrived in time to take part in the sharp initiative conflict which preceded the disasters brought on the American arms by incapacity or want of concert on the part of the leaders and the consequent disorganization of an untrained and badly armed militia. The rout of Bladensburg led the enemy directly into the city, and the results of the occupation, as regards the destruction of the public buildings, are matters of familiar history. It is not perhaps so generally known that a singular attestation was, at the same time, unintentionally afforded by Admiral Cockburn to the widespread fame and commanding influence of the National Intelligencer. He caused the office of its publication to be sacked and its valuable contents to be destroyed; too many incitations to patriotic effort had issued from that sanctuary to escape an ignoble vengeance.

It has been seen that the course of Mr. Seaton's life, from a period little advanced beyond boyhood, was such as to insure, indeed to necessitate, an intimate familiarity with the men and events of his time, with all changes of public opinion, with all discussions of constitutional law, with all the movements of interest, prejudice and affection by which the affairs of the world are governed. The thoughts, the passions, the motives of his fellow-men were necessarily with him subjects of scrutinizing observation and intelligent reflection. When he removed to Washington the sphere of his observation and influence was of course greatly widened. The trusted friend and counsellor of the earlier administrations, there can be no doubt that, as he was the depositary of their confidence, he often contributed in no small degree to shape their measures. The intimate and honored associate afterwards of such men as Adams, Webster, Clay, Calhoun, Berrien, and all the eminent statesmen of the past age, he could scarcely fail, with his quick and penetrating intelligence, to gain such insight into public affairs and to gather such stores of varied information as are rarely within reach of a single mind. To him, therefore, would naturally resort politicians and statesmen of every cast; for it was instinctively felt that in consultation with him there was a candor which knew no disguise, a courtesy which never failed, a fulness of information and clearness of judgment which his intrinsic goodness of heart placed at the service of all who needed them. The same would be the case with the representatives of foreign governments, and with all enlightened strangers, and thus his influence was often propagated to other countries than his own. But this subject has been so graphically, if quaintly, touched by a contemporary journalist that we cannot do better than use his words: "There is a parlor in Colonel Seaton's old house at Washington," says this writer, "which, could its walls speak, would be more eloquent than the walls of any other apartment in America. In that well-known room it was not uncommon—we should rather say it was for years a weekly custom—for the greatest men in the country, and the representatives of other nations, to gather in the freedom of social intercourse. And this may be said with undoubted truth, that in those free social conversations and exchanges of thought were born many of the great measures of government which added lustre to the American name, so that that room may be regarded as the birthplace of much of our national glory."

After having filled successive municipal offices, Mr. Seaton, in 1840, yielded to solicitations which had been often resisted, and accepted from the citizens of Washington the dignity of the mayoralty, the highest which, under the public law, it has ever been in their power to confer. During the succeeding ten years, in which he was uninterruptedly recalled with unprecedented unanimity to preside over the affairs of the city, it is superfluous to say that he brought to the discharge of his duties a fidelity and energy which distinguished him in all situations, and which have stamped his administration as a model worthy of imitation by all civic dignitaries. It would seem indeed to have rested only with himself to fill the office to perpetuity, for when, at the end of the above period, he peremptorily declined a re-election on the score of advancing years, his retirement was regarded by all with undissembled regret. Nor is this matter of surprise; for if, in the unswerving discharge of duty, he had evinced an impartiality and firmness worthy of honor, he had still more won the popular heart by personal qualities which appealed to the sensibilities of all the good and all the suffering. Accessible to all classes, listening with patient sympathy to the story of need or wrong, which was ever promptly relieved or redressed, tenderly considerate of the humble and poor, his charity a household word wherever he was known, he called forth a respect and love not accorded to the many, and at last descended to the grave crowned by the blessings of those to whom the withdrawal of his earthly presence seemed little less than a domestic calamity.

One point only in Mr. Seaton's municipal administration is it thought needful here to particularize; his persistent efforts in the establishment of the present admirable system of public schools in Washington. A just tribute to his important work in behalf of education has been thus rendered by a municipal colleague: "When Mr. Seaton entered upon the duties of the mayoralty there were only two public schools in the city; but justly estimating the value of a new and improved system, he continued from year to year to press the subject on the attention of the legislative branches of the government, until it was adopted in the fourth year of his administration, from which time the number of schools has increased until their scholars now amount to twice as many thousands as there were hundreds at the time of his inauguration. Among the many beneficent acts of his official life this will stand pre-eminent; and among the many friends in whose hearts his memory will be longest cherished, there will be thousands who, but for his efforts, would have been denied the blessings of education, and the manifold benefits resulting from that mental and moral culture which the children of all classes of our fellow-citizens have since enjoyed by means of the liberal and enlightened system he so opportunely introduced and established."

This rapid sketch would be culpably deficient did we not endeavor to convey some idea of the rare personal gifts and virtues of Mr. Seaton, in the sphere where they naturally shone with a more benign lustre—his home; and this we prefer to do in the words of one who knew and loved him well: "The centre of all household thought; obeyed by his inferiors with a service of love recalling a patriarchal age; it was at home, in the daily amenities of domestic and social life, that he was supreme. Who can forget Mr. Seaton as host? In the gatherings about his generous board mingled the cordial welcome and that air of an older and better school which constantly distinguished him—the kindly and reassuring attention, unaffectedly bestowed on the least distinguished guest, the colloquial charm, which extended the fame of his hospitality far beyond the sphere of its exercise. His conversation was indeed of an exalted character, lighted up by a quaint humor and ready wit, enriched with varied and solid information derived alike from men and books, marked also by originality of thought, by an utter absence of self-assertion or dogmatism, by a delicate tact in shielding others from the wound which a thoughtless or unkind word might inflict, and in drawing forth to the best advantage the talents and attainments of each. Doubtless, no unimportant part of the charm exercised by Mr. Seaton resided in his engaging presence—in the winning smile, the bright eye, the gentle voice, the benignity of a countenance upon which a long life of manly effort and kindly purpose had left its impress. In recalling these characteristics some idea may be conveyed of an attractiveness which was not only widely recognized among ourselves, but acknowledged by foreigners, especially the diplomatic representatives of other governments, solicitous of obtaining from his lips an explanation of our involved politics and those views of public measures which have been known on several noted occasions to have materially influenced the deliberations of foreign cabinets and determined their international policy."

We know not how we can better close this account of the life and character of a lamented colleague than by quoting the following passage from a discourse delivered on the occasion of his death: "One of the finest intellects of this country, and of the most devout, almost austere, evangelical faith, has repeatedly said, 'that of all the men he had ever known Mr. Seaton was nearest perfection and most ready to enter God's presence.' One of the texts on which the deceased sometimes dwelt as being to him exceedingly suggestive was, 'As a man *thinketh* so *is* he;' and this might be termed the key-note to his own character; without guile, trusting all, believing in all, his wide mantle of charity and love covering all creeds, all humanity, his was truly 'the spirit which thinketh no evil.' One of his most striking characteristics was the indomitable courage which, through life and to its extremest verge, led him to brave difficulties and adversities with the same calm, unflinching decision with which he confronted personal danger, and which enabled him to endure, with unsurpassed fortitude, the sufferings of mortal illness. Self-reliant, self-poised, upheld by his consciousness of right and just endeavor, firm in his grasp of the immutable principles of truth, and in his reliance upon a gracious and superintending Providence, his life was guided by a sense of responsibility as a free moral agent, from whom a strict account of the talents committed to his charge was hereafter to be exacted. He was constant and fervent in prayer. Often, months before, and during his illness, his voice was heard in the stillness of night raised in petition to God. His last words on earth, the last tones of his voice vibrated with the name of the Saviour. He was a 'devout Christian; exalted honor his instinct, Christianity his guide.'"

GENERAL APPENDIX

TO THE

REPORT FOR 1866.

The object of this appendix is to illustrate the operations of the institution by 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.

MEMOIR

OF

MAGENDIE.

BY M. FLOURENS, PERPETUAL SECRETARY OF THE FRENCH ACADEMY OF SCIENCES.

TRANSLATED FOR THE SMITHSONIAN INSTITUTION BY C. A. ALEXANDER.

"WHEN one has otherwise a great deal of merit," says Fontenelle, "a general conformity with other men is an added merit." In the truth or value of this maxim the eminent academician of whom I am about to speak was so far from acquiescing that, in speculation as well as practice, he was generally prone to assert a perhaps undue independence of the opinions and contempt for the observances of others.

Of a spirit firm but sceptical, upright but aggressive, if his quick penetration enabled him to discover truth, if he knew how to exhibit it with simplicity and clearness, he was just as capable of putting forth a rough energy in combating it whenever it did not come to him of its own accord. We might imagine him armed with the lantern of Diogenes, and concentrating its light to see only the results which he himself obtained; results, indeed, which elucidate one of the most delicate points in the human organism, and will insure the duration of a name for which he has earned honor and consideration. That name had been transmitted to him by a surgeon, originally of Béarn, who exercised his profession at Bordeaux when François Magendie was born, October 15, 1783. In the case of this infant, the pious solicitude, the tender affection which nature reserves for the first stage of life, were sadly abridged; he was deprived, by an acute malady, of his mother, almost before he could know the happiness of being loved by one.

To the pleasing unconcern and confidence of infancy succeeded a precocious and rude apprenticeship. Transplanted, as early as 1792, to Paris, he heard nothing spoken of but the superlative work of *Social Regeneration*. His father, a man of upright purpose, but incapable of allowing a folly to pass without taking his share in it, conceived that, in order to endow his son with a civic energy corresponding to the elevation of his own principles, it was necessary to educate him according to the precepts delivered by Jean Jacques. The new Emilius, left to his own devices, wandered at will, with a liberty which strongly resembled absolute abandonment. In order to preserve him from instructions which might warp his judgment, he was left, on a principle of education, in complete ignorance. His only resource as regarded the world of intelligence was observation, which alone (so said his guide) could secure to him entire *independence*.

Finding, perhaps with reason, less difficulty in reforming abuses than in combating maladies, the enthusiastic patriot had abandoned a practice which annoyed him for the more congenial pursuit of unproductive civic appointments. With the practice went the comforts of the household; but what imported such a sacrifice as this? The exaggeration of patriotism and the reality of discomfort proceeded to such lengths that he would have constrained his pupil, while seeking to persuade him that this would be a further step towards independence, to make his own shoes. At this point, the good sense of the young man revolted; he protested against all these follies; declared that he preferred to be dependent and well-shod, and concluded by asking to be sent to school.

The primary school had no pupil more ardent; admitted late, and endowed with an energetic will, the young Magendie quickly outstripped all competitors. His father was not at all shocked at the inequality which his son found the means of establishing from the first; but very generously pardoned him for it, and clapped his hands at hearing the great prize adjudged to this neophyte of fourteen years for an exercise "*On the knowledge of the rights of man and the Constitution.*" The *Journal des Hommes Libres* soon afterwards announced "that there was still hope for the most tender age, when the corrupting poisons of the reaction had not blasted it in its first bloom, since the son of citizen Magendie, municipal officer, elector, member of the commune, &c., having met with a child who was weeping and dared not appear in the presence of his father, had comforted, encouraged, and carried him back to the bosom of his family," a tender asylum which had often been wanting to this extemporized protector himself. A prize of *virtue*, ostentatiously awarded on this occasion, completed the glorification of the young republican. It was with reference to this incident that, in later life, M. Magendie, when the *liberty* was taken by any one of protesting against the too real asperities of his *temper*, would pleasantly reply that before his fifteenth year he had obtained the proud triumph just spoken of under the regime of *equality*, which must not be supposed to mean *equality of temper;* but that few certainly could boast of so precocious a virtue as himself.

According to the nearly constant practice of those who preach liberty, the father of M. Magendie reserved the exclusive use of it for himself. He announced to his son that, not to derogate from the family dignity, he must prepare to invest himself with the robe and bonnet of the doctor. It had been well if at the same time he could have inculcated the unhesitating faith and placid self-importance, essential qualifications, from which the acute and discriminating genius of the young man was soon to claim perpetual dispensation.

Introduced into the hospitals, the future adept there commenced his studies. The judicious Boyer chose him for his prosector, and in a few months this prosector transformed himself into a professor of anatomy.* Having obtained by competition the position of house-surgeon, (*interne,*) at the age of eighteen M. Magendie was competent to his own support. He distributed his time into three parts: the larger share was devoted to study;† a second portion was reserved for instruction, which, practiced from the outset, extended to all that he himself had learned, and formed at once the delight and resource of his youth;‡ then, poor but self-reliant, although the impressions received from his father had imbued him with a republican roughness, yet by a sort of instinct, the lively and last spark of the social distinction of his mother, he loved and courted the refinements of good society; aristocratic and culpable refinements, which however educate the character, mould the taste and invigorate the intellect; these had for him something of the prestige of forbidden fruit. A third part of his time, therefore, was consecrated to the intercourse of those saloons which, after the revolutionary tempest, had opened at the first return of tranquillity, and in which,

* It was about this time that he contracted his intimacy with Dr. Ferrus, who is known for his ingenious investigations and just views regarding the maladies of the mind.

† With his medical he combined literary studies. Although he had been reared at an epoch when it was the custom to swear only by Athens and Rome, when habits, manners, and principles were all borrowed from those two republics, he had been taught nothing of the ancient languages. Desirous of supplying this deficiency, he found the means of doing so through the excellent courses of M. Lemare, which were attended also by some of his most distinguished cotemporaries. He always felicitated himself in after life for having had courage for this undertaking.

‡ He thereby formed a talent which was peculiarly his own, that, namely, of teaching forthwith whatever he had learned, and at the moment of learning it. This communicated an indelible stamp to his instructions, which was particularly pleasing to the young.

through a memory of the common calamity, all were friends. Here he was received as a young man of elegance and worth, and here he dissembled, with truly Roman stoicism, the distresses of his situation. "Yet," as he himself would jocosely say in after times, "during a period which seemed no short one, there remained for me, all deductions made, not more than five sous a day to live upon; and still I had a dog. We shared with one another; and if he was not fat, neither was I."

This energetic labor, this patient poverty, this aspiration for distinction, fortify and elevate the spirit. Honor to the poor student who sustains them! If, with his scanty appointments, in his bare apartment, he cheers his vigils and animates his ceaseless labors by a dream of success and glory, he does not deceive himself; it is at this price that they are purchased.

M. Magendie became assistant, and then prosector to the Faculty. This scholastic apprenticeship opened for him a career; his dexterity as an anatomist, his coolness, his hardihood, gave presage of a superior surgeon. But the life of compelled fellowship, of equality reduced to practice, the contact of rivalships never divested of means of offence, proved an intolerable ordeal for this austere and imperious nature. From this ordeal sprang an invincible repugnance to all acknowledged competition, and in order to escape this contingency he renounced surgery.

To find in this world a path where one's elbows shall be free is no easy matter. Our difficult youth brooded with so melancholy a spirit over the obstacles before him that his retreat was invaded by that bitter discouragement which long suffering entails, and which the youth, especially the young physician, never fails to attribute to some malady, assumed to be incurable, but which invariably vanishes before a gleam of good fortune. Magendie wished no longer to live; in fact he asserted that he could not. But one morning a man of the law presents himself at the asylum of the student, who, having neither process nor business, asks with natural surprise, what was wanted with him? "Nothing," replied the stranger, "that can be disagreeable to you. You have become the heir of a sum of twenty thousand francs, and I am here to place it at your disposal."

The invalid found himself at once in a state of convalescence. Accepting this unexpected supply as but a temporary remission of the severity of his life, he immediately made arrangements for the acquisition of showy horses and sportive dogs, all placed in the care of a sprightly and fashionable groom, who was charged, besides, with the duty of keeping a light equipage always in readiness for the use of the improvident but joyous owner of these superfluities. And that not a moment of this transient prosperity might be lost, and at the same time no encroachment be allowed on his conscientious studies, the whole of this apparatus of luxury was lodged as near as possible to the hospital. "Thither," said M. Magendie afterwards, "I used to run when I had an instant to spare, so that my whole recreation was literally centred in the stable." The twenty thousand francs were, of course, soon spent, but a little relaxation does much good; it had renewed the elasticity of his spirit.

Independence, the golden dream of youth, seemed to withdraw itself for M. Magendie within a circle which left him no choice but to be a physician, if *in spite of himself*. Such he was in effect, but he indemnified himself by maintaining a state of permanent revolt by obstinately refusing to yield his faith and homage to what he called *the grand idol of human credulity*. This conflict, in which he displayed infinite address and good sense, discovers to us the sceptic disentangling from prejudices the art which he respects, and thus giving himself the right to make his acquisition a somewhat costly one to the profession—a profession which must still honor the superiority of his views and the austere probity of his character.

The physicians of antiquity, beginning with Hippocrates, were at once physicians, surgeons, and apothecaries. "In the sequel," says Fontenelle, "the physician was divided into three; not, however, that one of the ancients was worth three of modern times." *Three* might suffice for the days of Fontenelle; at present it would be necessary to divide into four. Under a vigorous impulse, which is not yet spent, a new science had established its right of naturalization in our schools. Physiology, full of promise, lending itself readily to doubt and controversy, captivated the enterprising genius of Magendie, and opened for him a path to independent distinction.

"We anatomists," said the old academician, Mery, "are like the porters of Paris, who know all the streets, even to the smallest and most obscure, but who know nothing of what is passing within the dwellings." Physiology is precisely the knowledge of what is passing within the human dwelling.

The study of the forces by which life is nourished and maintained was cultivated in antiquity only by Galen, whose genius cast upon it some admirable gleams of intelligence. A long silence followed; more philosophic than practical, more inquisitive than indispensable, at least in the beaten routine of human culture, this science was transmitted without progress to modern times. In the seventeenth century, an English physician, guided by some confused lights derived from a school of Italy, and protected by an enlightened sovereign, ventured, in spite of popular prejudice, to practice upon living animals experiments which had become indispensable for the solution of the problem he had proposed for investigation. In this way Harvey detected the secret mechanism by which heat and life are maintained in the animal organism, and demonstrated the circulation of the blood. This discovery was a catastrophe for our old Faculties,* accustomed to enjoy in peace all the sweets of time-honored ignorance; they protested, they conspired, but in vain; the day of their ascendency was at an end.

From the identical school in which Rabelais had taken his degrees before going forth to scourge fools and false knowledge with the lash of his caustic genius, proceeded, in 1648, a young man who, applying himself to persistent research, succeeded, like Harvey, in demonstrating a grand phenomenon, the course of the chyle, thereby completing the explanation of our vivifying forces. The admirable discovery of Pecquet has scarcely availed, however, to save his name from oblivion.†

Expelled by the vindictive thunders of our medical councils, physiology fled for refuge to a German university, to Göttingen. There, under the inspiration of Haller, was opened a series of delicate and profound investigations,‡ ascending from the study of the organs to that of the springs by which they are set in action, a progression which leads to the loftiest summits of philosophy.

At length appeared, at the commencement of the present century, the daring genius for whom was reserved the distinguished mission of popularizing phys-

* See my work entitled *Histoire de la Decouverte de la Circulation du Sang.* Paris, 1857, (second edition)

† No *eloge* of Pecquet has been read in our academy. Astruc barely names him in his *Histoire de la Faculté de Medicine de Montpellier;* and I have nowhere been able to find the date of his birth. Condorcet, in his *Liste des Membres de l'Ancienne Academie*, contents himself with saying: "He made in his youth, while at Montpellier, the discovery of the thoracic duct and of the reservoir of the chyle." Now, Pecquet did not make the discovery of the *thoracic duct*, made nearly a century before by Eustachius. He made that of the *reservoir of the chyle.* He traced—and this was the capital point—all the *lacteal* or *chyliferous* vessels (which Aselli, who had discovered them, still supposed to proceed to the *liver*) to that *reservoir*, by that reservoir to the *thoracic duct*, and by the thoracic duct to the heart, thus changing the whole doctrine on the course of the chyle. (See my *Histoire de la Decouverte de la Circulation du Sang.*) Sprengel very properly says: "Certainly the discovery of Pecquet shines not less in the history of our art than the truth for the first time demonstrated by Harvey." *(Histoire de la Medicine.)* The truth would be, the discovery of Pecquet yields, in physiology, only to that of Harvey.

‡ The experimental analysis of the vital forces begins with the two fine memoirs of Haller on *sensibility* and on *irritability*. (See my work entitled *De la Vie et de l'Intelligence*, second part, p. 69)

iology in France. Bichat combined with the experimental method of Haller bold and judicious views, drawn from a source where prejudice had been accustomed to look only for style; he threw into technical language the ideas of Buffon;* invested them with the forms of the school, and supported them by anatomical demonstrations. By the energetic cast of his genius Bichat swayed his cotemporaries to the study of a science for which his own ardor extended even to the sacrifice of his life. One of his school-fellows, Le Gallois, died also in the same task; but invested neither with the prestige of easy eloquence, nor with the facilities of success afforded by comradeship, this modest precursor of modern studies on the nervous system obtained from renown nothing but the scantiest justice.†

Le Gallois was still living when M. Magendie presented himself in the arena as a champion of undoubted pretensions. In his own person a strange but favorable result of a training conducted on the principles of Jean Jacques, completed by the severest teachings of our first republic, he had proceeded to frame for himself a code of duties; a code which did not save him from the eccentricity of manifesting by turns the most inflexible selfishness and admirable disinterestedness; a rigid probity in the statement of his own labors, a culpable injustice or contempt for those of others; a hard and intolerant humor towards every one who seemed to stand in his way; a kindness and generosity without bounds for the feeble or afflicted.

It was by criticism that M. Magendie first made himself known.‡ In 1808 he reproached Bichat with having abandoned himself to hypotheses, and declares that, for his own part, he shall admit no facts which do not find their confirmation in experiments competent for himself to repeat. In 1809 he presented to the Academy of Sciences a memoir on one of the most important phenomena of the animal economy, that of *absorption*. If an active substance, a poison or virus, is introduced into any part of the body, that substance is immediately absorbed—that is to say, is carried from the more superficial parts to the deeper and more essential. By what organs is this transfer effected? Is it by the veins, or by the lymphatic vessels? Haller thought that it was by the former; John Hunter, by the latter; other physiologists hesitated. By a bold experiment, M. Magendie suppresses the lymphatic vessels;§ he leaves only the veins;

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* See, as regards the obligations of Bichat to Buffon, my work, *De la Vie, &c.*, part second, p. 17 *et seq.*

† See the work of Gallois entitled *Expériences sur la Principe de la Vie, notamment sur Celui des Mouvements du Cœur et sur le Siége de ce Principe.* Paris, 1812. It is from this book, a conscientious and profound labor, that dates, in France, the physiological study of the nervous system.

‡ See his memoir entitled *Quelques Idées Génerales sur les Phénomenes particuliers aux Corps vivants, (Bulletin des Sciences Medicales*, 1809, p. 145.) M. Bernard, the distinguished disciple and friend of M. Magendie, has given a good summary of his little tract, which is essentially but a criticism of the *vital properties* of Bichat. "Why invent," asks M. Magendie, "on occasion of each phenomenon of living bodies, a particular and special vital force? Might we not content ourselves with a single force which we should call *vital force* in a general manner, in admitting that it gives rise to different phenomena according to the structure of the organs and tissues which operate under its influence? But is not even this single vital function too much? Is there not here a simple hypothesis, since we cannot detect it? It would be of greater advantage if physiology only commenced at the instant when the phenomena of living bodies become appreciable by our senses." *(Notice sur M. Magendie; Leçon d'Ouverture du Cours de Medicine au College de France*, 1856, p. 7.)

§ "M. Delille and I separated from the body the thigh of a dog previously comatized with opium; we left untouched only the crural artery and vein, which preserved the communication between the thigh and the trunk. These two vessels were dissected with the greatest care; their cellular tunic was removed, for fear that it might contain some lymphatic vessels. Two grains of a very subtle poison *(upas ticuté)* were then injected into the paw; the effects of this poison were as prompt and intense as if the thigh had not been separated from the body. It might be objected that, in spite of all the precautions taken, the walls of the crural artery and vein still contained lymphatics, and that these vessels sufficed to give he even substitutes for the veins a *quill*, since the walls of the veins might possibly still contain some lymphatic branches; absorption takes place as rapidly as usual, and thus absorption by the veins is demonstrated.

At all times there have been useful dissidents who have seemed charged with the function of keeping our learned corporations awake. Such an one, among the predecessors of Magendie, was the physician Chirac, who had heretofore maintained before the assembled faculty that the stomach remains inactive in *vomiting*. Death had overtaken Chirac before he could conclusively verify the fact. So favorable an opportunity could not escape M. Magendie; he proved, by a decisive experiment, that Chirac was right, and that in the act of vomiting the stomach does in fact remain inactive.*

In a multiplied series of labors,† one of the most ingenious is that which he made public, in 1817, on the *elasticity* of the arteries. Doubts were still entertained respecting the precise faculty by which the arteries co-operate in the movement of the blood. By some it was maintained that they were *irritable* or *contractile* like the muscles; by others that they were altogether *passive*. M. Magendie proved that they are active, but in a manner which is peculiar to

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passage to the poison. In order to remove this difficulty, I repeated on another dog the preceding experiment, with this modification, that I introduced into the crural artery a small quill, on which I secured this vessel by two ligatures; the artery was then cut circularly between the two ligatures; the same thing was done for the crural vein: by this means there was no longer communication between the thigh and the rest of the body, if not by the arterial blood which flowed to the thigh and the venous which returned to the trunk. The poison, afterwards introduced into the paw, produced its effects in the usual time—that is, at the end of about four minutes." (Magendie, *Precis Elementaire de Physiologie*, t. II, p. 265, third edition.)

* "It had been long thought that vomiting depended on the sudden and convulsive contraction of the stomach; but I have shown that this viscus is nearly passive in the act, and that the true agents in vomiting are, on the one hand, the diaphragm, and, on the other, the large muscles of the abdomen; I have even succeeded in producing it when, in a living dog, I substituted for the stomach the bladder of a hog which I afterwards filled with a colored liquid." (Magendie, *Precis Elem. de Physiologie*, t. II, p. 154.)

† It will not be out of place to give here a summary account of a few among the numerous works of Magendie not alluded to in the text.—*Memoire sur l'usage de l'epiglotte dans la deglutition*, 1813. From the experiments of this memoir, the author concludes: 1st, that the epiglottis is not indispensable to the integrity of deglutition; 2dly, that it is especially the movement by which the glottis is closed that guards the larynx during the passage of aliments swallowed.—*Memoire sur les images qui se forment au fond de l'œil et sur un moyen tres-simple de les apercevoir*, 1813. This means consists in making use, for the examination of the images formed at the bottom of the eye, of the eyes of *albino* animals, (rabbits, pigeons, &c.,) in which the sclerotic coat is transparent.—*De l'influence de l'emetique sur l'homme et les animaux*, 1813.—*Memoire sur l'œsophage et ses fonctions*, 1813.—*Memoire sur la deglutition de l'air atmospherique*, 1813. These three memoirs complete the *Memoire sur le vomissement*.—*Memoire sur les propriétés nutritives des substances qui ne contiennent pas d'azote*, 1816. The result of this investigation is that substances which do not contain azote (sugar, gum, &c.) are improper for nutrition. Although the animals submitted to experiment were allowed these substances at discretion, and even consumed much of them, they did not the less certainly die of *inanition* at the end of some days. Still further, it is shown that whatever the aliments employed, whether azotized or not, it is necessary to vary them. "A rabbit and a Guinea pig, nourished with a single substance, such as wheat, oats, barley, cabbage, carrots, &c., die, says M. Magendie, with all the appearances of inanition, usually in a fortnight, but sometimes much sooner. Nourished with the same substances given concurrently, or successively at short intervals, these animals live and thrive. The most general and essential consequence to be deduced from these facts is that diversity of aliments is a most important rule of hygiene." (*Precis element. de physiologie*, t. II, p. 504, 505.)—*Recherches physiologiques et medicales sur les symptomes et le traitement de la gravelle*, 1818. "The persons attacked by gout and gravel," says M. Magendie, "are ordinarily great eaters of flesh, fish, cheese and other substances abounding in azote, (nitrogen.) Most of the urinary gravels, a part of the urinary calculi, arthritic tophus, are formed by uric acid—a principle which contains much azote. By diminishing in the regimen the proportion of azotized aliments, we succeed in preventing and even curing gout and gravel." (*Precis element. de physiologie*, t. II, p. 503.)—*Recherches physiques et physiologiques sur l'ipecacuana*, 1816. The result of these researches, made in common with M. Pelletier, was the discovery of the active principle of ipecacuanha or *emetine*.

them.* Their action depends on their *elasticity*, an elasticity which is very decided, but yet purely physical. The first object of experiments in physiology is the *distinction of forces;* M. Magendie already recognized this great aim, and attains it here by a skilful discrimination. The circulation of the blood commences through the *contractility* of the heart, a vital force, and is continued through the *elasticity* of the arteries, a physical force.

It is well known by how profound an aversion for all conjecture, by how exact an observation of facts, the researches of M. de Laplace were marked, and how much they contributed to maintain in the academy the severe spirit of the experimental method. Laplace desired that all science should be but an assemblage of facts rigorously concatenated; and after having, according to the felicitous expression of M. Cuvier, *subjected the heavens to geometry,* he did not despair probably of establishing the same order of things upon earth. The decisive and absolute manner in which the young physiologist was accustomed to draw his conclusions, not always safe, however, from subsequent retraction, appeared to Laplace well suited to the style of a geometer. And if it be an essential quality of the latter to yield deference to no one, few certainly could, in this respect, be better qualified for the function than our colleague.

M. Magendie, confident in his own strength, held himself aloof with a disdainful pride, eluding every ordinary encouragement. But it came to pass that one day the illustrious, the rigid, the judicious Marquis de Laplace volunteered the first advances towards him. Electricity is less potent, for this acts not on the spirit, than are the few words of encouragement which fall from the lips of a great man. Our sceptic thought himself secure from all enthusiasm, and was only the more hurried away by it. It was not long after this that M. de Laplace said to his old friend M. de Montyon: "It is greatly to be regretted that learned

* The conclusions at which he arrived are: "1st, that the arteries, great and small, present no trace of *irritability.*" This we now know to be too absolute; the arteries, especially the small ones, are *irritable.* This does not hinder, however, their function in the circulation from being principally due, as Magendie says, to their *elasticity*—to the elasticity of their *middle membrane,* their *yellow tissue.* "2dly, that the contraction of the left ventricle and the *elasticity* of the arteries supply a sufficient mechanical reason for the movement of the blood in these vessels." (Magendie, *Journal de physiologie experimentale,* t. I, p. 114.)

Long afterwards, in one of his best lectures at the college of France, he recurred to the part borne by the *elasticity* of the arteries in the circulation, and with a development well worth remarking: "The part which elasticity bears in the great act of circulation is too important not to detain us for a moment. The heart—a central organ which may be compared to an hydraulic pump—is intended to force continually, but by alternating impulsions, blood into the system of tubes, which goes on subdividing itself, and constitutes what we call arteries. These become reduced into extremely slender ducts which, under the name of capillary vessels, proceed to inosculate with another system of tubes—the veins—which convey the blood back from the periphery to the common centre whence it flowed. Such, on a large scale, is the phenomenon of circulation. It is readily conceived that the contraction of the left ventricle may be sufficiently energetic to drive the liquid into the arterial system, but is its action propagated even into the capillary and venous conduits? This problem must now be resolved in the affirmative." [See on this point the researches of M. Poiseuille, one of his pupils, whom Magendie regarded as doing him most honor.] "A first phenomenon is this: the heart, each time that it contracts, throws into the arterial system a wave of blood, and as each contraction is alternate, it follows that the blood must be projected by interrupted jets. This consequence is plain; yet observe now what passes in the vessels where it circulates. If we open an artery near the heart the blood escapes in distinct jets; if the vessel is distant from the heart the flow is uniform and continuous; and if we open finally one of the small arterial ramifications which form the capillary net-work, the blood spreads itself uniformly in a sheet. How comes it that an alternating pressure, like that of the contraction of the ventricle, can in the end produce a continuous efflux? By what process of nature is this remarkable result accomplished? By the elasticity of the walls of the arterial vessels. If I be not mistaken, I was myself the first to insist on this wholly mechanical explanation, the only one which renders an exact account of this singular phenomenon. In effect, the jet of blood which the ventricle projects into the aorta makes itself felt in all the arteries whose walls it distends; the impulsion ceases, but the sanguineous current is not on that account interrupted, for these walls contract upon themselves in virtue of their elastic property, and exert on the contained liquid an energetic compression." (*Leçons sur les phénomenes physiques de la vie,* t. I, p 171–183.)

bodies have not at their disposal the means of sustaining the zeal of inquirers who have established themselves in the right method of procedure; the young Magendie, for instance, who gives to physiological labors the invariable basis of experiment, would deserve to be encouraged." "Are not your own exhortations the most powerful of encouragements?" "They are not sufficient," replied Laplace; "for those who aspire to reach our academies there should be graduated approaches, which would consist in competitions and prizes." "Yours, then," rejoined the unostentatious benefactor, "be all the honor of the conception: dispose of whatever you think necessary; I desire for myself nothing more than to have satisfied one of your enlightened wishes." The prize of experimental physiology was thereupon established, and on Magendie was conferred the first distinction.*

His reputation attracted numerous auditors to the courses which he had long before opened. In experimenting before them, he initiated them in his researches, brought into play their sagacious curiosity, inspired them with the pleasure of refutation. Truth was what he sought with all his energy; to attain it, no surer means, he held, could be found than to borrow nothing from either ancients or moderns. Starting with the principle which he had established for himself, that in science everything was to be reconstructed, he called everything in question, allowing nothing to stand but what was capable of resisting his incessant controversy.

The novelty of this instruction had its charms for the young; but the professor was reproached for the sacrifices to which it condemned him. He held, however, the transient sufferings of the victim a consideration of little moment in view of the high and useful aim, the welfare of his species. As the practitioner, to save a life, hesitates not to provoke a pain, so M. Magendie's persistence in experimenting on the living animal was no proof of want of sensibility. Let us remember that he shared with his dog when he had but five sous to live on, and judge whether it is likely that he would be needlessly cruel.

In 1816 M. Magendie had published an *Elementary Compend of Physiology.*† The science which, a century before, was only accessible to a few savants, is, in this work, condensed and definitely circumscribed, so as to present to youth a lucid and practical manual. In 1820 he founded a *Journal of Physiology*, which, in a duration of ten years, collected the labors of diligent inquirers, promoted the progress of the science, and extended the reputation of its editor.

About this time, impelled by an insatiable curiosity of observing and of knowing all that was being done in analogy with his own researches, he crossed the channel. His presence in London was scarcely known before he was called upon, at the solicitation of the principal physiologists whom the country of Harvey then numbered, to repeat, with a skill which seemed to pertain to prodigy, the experiments by which he was enabled at will to suspend, accelerate, or reduce the forces of life.‡ So ardent was the admiration excited that it

* In 1814, M. Magendie, who had been twice before designated by the conscription, was again summoned. On this occasion the academy interposed; in view of his prospective services, it requested his exemption, which was accorded by a special decree. "You owe this favor," wrote the minister, "to the success which you have achieved in the sciences."

† The date of the first edition was 1816; of the fourth and last, 1836.

‡ "I was in the laboratory of Wollaston, engaged in repeating before this illustrious observer some of my experiments on the nervous system. He was especially desirous of verifying by his own inspection the effects of the section of the fifth pair. I opened the cavernous sinus or carotid artery, and a profuse hemorrhage ensued around the brain. The animal was at once seized with convulsive trembling, and fell as if dead. Wollaston considered it to be so, and requested me to repeat the experiment on another. I would prefer to recall this one to life, said I, and, what is more, cause it to run as far as you please. He thought that I was jesting. I then cut a certain point of the brain, and the animal darted off like an arrow. Wollaston, of a spirit as judicious as exact, and habituated to reflection, was vividly struck with the certainty and novelty of these results." (*Leçons sur le systeme nerveux*, t. I, p. 198.)

gave rise to a counter-party. A prudish memorial was even laid before the House of Commons, denunciatory of "this stranger whose offensive temerity had broken through all the hümanitarian barriers established by English zoophilism."

The institute had been the supreme ambition of M. Magendie; to reach it he had combined both efforts and studies. It was a distinction which suited his instincts of independence, of recognized superiority, of noble disinterestedness; not so the free test of the ballot; this suited him very little. The decisive day having arrived, two of his pupils were commissioned to bring him news of the result, which he awaited at home with lively anxiety. At length one of his confidants hurries to him with tidings of his success. "This," he exclaimed, "repays all my labor; my object in life is attained!"

From this epoch dates the special application of M. Magendie to the study of the nervous system. "The inner man," said Van Helmont, "is all nerve"—*Homo interior totus nervus.* It is, in effect, through the nervous system that man feels, moves, wills, perceives, that he has intellectual life; all other parts exist only for the service and support of this system.

"If we admire," said, two centuries ago, the great anatomist Stenon,* "the artifice of the fibres in each muscle, how much more must we admire it in the brain, where these fibres, included in so small a space, perform each its operation without confusion and without disorder!" Stenon was right. What strikes us most in the nervous system is the marvellous *artifice* with which all is there arranged. The fibres, springing from the brain, in their prolongation form the spinal marrow; in detaching themselves, by distinct fascicles, from each side of the trunk, they furnish successively all the nerves of the body; twenty times they become associated, as often separated; some run parallel with, others cross one another; all is united and all distinct; everything touches, and nothing is confounded; each fibre preserves its special play, its proper function; nowhere is there disorder; and from the most intimate connection of the constituent elements of the organ results the free exercise of all the faculties. How abysmal a depth! and, in man himself, what subject more worthy of the meditations of man!

Hence the first and perhaps the most ingenious, the most inventive of physiologists, Galen, seems to have concentrated for this great study all that he possessed of penetration, of ardor, of critical discrimination. He blames Hippocrates for having confounded the *nerves* with the tendons; Aristotle for having taken the *heart* as the origin of the *nerves*, an error which he seems to impute to him as a crime, (*crimini dandum.*) Aristotle had founded his opinion of the *heart*, thus assumed to be the origin of the *nerves*, on the appearance of certain parts. "But how long, oh, excellent Aristotle," exclaims Galen, "has it been the rule to judge of the parts by their appearance? It is by their uses, their properties, their functions, and by these alone, that we must judge of them." Galen was the first to distinguish clearly the nerves from the tendons; the first to see the true origin of the nerves; it was he who first proposed the problem of the separate loss of *sensation* and of *movement*:† a fundamental problem which it was reserved for our own age to propound anew and to solve.

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* I quote the entire passage: "We are sure that wherever there are fibres in the body, they maintain a certain correspondence with one another. If the substance of the brain be everywhere fibrous, as in effect it appears to be in several parts, it must needs be admitted that the disposition of these fibres is arranged with great art, since the diversity of all our sensations and all our movements depends on them. We admire the artifice of the fibres in each muscle; how much more must we admire it in the brain, where these fibres, enclosed in so small a space, perform each its own operation without confusion and without disorder!" *Discours sur l'Anatomie du Cerveau, lu par M. Stenon dans une assemblée chez M. Thévenot, en* 1668.

† *Ubi vero pars aliqua convulsa est, &c.* "When any part is convulsed, it must needs be that the nerve appropriated for its movement, or the muscle, is affected. Hence if we found our practice on the anatomy of the nerves, proceeding to the several parts, we shall more successfully treat their loss, whether of sense or of motion. These, however, were not dis-

In 1811 an English physiologist, a man of profound sagacity, after having long meditated on this vast network of nerves, whose complication seems *inextricable*, published a pamphlet of a few pages, in which, according to his own expression, *he submitted to his friends* his views and ideas.

The principle with which everything in this tractate connects itself is, that whenever two or more nerves proceed to the same part, it is not for the purpose of repeating, of reduplicating therein the same action, but to indue it respectively with a different function. For example, two nerves proceed to the face; the one is for the voluntary movement, the other for the respiratory movement. The tongue receives three nerves: one for the movement of deglutition, another for the voluntary movement, a third for the sense of taste. Each nerve, therefore, has its determinate office, its precise mission. But it remained to clear up a point still more difficult. The greater part of the nerves, all those, for instance, of the spinal marrow, are at the same time motive and sensitive. Now, how can this be? How shall two functions coexist in a single organ? It was at this point that, by an inspiration of genius, C. Bell conceived the idea of the duality of each nerve, each being composed of two, the one for *sensation*, the other for *movement*; in this we have the explanation why each nerve has two *roots*, and in each *root*, taken separately, is seen the primitive, the simple, the distinct nerve. M. Bell, therefore, submits each root to experiment; he obtains, as regards one of the two, a clear and precise result, and from the property manifested by this one he infers the property which resides in the other.

This experiment, an ever-memorable though incomplete one, was the first step. Ten years later, M. Magendie read to the Academy a memoir, in which he announced that having divided the *anterior root* of a nerve he had abolished only *movement*, and that having divided the *posterior root* he had abolished only *sensation*. In this he had simply completed the experiment of M. Bell, but this completion was in itself a new and important advance; for here nothing was left to deduction, but all was positive; the experimental demonstration was perfect. It seems that, in England, all the import of the discovery to which the name of Bell had been first attached, was only comprehended when it became known there how much admiration was excited on this side of the channel by the subtle investigations of Magendie.

The impression produced by the rare sagacity of our dexterous experimentalist was still in the ascendant when he himself, by one of those abrupt changes to which he was but too subject, gave a complete denial to his previous results. On this occasion the vacillation was not without excuse; in an inquiry so delicate, the further the exploration was carried the more complex became the enigma.

M. Magendie, an experimentalist far more practiced than M. Bell, could not multiply his researches without perceiving that the root recognized as *motive*, that is to say, the *anterior* one, yielded signs of *sensibility*. Whence did it derive that sensibility? Unsparing towards himself, fully as much as towards others, M. Magendie passed twenty years of his life in seeking the solution of this new problem; and we may say to-day, to the honor of his memory, and before this Academy which so greatly applauded it, that he found that solution.

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criminated by Herophilus and Eudemus, the first, after Hippocrates, who accurately described the anatomy of the nerves, and who thus have left no slight occasion of perplexity to physicians in regard to the fact that, through palsy of the nerves, sometimes the sense only, sometimes the motion, and sometimes both together, are abolished. Wherefore, when motion is lost, this we mostly call palsy *(resolutio)* of the nerves; but where the sense of certain parts has perished, we are accustomed to say rather that such part is devoid of sensation, although there are some who call this affection a palsy of the sense. We leave to each, however, to apply these names at his pleasure. Physicians lose sight of the fact that the nerves distributed through the skin of the hand, and by which the faculty of sensation is conveyed to it, have their appropriate roots, and that there are other roots of the nerves by which the muscles are put in motion." Galen, *De Loc. Affect. p.* 21, *apud Juntas*, Venice 1597

The sensibility of the *anterior* root, of the *motive* root, pertains not to that root, is not its own, is in fact but a derivative from the *posterior* root. This *derived* or *reversionary* sensibility, which M. Magendie afterward denominated *recurrent* sensibility, constitutes his discovery. And by this discovery, so delicate and so difficult to achieve, he has restored to the principle of *exclusiveness of action* all its purity, for he has shown that, considered apart and in itself, the *anterior* root is solely *motive*, as the *posterior* root is solely *sensitive*. As long as M. Magendie lived these striking results were contested; nay, they are still contested, but they are not the less incontestable. The recognition which contemporaries evade, is compensated by the admiration of posterity.

M. Magendie was admitted to the Institute in 1821. Having had sufficient address to secure his acceptance without dissembling his original humor, without subjecting his ineradicable spirit of sarcasm to constraint, he was quite sure to discard neither the one nor the other when he had attained his object. His colleagues—the practitioners—had admitted him into their Academy from its foundation, inasmuch as till then he had shown himself a sufficiently respectful disciple of Hippocrates, although he believed in nothing, and in medicine less than anything else. With a future conspiring to that end, convictions might have reached him; but a future like his, embellished by the sympathies which his scientific success assured him, could but conduct this intractable associate to open revolt.

In our own ranks he fulfilled conscientiously the duties imposed on him; in the labor of the committees he showed himself as active as he was judicious and clear-sighted; several reports of his were real studies. But he held in reserve, at the service of certain of his privileges, those abrupt sallies whose suddenness disconcerted prevision and set at naught all academic tradition. He never insinuated that an opinion was erroneous or a fact misstated; he plainly said so. When a professional colleague—a physician—aspired to the Institute, his suffrage was, of course, to be solicited; but unless the impulses of affection tended that way, he defended the position as one who did not believe in the necessity of sharing it, and opposed to the foibles of the candidate a frankness which left nothing to be guessed at. When substantial titles to success appealed to his probity, he would content himself with saying, as he turned away: "Well, well! you shall have my voice, but not my hand."

A still more serious danger beset our academician: having dedicated himself without reserve to physiology, he had arrogated that department of science to himself as a domain which belonged to him in his own right. No point of it could be touched upon without arousing his jealousy: either he had treated of it, or he held it mentally in reserve with a view to inquiring what new aspect could be given to it by experiment. In this state of things an aspirant who stepped from the ranks became an enemy. Transported beyond all self-possession, M Magendie would on such occasions reappear before us as the man who had been reared in a complete exercise of the privileges of democracy, until reflection and the intrinsic probity of his character admonished him how much, by such injustice, he had descended below his proper level.

Sarcastic, self-confident, and intellectual, there was here more than enough to insure a good position in the world. Accordingly a select body of patients awaited him without his seeking; but it was necessary that these patients should renounce the comfort of being consoled for imaginary complaints; that, conformed to his humor, they should accept plain truths, and submit to reprimands and caprices. In spite of all, as Sganarelle expresses it, "they were so bewitched with an idea of the man's skill," that, on the noise of the reputation which they created for him, he was sometimes approached by those unhesitating advocates of medical infallibility who venerate the yoke and would think all was lost if their faith did not increase in proportion to the obscurity of the doctrine. In laying before such persons the scanty inventory of his own creed, M. Magendie would throw their frankness into ludicrous perplexity, and finish by assuring them that all they needed was to be cured of their tendency to credulity.

To the ardor of young practitioners vaunting the success of their prescriptions he opposed his own experience, saying to them with pleasant irony: "It is plainly to be seen that you have never tried the plan of doing nothing." If the extreme simplicity of this mode of treatment elicited not unreasonable objections, "Be assured," he would add, "that, for the most part, when disturbance manifests itself, we cannot discover the causes; we can, at most, only verify the effects. Our usefulness, in presence of the efforts of nature, which, in general, tends to the normal condition, consists in not interrupting them; it is only now and then that we can aspire to be sufficiently skilful to aid them."

On a certain occasion, when quitting a young person whose condition presented alarming symptoms, he said: "Let him do just what he pleases; that is all I prescribe." Usually sparing of his time and visits, in behalf of this child he lavishes both, but adds nothing to his medication. On the evening of the third day, all at once his brow clears up, and, taking the invalid by the ear, he exclaims: "Little rogue, you have not allowed me a moment's rest; go now and walk about." The delighted father asks: "What, then, was the matter with the child?" "What was the matter? upon my word I don't know; neither I, nor the whole faculty, if they were sincere, could tell you; what is certain is, that everything has returned to its normal state." And with this he disappeared.

The greater portion of the medical career of M. Magendie was devoted to the unfortunate; he preferred the hospital to other practice. Twenty years of service as physician in the wards showed us this rigid and wayward man, gentle and patient on approaching the bedside of the indigent; the earnest thinker, the inflexible censor listening to and consoling the poor women of the Salpétriere; receiving with emotion the humble testimonial which they offered him; and not quitting that establishment for the Hôtel-Dieu, in 1830, without stipulating for himself the right of continuing to extend to the former his disinterested benefactions.

A chair of medicine having become vacant at the College of France, the minister, desirous of reconciling public opinion with the tendencies of the government, thought proper to require some concessions from the rigid candidate whom that opinion indicated. Conducted by a friend to M. Frayssinous, and surprised at having allowed himself to be thus entrapped, the strange candidate maintained so stately and formal a reserve, that the eloquent minister—a great master in point of *conferences*—saw this one come to an end without having produced any relaxation of the jealous rigidity of his interlocutor. On retiring, our humorist shook his head and remarked: *He is not yet strong enough for me.* M. Recamier received the nomination.

Three years later, in 1830, M. Magendie was put in possession of the chair. It was then that he gave free course to his ardor for experimental art, and it is surprising to what an extent he lavished his experiments. Yet for this who can justly blame him? It was from these extemporized experiments that often sprung the boldest and most happy results. He had the gift of seizing phenomena in passing, and, as it were, on the wing. Endowed with keen curiosity, prompt and impulsive by nature, improvisation was of the essence of his genius.

The success of hap-hazard experiments, however, is not art. Art demands, first of all, combination, reflection. It is not the experiment which investigates: it is the mind which investigates through the experiment; it is the mind which discovers, which invents the means by which the discovery is made. Buffon has said with a profound sense: "The best crucible is the understanding."

The lessons of M. Magendie at the College of France have been collected in two separate works: one on the *Nervous System;* the other on the *Physical Phenomena of Life.* The latter presents the professor under a new aspect.

Bichat had exaggerated the part played by the *vital properties;* M. Magendie exaggerates, in turn, the part played by the *physical properties.** Their opposition has rendered both more useful; it is only by multiplying special points of view that we arrive at a view of the whole.

Through a profound alliance, which forms the *nodus* of life, everything in our organism concurs: the *physical forces* like the *vital.* And even these two orders of forces do not represent the man in his completeness; above the *vital* forces preside the *psychical* or *intellectual* forces. *Sensibility* is not a *physical* force, nor is *thought* a *vital* force.

As there is a superior philosophy, so there are commodious philosophies. I call commodious—and without allusion to M. Magendie, who belonged to none†—all philosophy which breaks its subject to pieces and takes a fragment for the whole. The superior and true philosophy embraces the complex being, and, in that complex being, arrives at unity, not by the arbitrary exclusion of such or such parts, but by a clear and distinct view of the precise function of each.

M. Magendie would, for his part, have said with Pascal, though with less profound meaning: "We do not think the whole of philosophy worth an hour's trouble." His vocation did not lie therein, but was that of a great experimentalist. Having received from Bichat the torch of experimental art, he bore it with a steady hand for forty years; indefatigable in labor, bold in exploration, making no account of any sect, neither of *materialism* nor of *vitalism*, incapable of the spirit of sectarism, he sought truth with entire independence. This emancipated reason of his was his distinctive stamp, securing him the esteem

* One of the points, of this kind, in which he has indulged the greatest exaggeration, is that which regards *absorption;* this he reduces to *imbibition;* but even here the exaggeration consists rather in the expressions than in the fundamental idea. He calls absorption a ***wholly physical phenomenon***, and in this he is mistaken; there is but one thing purely ***physical*** about it, namely, the *imbibition.*

He very properly says: "Now, we all know that every substance, whether acid or alkaline, wholesome or deleterious, is absorbed as soon as it is placed in contact with our tissues. There is herein nothing but a phenomenon of imbibition, and all that has been said of the intelligence of the pores is only a romance at present out of date." What he adds touching the different functions of the ***veins*** and ***lymphatic vessels*** in ***absorption*** is also very just: "No doubt the lymphatic vessels can absorb, since their walls, like those of the veins, are porous and susceptible of being imbibed with the liquids with which they happen to be in contact. Recall now the division which we have established in the mechanism of absorption. We there see two phenomena entirely distinct: on one hand, a local imbibition of the liquid; on the other, a transfer of the liquid imbibed into the current of the circulation. The former property is common to the two orders of vessels; but, as regards the second, do we find united in each the conditions necessary for its being effected? I have satisfied myself that, in most circumstances, the lymphatic vessels are not filled with liquid, nor traversed by an interior current; hence, most frequently they are not, they cannot be agents of absorption. The veins, on the contrary, destined to convey without cessation the blood from the periphery to the centre, must, by just right, be considered as the habitual channels by which the liquids are absorbed." *(Leçons sur les phenomen. physig. de la vie,* t. I.)

† It is very true that he would willingly have explained everything by ***physical forces***, if he could; but he clearly distinguished in the living organism the ***physical*** from the ***vital.*** And who could confound them? The art is in separating them. "Begin always," he says, "by analyzing the phenomena, by isolating what is physical from what is vital." *(Leçons sur le syst. nerv.,* I, p. 4.) "I distinguish, in vitality, two great classes of phenomena: the one comprises ***physical phenomena***, the other ***vital phenomena.***" *(Leç. sur les phen. phys de la vie,* t. II, p. 14.) "Far be it from me," he further says, "to exaggerate the importance of physical explanations. Thus, why is it that, under the influence of a moral emotion, more or less vivid, we see the face redden or grow pale? There is here something peculiar, something which belongs not to the domain of physics." *(Ibid.,* t. I, p. 202.) Elsewhere he says: "To seek to explain a physical phenomenon by vital laws, only because that phenomenon occurs in a living body, is an idea quite as irrational as to speak of vitality in reference to an inorganic body." *(Leç. sur le syst. nerv.,* I, p. 3.) Finally, his conclusion thereupon is that "Physical laws lose nothing of their authority for being exercised in organized bodies. Observers only have been wanting to follow them into this living world, this microcosm of the ancients. Each function, each organ would easily furnish us the proof of it; and is it not, of itself, exhibited in the senses, the movements, the voice, the circulation of the blood, &c.?" *(Leç. sur les phen. phys. de la vie,* t. I, p. 310.)

of thinkers, who know how little intellectual partisanship is worth, and the sympathy of the young, who delight to owe their convictions only to themselves.

"Men pass," says the philosophic Bacon, "and knowledge is increased;" *multi pertransibunt et augebitur scientia.* Haller, Bichat, Magendie, had scarcely constituted physiology, properly so called, the human physiology, when a much wider horizon was disclosed. Thanks to comparative anatomy, that antique study restored to our age, the view of the physiologist has been enabled to embrace the total assemblage of living beings. To observation, to experiment, it has become possible to add the art, not less subtle and prolific, of uninterrupted comparisons; comparisons have brought to view relations; relations have guided us to laws. "Laws," says Montesquieu, "are the necessary relations of things."

A new philosophic spirit, born of science and superior to science itself, propounds all the great questions of life, no longer studied only in each particular being, but considered as a constituent element of the universe: its origin, its antiquity, its gradations, its successive variations; it is a spirit which shrinks not from disentangling, from following up the profound relations which connect the history of life with the history of the globe; it sees the globe and life developed by a common evolution; concerted progress reveals the unity of design; and to recall an eloquent phrase of the Roman orator, "it almost lays hold upon Him who governs and controls everything—*ipsum cuncta moderantem et regentem penè prehenderit.*"

Towards the commencement of 1832 the ordinary course of M. Magendie's life was turned aside. Vague and ill-boding reports were spread abroad, nor was the phantom, though distant at first, slow in disengaging itself from its shadows and presenting to our excited imaginations the assurance of a coming pestilence. Then, when under the pressure of fear personal susceptibilities had become acute even to cruelty, the noblest of purposes formed itself in the mind of our colleague. Coming one day to our ordinary session he said to us: "I am a physician, and that vocation summons me to the focus of the evil. I go to Sunderland; hoping that I shall be able to bring you thence, by studying the cholera in the place of its appearance, some useful indications! Invest me, by delegation from your body, with more authority." Everywhere he was received with interest and respect. Having reached the seaport, the centre of contagion, he is informed that in a population of fishermen scattered along the neighboring coast had occurred the outbreak of the disease. Proceeding directly thither he finds collections of individuals exposed under miserable huts to all the rigors of humidity, uncleanness, and vice, living, sleeping, eating between the dead and the dying, and with instincts so brutal as to forbid the hope of any helpful intervention.*

Ordinary contagion was not an admissible theory. When anxiously asked, on his return, "What is it? what shall we do?" the only answer which could be drawn from him in his dejection was; "I do not clearly know."

Paris was still throbbing under the dread presentiment when the pestilence, clearing the interval at a single bound, burst upon us like an explosion. Who does not remember that at the outbreak, the extreme violence of which had till then been unexampled on our continent, a man stricken was a man dead? Summoned on the first attack, from that moment M. Magendie was no longer at his own disposal; it was towards the hospital that he directed his steps. "The rich," he said, "will not want for physicians;" and traversing the ranks of a deluded and infuriated crowd from which issued the cry: "Vengeance, death to the physicians, death to the poisoners!" he ascended the steps of the Hôtel Dieu, renewing his self-forgetfulness on a thousand occasions in behalf of

* He was accompanied in this expedition by M. Natalis Guillot, at present one of the most distinguished members of our Faculty.

wretches reduced to the state of an inert mass, feeling no pain but that of inspiring distrust, and finding his recompense in the liberality with which his purse was emptied for the succor of such as were saved and restored to their families.

The duration, the rigor of the pestilence found the forces of M. Magendie at their own level. Calm having been somewhat re-established, the cross of the legion of honor was sent to him. "I think it very well awarded," he remarked.

Having satisfied his conscience, he returned to his labors, occupied himself anew with inquiries respecting the nervous system, resumed his instruction; but his lectures on the cholera, and still more his bearing, evinced that this man, who was accused of insensibility, had not passed with impunity through an interval of heart-rending emotions. Impelled by a secret preoccupation, he applied himself to an investigation of the agents under the influence of which epidemic maladies may be generated.

Being shut up one day in his laboratory, he was giving his whole attention to an experiment, when lifting his eyes they met those of a sedate and portly personage who, in abrupt tones, asked to speak with Magendie. From the first glance at the broad-brimmed hat scrupulously kept on the head, the short breeches, the peculiar cut of the vesture, the experimenter comprehended the quality of his visitor. "I had heard speak of thee," said the Quaker, "and was not misinformed; I come to say to thee that thou shouldst desist from these experiments. Who has permitted thee to dispose of the life of these animals?" "Your countryman Harvey," replied M. Magendie, "would not have discovered the circulation of the blood if he could not have sacrificed the deer in the park of Charles I. Here science contends against the maladies inflicted on humanity, as elsewhere war contends against the incursions of barbarism. Perhaps," he added with complaisant deference, "you would condemn the chase." "Certainly," replied the inflexible Quaker; "I condemn the chase, I condemn war, and experiments upon animals; man therein assumes rights which do not belong to him: I mean to prove it, and I shall travel until I have made those wrongs disappear from the world." Probably this reformer is still on his travels.

A sojourn in the country came very opportunely to diversify our savant's course of life. Under the influence of a smoother existence this heretofore unmanageable nature was led to unbend itself. He had married; he saw that he was understood even in the foibles of his character, and, like a man of sense, took the part of laughing at them. "I agree," he said resignedly, "that I am nothing but a real bull-dog." Happy days were ushered in by this frankness; friendly neighbors came around him to applaud his experiments on vegetation, on agricultural ameliorations, attempts which, he said, might enrich science and the country, but whose immediate result was to diminish his own fortune. To fire-side happiness, to the charms of a lively society, he joined the pleasure of doing good. For the suffering in his neighborhood, dispensing with a part of his medical principles, he had established in his house a small, but very small pharmacy. Of all remedies that which he oftenest put in practice was to pay the invalid for the consultation which the invalid received.

No trace of the republican recklessness and rudeness remained, except perhaps in the malign pleasure which from time to time M. Magendie still allowed himself, of demolishing all our governments; and that with so hearty a denunciation that a judicious friend once said to him: "If to-day a government were created to your mind, in six weeks you would find it the most detestable in the world." "Very possibly," he replied. "It is certain, however, that not one of them can boast of having received a solicitation from me."

It was, in effect, without solicitation that in 1848, on the establishment of the consultative commission of public hygiene, he was nominated its president. The firmness with which he interdicted the admission of all charlatanism into this institution, the clearness and justness of his views, rendered his influence there of great utility; there, as everywhere else, he commanded general esteem, and more than elsewhere showed himself accessible to friendship. He had eight years before been called by the ministry of war to the presidency of the commission of hippiatric hygiene. Here the information which he conveyed became the source of ameliorations of which our cavalry still enjoys the benefit. In 1851 the cross of commander of the legion of honor was sent to him; at this, however, he took umbrage, fearing that the services which he had rendered would thus lose the merit of disinterestedness.

A grave alteration which manifested itself in the health of M. Magendie revealed that one of the organs essential to life was attacked. Thenceforth he came but rarely to our meetings; thenceforth there survived for us, in place of the capricious and difficult colleague, only the man of uncontested eminence. Nothing was remembered but that undeviating rectitude which gave so much strength to the personal attachments of which he was the object; and when, still later, it was known that the malady had become more serious, devoted disciples, grateful pupils, relations, friends, colleagues felt a common alarm; asperities were forgotten. It was said by Fontenelle of an academician of his time: "Those who might have had something to complain of in regard to his bluntness, all went to see him; he was touched by the expression of sentiments which he had merited more than he had attracted." M. Magendie also received with cordiality and acknowledgment the expression of the sentiments he had merited. "Know," said he to his old competitors, "that my asperity increased in proportion to the worth which I recognized in those towards whom I exercised it." So ingenious is self-love that each found in this strange mode of appreciation wherewith to be satisfied.

It was impossible to enter the room of the invalid without being struck with the grief which impressed the countenance of a servant whom thirty years' contact had rendered the grotesque but faithful copy of his master. Watching anxiously by his pillow this attendant heard him announce, with calmness, the hour of separation. "Courage, my good master," he exclaimed, carried away by a pious attachment, "courage, I beseech you; we shall still grumble on together."

The moral force which this upright man had so studiously cultivated was respected by the malady. His sufferings did not distract him; he studied them as phenomena. "You see me here completing my experiments," were his parting words to a colleague; "never has the science to which I have devoted all my strength appeared to me environed with more grandeur; the springs of life, so marvellously combined, are quickened in order to make of each of us an instrument of passage, which in perishing is regenerated. In my restricted course may I but have succeeded in planting some way-marks along the route which leads to Truth, the sole power to which I have subordinated my reason!"

M. Magendie died the 7th of October, 1855

APPENDIX I.

ON THE DISCOVERY OF THE DISTINCT FUNCTIONS OF THE ROOTS OF THE NERVES.

I.—M. Bell published, in 1811, a pamphlet, under the following title: *Idea of a New Anatomy of the Brain, submitted for the observations of his friends.* In this he first announces his ingenious idea that each nerve, which is both *motive* and *sensitive,* is a double nerve, and secondly describes his experiment. In regard to his idea of *double* nerves, or, as the author calls them, *mixed* nerves, he says: "I have to offer reasons for believing that * * the nerves which we trace in the body are not single nerves possessing various powers, but bundles of different nerves, whose filaments are united for the convenience of distribution, but which are distinct in office as they are in their origin from the brain." (Bell: *The Nervous System of the Human Body,* &c.; 3d edition, Edinburgh, 1836, p. 442.) He adds: "Considering that the spinal nerves have a double root, and being of opinion that the properties of the nerves are derived from their connections with the parts of the brain, I thought that I had an opportunity of putting my opinion to the test of experiment, and of proving at the same time that nerves of different endowments were in the same cord and held together by the same sheath." (*Ibid,* p. 443.) In stating his experiment, M. Bell says: "On laying bare the roots of the spinal nerves, I found that I could cut across the posterior fasciculus of nerves which took its origin from the posterior portion of the spinal marrow without convulsing the muscles of the back; but that on touching the anterior fasciculus with the point of the knife, the muscles of the back were immediately convulsed." (*Ibid,* p. 443.)

We thus see what was done by Bell: first, it was he who first conceived that each nerve might be *double,* or composed of two; secondly, it was he who, before all other physiologists, directed experiment to the *roots* of the nerves; thirdly, from his experiment, however incomplete, he arrived at the conclusion of a distinct function for each root. All this was accomplished by M. Bell ten years before the researches of M. Magendie.

II.—The titles of Magendie to the important discovery are distributable into two series: by the first he completed the labors of Bell; by the second he discovered the *recurrent sensibility.* I shall begin by reproducing entire the first "Note" of M. Magendie, and it will be seen how felicitous was his insight on this first attempt. His first introspection was in fact always the surest.

Experiments on the functions of the roots of the rachidian nerves, by M. Magendie; 22d July, 1822: "I had long desired to make an experiment in cutting, in some animal, the posterior roots of the nerves which spring from the spinal marrow. I had several times attempted it without succeeding, on account of the difficulty of opening the vertebral canal without injuring the medulla, and consequently destroying or at least sorely wounding the animal. Last month, a litter of eight pups, six weeks old, was brought to my laboratory, and these seemed well suited for a renewal of the attempt to open the vertebral canal. In effect, I was able, by the use of a very sharp scalpel, and so to say at a single stroke, to lay bare the posterior half of the spinal marrow surrounded by its envelopes. To have this organ almost naked, it only remained for me to cut the *dura-mater* which encloses it, and this was done with facility; I had then before my eyes the posterior roots of the lumbar and the sacral pairs, and by raising them successively with the blades of the small scissors I could cut them on one side, while the marrow remained untouched. I knew not what would be the result of this attempt; but I reunited the wound by a suture of the skin and observed the animal. I at first thought the member corresponding to the nerves entirely paralyzed; it was insensible to punctures and to the strongest pressure; it seemed also to be immovable. But presently, to my great surprise, I saw it move very apparently, although the sensibility was all the time wholly extinct. A second, a third experiment gave me exactly the same result; I began to regard it as probable that the posterior roots of the rachidian nerves might well have different functions from the anterior roots, and that they were more particularly destined for sensibility.

"It naturally occurred to my mind to cut the anterior roots, leaving the posterior ones undisturbed; but such an undertaking was more easy to conceive than to execute; how could it be possible to uncover the anterior part of the medulla without injuring the posterior roots? I confess that at first the thing appeared to me impracticable; yet for two days I never ceased thinking about it, and finally decided to attempt to pass before the posterior roots a sort of knife for cataract, whose very narrow blade would allow of cutting the roots by pressing them with the edge of the instrument on the posterior face of the body of the vertebræ; but I was obliged to renounce this expedient on account of the large veins which the canal contains on that side, and which were opened at every movement in advance. In making these essays, I perceived that by pulling upon the vertebral *dura mater* I could discern the anterior roots united in fascicles, just when they are about to penetrate that membrane. I wanted nothing more, and in a few moments I had cut all the pairs which I wished to divide. As in the preceding experiments, I made the section only on one side, in order to have a term of comparison. It may readily be conceived with what curiosity I followed the effects of this section, nor were they ambiguous; the member was completely motionless and lax, while it preserved a sensibility not at all equivocal. Finally, to neglect nothing, I have cut at once the anterior and posterior roots; there was then an absolute loss both of sensibility and movement.

"I have repeated and varied these experiments on several species of animals; the results just announced were confirmed in the most complete manner, whether as regards the anterior or posterior members. I am prosecuting the researches, and will give a more detailed account in the coming number; it suffices that at present I am enabled to assert that the anterior and posterior roots of the nerves which issue from the spinal marrow have different functions; that the posterior appear more particularly destined for sensibility, while the anterior seem more specially connected with movement."

Having seen the first note of M. Magendie, let us pass to the second; the retrograde step cannot fail to occasion surprise:

Note on the functions of the roots of the nerves which spring from the spinal marrow; October 22, 1822: "The facts which I announced in the preceding number are too important not to have led me to seek to elucidate them by new researches. I wished first to ascertain if the anterior or the posterior roots of the spinal nerves might not be cut without opening the great canal of the vertebral *dura mater*; for, by exposing the spinal marrow to the air and to a cold temperature, the nervous action is sensibly enfeebled, and consequently the results sought for are obtained in a manner but little apparent.

"The anatomical disposition of the parts rendered this not impossible, for each fasciculus of a spinal root proceeds for some time in a particular canal before uniting and being confounded with the other fasciculus. In effect, I have found that by means of scissors blunted at the point, enough of the films and lateral parts of the vertebra might be removed to expose to view the ganglion of each lumbar pair; and then with a small stylet the canal containing the posterior roots may be separated without any insurmountable difficulty, and there is no further impediment to the accomplishment of the section. This mode of making the experiment has yielded me the same results with those I had already observed; but as the experiment is much longer and more laborious than by following the process in which the great canals of the spinal *dura mater* is opened, I do not think that this method of making the experiment should be pursued in preference to the former.

"I desired afterwards to submit to a particular proof the results of which I have previously spoken. Every one knows that the *nux vomica* causes in man and animals general and very violent convulsions. It was a matter of curiosity to know whether these convulsions would still take place in a member whose nerves of motion had been cut, and whether they would show themselves as strong as usual when the section of the nerves of sensation had been made. The result has proved altogether accordant with those preceding that is to say, that in an animal in which the posterior roots were cut, the tetanus was completed and as intense as if the spinal roots had been all intact; on the contrary, in an animal in which I had cut the nerves of movement of one of the posterior members, that member has remained supple and motionless at the moment when, under the influence of the poison, all the other muscles of the body underwent tetanic convulsions of the most decided kind.

"By directly irritating the nerves of sensation or the posterior spinal roots, would contractions be produced? Would direct irritation of the nerves of motion excite pain? Such are the questions which I proposed to myself, and which experiment alone could resolve.

"I commenced by examining under this aspect the posterior roots or nerves of sensation. What I have observed is this: on pinching, pulling, pricking these roots, the animal evinces pain, but not to be compared, for intensity, with that which is developed if the spinal marrow be touched, even lightly, at the place whence these roots spring. Nearly every time that the posterior roots are thus excited, contractions are produced in the muscles where the nervers are distributed; these contractions are, however, but slightly marked, and vastly more feeble than when the medulla itself is touched; when at the same time a fasciculus of the posterior roots is cut, a movement of the whole is produced in the member to which the fasciculus proceeds.

"The same trials were made upon the anterior fasciculi, and analogous results obtained, but in an inverse sense; for the contractions excited by pinching, pricking, &c., are very strong and even convulsive, while the signs of sensibility are scarcely visible. These facts, then, are confirmative of those which I have announced; only, they seem to establish that sensation resides not exclusively in the posterior roots, any more than movement in the anterior. Yet one difficulty may be raised. When, in the experiments which precede, the roots have been cut, they were continuous with the spinal marrow: might not the shock communicated to this be the real origin both of the contractions and of the pain experienced by the animals? To resolve this doubt, I have repeated the experiments after having separated the roots from the medulla, and would say that except in two animals in which I observed contractions when I pinched or pulled the anterior and posterior fasciculi, I perceived in all the other cases no sensible effect from the irritation of the anterior or posterior roots thus separated from the medulla.

"I had still another kind of test to which to submit the spinal roots: this was galvanism. I have, consequently, excited those parts by this means; first while leaving them in their ordinary state, and then by cutting them at their spinal extremity, so as to place them on an isolating body. In these different cases I have obtained contractions with the two kinds of roots, but the contractions which followed the excitation of the anterior roots were, in general, much stronger and more complete than those produced when the electric current was established by the posterior ones. The same phenomena took place whether the zinc or copper pole were applied on the nerve.

"It would now remain to render an account of the researches which I have made in attempting to follow the isolated sensation and movement beyond the roots of the nerves—that is to say, in the spinal marrow; with this I shall by and by be occupied. Before terminating this article I should offer some explanations respecting the novelty of the results which I have announced.

"When I wrote the note contained in the preceding number I believed myself the first to have thought of cutting the root of the spinal nerves; but I was undeceived by a communication from M. Schaw, a young and studious physician, on receiving the last number of my journal. He informs me that M. Ch. Bell had made this section thirteen years before, and had ascertained that the section of the posterior roots did not prevent the movements from continuing. M. Schaw adds that M. Bell had recorded this result in a small pamphlet, printed solely for his friends, not for publication. Having requested of M. Schaw to send me this pamphlet, if possible, in order that I might render full justice to the author, I received it a few days after; its title is, *Idea of a new anatomy of the brain, submitted for the observations of his friends, by Ch. Bell*, F. A. S. E. It is curious, certainly, to remark therein the germ of the recent discoveries of the author on the nervous system, and I transcribe entire the passage indicated by Dr. Schaw:

"'Considering that the spinal nerves have a double root, and being of opinion that the properties of the nerves are derived from their connections with the parts of the brain, I thought that I had an opportunity of putting my opinion to the test of experiment, and of proving at the same time that nerves of different endowments were in the same cord and held together by the same sheath. On laying bare the roots of the spinal nerves I found that I could cut across the posterior fasciculus of nerves which took its origin from the posterior portion of the spinal marrow without convulsing the muscles of the back, but that on touching the anterior fasciculus with the point of the knife the muscles of the back were immediately convulsed.'

"It will be seen by this citation from a work which I could not know, since it was not made public, that M. Bell, guided by his ingenious ideas on the nervous system, very nearly arrived at a discovery of the functions of the spinal roots; yet, the fact that the anterior are destined for motion, while the posterior pertain more particularly to sensation, appears to have escaped him; it is to the having established this fact in a positive manner, therefore, that I must limit my pretensions."

Reply of M. Bell to M. Magendie. For the convenience of the discussion M. Bell attributes this reply to a pupil, but this *pupil* may well have been M. Bell himself: *

"Although the original experiments (those, namely, of M. Bell) have much more value in all their results than those which have been reported by M. Magendie, yet it is noticeable that the latter, when he found himself compelled shortly after to abandon his pretensions to novelty, had the assurance to affirm that the experiments made by himself were the most careful. It was really amusing to see M. Magendie make a parade of superior exactness, when, in the same memoir, instead of persisting in his first decision, he changes in the most essential manner what he had established respecting the functions of the nerves in question. In this second publication on the subject, he gives a relation altogether different from that which he had offered in his previous memoir.

"The results derived by M. Magendie from his first experiments were altogether accordant with those which our author had announced in 1811. Consequently, when an account of them arrived in this country, and it was seen that he gave this discovery as original, measures were taken to make known to the

* I am indebted for the communication of this paper to M. Benjamin Brodie, a highly competent judge in the matter. M: Brodie knew M. Magendie well, and, in writing to me, justly places him not far from Haller and Bichat: from Haller, of whom he had not the erudition; from Bichat, of whom he had not the *expansive* views; but equal to both, and perhaps superior, for industry in the invention of experiments and skill in their execution.

profession to whom it was that the merit of the discovery really pertained. In this there was no difficulty; but M. Magendie having abandoned the opinions set forth in his first memoir, and adopted views diametrically opposite to those of Sir Ch. Bell, there was no further question of priority between them; the question then presented was not to determine who had first seen, but who had seen aright.*

"It is proper to say that if the views given by M. Magendie in his second memoir on the functions of the roots of the spinal nerves had been exact, they would have left us no hope of ameliorating our knowledge respecting the nervous system by reasonings founded on anatomy. The fundamental part of the original doctrine was that the nervous root yielding the sensation is altogether distinct from that which supplies movement;† that these properties are so different in their nature that they cannot both pertain to the same nerve; that when a nerve, in its passage, possesses at once movement and sensation, it is a sign that it is a double nerve, composed at its origin of two roots, one the source of sensation, the other the source of movement; that it is not reasonable to suppose that a nerve can conduct the nervous influence in two opposite directions at once, as would be necessary if a nerve transmitted at the same time the motive power and received the sensations. Now, what M. Magendie presents in his second memoir,‡ as being of such remarkable exactness, tends directly to overthrow all this. Each of the two roots, as he views it, can at the same time transmit movement and sensation; it is not true that the anterior root is exclusively for movement, and the posterior for sensation; the former partakes to a certain degree the function of the latter, and the latter that of the former; all that can be alleged respecting the distinction between the two roots is that the anterior has more influence as a nerve of movement than the posterior, and the posterior more influence as a nerve of sensation than the anterior; that the anterior may confer sensation, and the posterior movement," &c.

Remarks of M. Magendie on the occasion of a note of M. Flourens, March 1, 1847: "I must thank M. Flourens for having given before the academy the explanations which I asked of him in the preceding session. The greater part of the facts which he cites seem to me exact; only he interprets them in a manner which I cannot admit.

"And first, if I have maintained silence in the circumstance recalled by my colleague, no one is authorized to regard it as a sort of abandonment of my right; for the report made to the academy for the prize of physiology of 1841 says, in so many words, that I *had thought right to excuse myself as not competent to be judge and party in questions in which I was so much concerned.* I pass now to the researches of Ch. Bell.

"It was I who first made these researches known in France. I analyzed them in my *Journal de Physiologie.* I even signalized their originality at a public sitting of the Academy of Sciences; and if the discovery that is now sought to be attributed to the English physiologist had been announced, or even indicated in his memoirs, I had certainly not failed to give it the greatest prominence and to point out all its importance. He was himself well satisfied with the reception which I gave to his labors. The proof that he recognized my having rendered him full justice, is the fact that, the 10th of June, 1822, he

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* All this is well enough said and very true, as long as abstraction is made of the ***recurrent sensibility***, or the latter is not known, as was the case with M. Bell.

† Agreed: such is in effect of the fundamental part of the original doctrine—***exclusiveness of action.*** But in truth there is something more; there is the ***mobile sensibility*** of the ***anterior root.*** M. Bell did not see it, but some physiologist would in the end have seen it, and so long as some physiologist, as skilful as M. Magendie, should not have discovered the character of ***reversion***, of ***derivation***, the fine principle of exclusiveness could not have been admissible.

‡ In this second memoir, M. Magendie is mistaken; but how fortunate the mistake (***felix culpa***) which leads us to the discovery of ***recurrent sensibility!*** (See further on what I say under the title of ***Conclusion.***)

wrote in his journal: 'My discoveries have made more impression in France than here; I have received a second letter from Magendie, where he says that if I would send him a short analysis of my experiments, I would receive the medal decreed by the institute.' (*Biography of Sir Charles Bell, British Review*, October, 1846.) Whoever knows the susceptibility and jealous character of Charles Bell will readily agree that he would not have expressed himself in this manner regarding a stranger who had omitted in an appreciative account of his labors the finest of his discoveries.

"On the occasion of my first publications respecting the functions of the roots, M. Schaw wrote to me that Ch. Bell had formerly made some experiments analogous to mine. He sent me a small pamphlet, dated 1811, and which had been communicated only to the author's friends, with a view, as he said, to have their opinion touching his new but still undecided ideas on the anatomy of the brain. I lost no time in transcribing word for word, in my *Journal of Physiology*, the passages which had reference to the roots, and I took care to add that neither myself nor any one else in France had the least suspicion of the existence of this tract. Fortunately for my own researches, it contained nothing which touched upon the capital fact, the distinction, namely, between the two rachidian roots—these as nerves of sensation, those as nerves of movement.

"In effect, Ch. Bell, preoccupied with his ideas on irritability, simply says that in cutting *the posterior root, he had produced no contraction in the muscles, while the muscles were contracted when he touched with the point of the instrument the anterior root.* Such is the experiment as he describes it. We see that not only had he not distinguished the roots into sensitive and motive, but that even the word *sensibility* had not been pronounced. How could it be otherwise, since he operated only on animals recently dead?

"In fine, Ch. Bell had had before me, but without my knowledge, the idea of separately cutting the rachidian roots; he had also had the merit of discovering that the anterior influences the muscular contractility more than the posterior. As regards priority in this, I have, from the first, done him entire justice; but when the question relates to having established that these roots have distinct properties and functions—that the anterior preside over movement, and the posterior over sensation—this discovery I must claim as my own. Ch. Bell had not indicated, nor even caught a glimpse of it, since it in no wise results from the experiment which he relates. It is therefore my own work, and must remain as one of the columns of the monument reared by French physiology since the commencement of the present century."

Of recurrent sensibility.—The discovery of *recurrent sensibility* was the result of the second series of M. Magendie's researches. On this interesting subject, which he alone had yet ventured to attempt, the following is his first Note, (May 20, 1839,) containing the summary of his new experiments on the nervous system:

"The sensitive nerves and the rachidian motors are equally sensible when they are both intact.* If we cut the sensitive nerves, the motor nerves immediately lose their sensibility.† If the motor nerves are cut in the middle, the end which remains attached to the spinal marrow is wholly insensible; the opposite end preserves, on the contrary, an extreme sensibility.‡ In that case, the sensibility proceeds from the circumference to the centre.§ If the sensitive nerves be cut at their middle part, the end which attaches to the marrow is highly sensible; the end which attaches to the ganglion has, on the contrary, lost all sensibility."‖

We come now to the last Note of M. Magendie on the recurrent sensibility, which is also the most important, June 28, 1847:

*An assured fact, if account be taken of the recurrent sensibility. †A real fact and wholly new. ‡Again a fact, and still a new one. §Evidently. ‖ *Comptes rendus de l'Academie des Sciences*, t. viii, p. 76.

"Scientific discoveries have very different destinies. Some, accepted with enthusiasm from their very origin, traverse the world, everywhere exciting admiration. So vast and facile is their success that we are tempted to assign them to that class of brilliant errors which spring up at periods only too frequent to satisfy an imperious necessity of the human mind, that, namely, of being deceived. Other discoveries remain in the obscurity in which they are produced, until some fortunate circumstance occurs to throw light upon them and confer renown on their authors. Others still, which at first enjoy a certain lustre, fail to surmount the ill-will of cotemporaries, and after having striven for recognition, eventually vanish and sink into oblivion, if they do not meet in time with some efficacious succor. A remark of some importance which I had the honor of presenting to the academy in 1839, and which may be regarded, I think, as a discovery, happens unfortunately to fall at this time within the last category.

"I shall not here recount its vicissitudes; I shall only say that it has been twice condemned by committees of the academy, since these committees have conferred a prize on two works in which my discovery is qualified as erroneous. Sustained by such respectable authority, it is the right of every one to think that I have labored under an illusion, (a thing in itself quite possible, for whoever investigates is liable to err.)

"Another ground may have existed for the same conclusion. I was a member of the two committees; I abstained from co-operating with them, as it was proper that I should; but it was in my power to have protested, and yet I have maintained a silence which has been doubtless construed as a tacit acknowledgment of my error, although, in truth, it had a wholly different signification. Probably my honorable colleagues acted on this occasion upon the maxim, *Amicus Plato, sed magis amica veritas.* I am myself a strong partisan of this wise maxim, and I have more than once put it in practice, taking care, however, to prefer to Plato nothing but truth.

"Why did my honorable colleagues not consult me? Why not ask to see my experiments, which I should have been only too ready to repeat before them? I can only explain it by a sentiment of benevolent discretion towards a colleague whose position might appear to them embarrassing. However this may be, to foreclose all discussion, I concede that those who have not seen my experiments might well regard my discovery as seriously compromised; for unfortunately the physiologists who have thought proper to reproduce them have done so in such a manner that it was impossible for them to verify the results. Yet the fact in question is one which I regard as opening a new way to experimental researches on the functions, still so obscure, of the nervous system. There rests, then, on me an obligation to recur to this point of physiology, and to put it in the power of every one to verify the exactness of the results which I made known in 1839.

"Let us first state in what consists the phenomenon which in the year just mentioned I denominated sensibility *en retour*, but which at present I think it preferable to call recurrent sensibility.

"If we lay bare, with suitable precautions, a pair of the rachidian nerves, we shall recognize that the two roots are *sensitive*, but that they are so by very different titles. In the posterior, the source of the sensibility is at the centre and diffuses itself to the circumference; in the anterior, on the contrary, the origin of the sensibility is at the periphery and is propagated towards the centre. It is for this reason that I give to the latter the name of recurrent sensibility.

"To prove that the sensibility of the anterior root really proceeds from the periphery, I divide it transversely towards the middle of its length, and of the two ends which result from its section, that at the periphery remains *sensitive*, while the central is *insensible*. To demonstrate that this sensibility of the anterior rachidian root is acquired and that it takes its source in the corresponding posterior root, I divide likewise this last, and instantly the anterior root loses all its sensibility. Of the two ends which result from the section of this posterior root, that which adheres to the ganglion has become insensible, while that attached to the medulla still possesses an acute sensibility. Hence this root directly receives its sensibility from the spinal marrow.

"I shall dwell no further on these results, which are literally the same that I announced to the academy in 1839, and which I have demonstrated in my lectures at the College of France; I will only add that I maintain them to be rigorously exact, and that I am now, as then, always ready to exhibit them to those who signify a desire that I should do so. At present I proceed to report certain facts which are of a nature to throw new light on the phenomenon of the recurrent sensibility of the anterior roots.

"Differing in this from the posterior roots, which are constantly sensible, it sometimes happens that on interrogating the anterior root we find it deprived of sensibility. This is particularly observable when, the opening of the rachis and the separation of the root having been laborious, the animals are weakened by pain and loss of blood. But this insensibility is only temporary; it suffices to wait a few instants, and the phenomenon reappears and subsists as long as the state of the wound and of the environing parts allows of its being observed.

"This momentary disappearance of sensibility in the nerve is a very singular phenomenon which pertains to the recurrent sensibility, and which distinguishes it essentially from the direct sensibility of the posterior root, a sensibility which I have never known completely to disappear. When, however, the experiment is suitably made, we recognize immediately, on the opening of the rachis, the recurrent sensibility of the anterior root, and I have realized that, to succeed, the best process is that in which the spinal marrow is laid bare only on one side and to the extent of one or two vertebræ. I have moreover remarked that, when the experiment has been as well performed as possible, if the animal has lost a certain quantity of blood, the phenomenon is not manifested; and I add that, at the moment when it is most apparent, it may be made to disappear by the operation of blood-letting.

"In my first experiments, before I knew the influence of the causes just pointed out, it had happened that I had found the anterior roots sometimes sensible, sometimes insensible. This result, which might then have appeared contra dictory, is, however, but the rigorous expression of the facts, and depends on that remarkable peculiarity, that a sensitive nerve may, under certain influences, temporarily lose it sensibility and afterwards recover it.

"The sensibility which I call *recurrent* does not pertain exclusively to the anterior roots of the rachidian nerves; I have found it also in the facial nerve, and it exists probably in still other nerves. I am now occupied with researches in this respect, and shall probably have the honor of communicating them to the academy. I shall merely add to this note the recital of some experiments which I have made with my habitual collaborator, Dr. Bernard, well known to the academy, and who has taken the trouble to describe them. *First experiment:* In a living and healthy dog, three to four months old, the lumbar medulla was laid bare on the right side to the extent of two vertebræ; the nervous roots exposed were the fourth and fifth lumbar. Immediately after the experiment, by which the animal was somewhat distressed, the anterior root of the fourth lumbar pair was pinched and yielded no manifest signs of sensibility; neither did the anterior root of the fifth lumbar pair. Those two roots were then cut in such a way that there might result from the section peripheric ends and ends adjoining the marrow. The animal evinced no pain at the moment of the section of the anterior nervous roots, and the pressure upon these two ends of nerves, resulting from the section of the root, produced no evident signs of sensibility. Afterwards the facial nerve on the jaw was laid bare and its branches were transversely divided, when again the peripheric ends, on being pricked, manifested no signs of sensibility. After these first trials the animal was left for some moments at rest from the general disturbance caused by the experiment. It then became possible to verify in the clearest manner the sensibility of the peripheric ends of the anterior roots of the fourth and fifth lumbars. The peripheric ends of the branches of the facial nerve also evinced sensibility in the most distinct manner. This recurrent sensibility of the anterior lumbar roots and of the facial nerve was prolonged with equal intensity during the whole day. Four hours after the experiment, the acute sensibility of the peripheric ends of the lumbar roots and of the facial nerve was still found to exist in the most lively activity, and continued to do so even the next day, (twenty-four hours after the experiment,) notwithstanding the commencement of swelling and suppuration of the wounds. *Second experiment:* In a lively and healthy dog, of medium size and from four to five months old, the lumbar medulla was laid bare on the right side to the extent of a single vertebra, the pair exposed being the fifth lumbar nerves. The anterior root of this nervous pair being lightly disengaged, was pricked and found to be evidently sensible. After this sensibility had been verified at several intervals, the anterior root was cut, and the peripheric end found to retain its sensibility, while the end adjoining the marrow had become completely insensible. The wound of the back was then sewed up. On being examined anew five hours afterwards, nothing had changed; the sensibility of the peripheric end of the anterior root remained in full activity, and the complete insensibility of the central end was persistent. The corresponding posterior root was now cut, and pain and agitation were apparent. But the recurrent sensibility of the anterior root immediately disappeared; so that of the four nervous extremities resulting from the division of the anterior and posterior roots, there was but one, the central end of the posterior root, in which sensibility continued to be lively. Twenty-two hours after the experiment the wound had become fetid, suppuration having commenced; yet the exquisite sensibility of the central end of the posterior root, and the complete insensibility of the three other ends, could be distinctly verified. *Third and fourth experiments:* From these, made on animals from four to six months old, the same facts result, namely: sensibility *en retour* of the anterior roots, and of the facial. Sometimes at the very moment of the experiment this recurrent sensibility is not quite evident, and it is then necessary to wait some instants for the animal to recover a little from the disturbance caused by the pain of the experiment. It was then ascertained that the section of the posterior root caused the recurrent sensibility of the anterior root constantly to disappear, and this even eighteen hours after the operation. It was seen also that the sensibility *en retour* of the anterior root having been once destroyed reappeared no more, however long a time it might be waited for. *Fifth experiment:* Made on an adult dog, with the same constant results of the recurrent sensibility persisting till the next day; (the root was attached with a thread.) *Sixth experiment:* Two lumbar vertebræ of an adult dog were laid bare, and it was found, after the animal had been left at rest for some ten minutes, that the anterior roots were sensible; the animal was then bled, and this sensibility *en retour* disappeared, while the sensibility of the posterior roots was always persistent."

Conclusion.—I have repeated all the experiments of M. Magendie on the *recurrent sensibility*, as well alone as in company with M. Bernard, and have caused them to be repeated perhaps twenty times in my laboratory by my *aide-naturaliste*, Dr. Philipeaux, whose skilful and practiced hand may justly be regarded as an additional guarantee. I have thus verified, even to the smallest details, all that M. Magendie had observed, and all that he has recounted in his Note of 1847. To reproduce these experiments with success, care must be taken to practice them under the requisite conditions: First, to open the rachis but on one side. Second, to expose the roots of but one or, at most, of two nerves. Third, not to explore the roots until the lapse of five or six hours after the first operation, in order that the animal may have recovered from the disturbance at-first experienced from the blood lost, and that the wound which has become cold may have regained a certain degree of heat and reaction.

These experiments prove: First, that the *posterior root* is exclusively and essentially *sensitive;* I say *essentially*, because if we divide the *anterior* root the *posterior* remains not the less sensitive. Secondly, that the *anterior* root is essentially *motive; essentially*, because if we divide the *posterior* root the *anterior* does not the less remain *motive*. And, on the contrary, the latter (the *anterior*) is essentially *sensitive;* for if we cut the *posterior root* it immediately ceases to be so. It is not so, then, in itself—it is only so through the other; and so exclusively through the other that if, leaving this other (that is, the *posterior root*) intact, it be itself cut, it is that one only of its two ends which is attached to the other (to the posterior root) that remains *sensitive*.

There remained a final experiment, and this M. Magendie proceeded to make. The *sensibility* of the *anterior root* flows to it, is derived from the *posterior root*, but how and by what means? What route does it pursue? Is this derivation accomplished immediately on the junction or union of the two roots, or afterwards? "This extinction," says M. Magendie, "of the sensibility of the anterior root by the section of the posterior root is a constant fact, and suggests the experiment of cutting the nerve beyond the junction of the roots, leaving these latter intact. I isolate the trunk of a rachidian pair at nearly six lines from the ganglion. You see plainly that the roots of this nerve are sensitive; I irritate them one after the other, and the animal appears to feel almost as acutely when I touch the anterior as when I touch the posterior root. I now cut the trunk of the nerve at about four lines from the junction of the roots. Let us try whether these roots remain sensitive: I pinch the posterior root and the sensibility is the same—it has neither diminished nor augmented. I pinch the anterior root and find that this is no longer sensible. I conclude, then, that the sensibility furnished by the posterior root to the anterior is transmitted at a point still more remote than that on which I have operated. New researches must be made before the place of the nerve can be determined at which this transmission occurs. Can we suppose that it is at the very extremity of the nervous divisions, and that the fibres of termination inosculate with one another?" (*Lectures on the Nervous System, &c.*, t. ii, p. 344.)

I have also repeated this experiment many times and with a uniform result; the section of the two *roots*, immediately after their junction, abolishes the *sensibility* of the *anterior root* just as it is abolished by the section of the *posterior root* itself.

EXPERIMENTS ON THE FIFTH AND SEVENTH PAIRS.

1. One of the finest experiments of M. Magendie was that which he performed on the *fifth* pair. M. Bell had already made this experiment, but by another procedure. He had contented himself with cutting the sub-orbital and frontal nerves, and the nerves of the chin at their exit from the canals of which they bear the name. M. Magendie was the first who cut the *fifth* pair within the cranium.

"The nerve which presides over the sensibility of the whole face," he says, "is the fifth pair. You will see that, by cutting it in the cranium, we abolish not only the tactile sensibility of the skin and soft parts, but even the special sensibility of the senses in the whole side of the face corresponding to the section. Thus the vision, the smell, the hearing, the taste, will be lost from the fact alone that the fifth pair has been cut; and yet the nerves that it is agreed to regard as presiding over the exercise of each of these senses shall have suffered no injury." (*Lectures on the Nervous System*, II, p. 27.) This is what he first said; he afterwards stated more exactly: "The section of the fifth pair acts on the nutrition of the organ of smell as on that of the eye; the disorders are secondary." (*Ibid.*, p. 43.) The section of the *fifth* pair is limited, in effect, to the immediate, the direct destruction of the *tactile sensibility*. It destroys vision, smell, hearing, only by the consecutive disorder. M. Magendie himself verified this as regards vision, or the sensibility of the retina. "I cut the optic nerve at its entrance into the eye; if the nerve of the fifth pair or any other could perceive light, the section thus made could not prevent it. But it is quite otherwise; the sight is completely abolished, as well as all sensibility, to the strongest light, even that of the sun concentrated by a lens. Wishing to submit to this last proof an animal whose fifth pair alone was divided, I easily recognized that on causing the eye to pass suddenly from shade to the direct light of the sun, there was an impression, for the eyelids closed. All sensibility, therefore, is not lost in the retina by the section of the fifth pair, though there is but a feeble portion which remains." (*Precis Elementaire de Physiologie*, t. i, p. 100.) M. Magendie had thus formed a just idea as regards *vision;* and the same would have been the result as regards *audition* and *olfaction* if he had made on the acoustic and olfactory nerves the experiment which he performed on the optic nerve.

2. It was known from the admirable experiments of M. Bell that the *sensibility* of the face proceeds from the fifth pair, and the *respiratory movement* from the seventh. A very fine experiment of M. Eschricht, of Copenhagen, suggested to M. Magendie an inquiry in order to determine the action of the fifth pair on the seventh. (See *Journal de Physiologie*, t. viii, p. 228 and 339.) In his *Lectures* we follow, step by step, the progression by which he passes from the idea of a recurrent sensibility by *nervous anastomosis* to the idea of a simple association of the fibres of one pair with those of another.

"The sensibility of the facial nerve," he says, (*Leçons sur le Systeme Nerveux*, t. ii, p. 166,) "is communicated to it by its anastomosis with the fifth pair. This expression of *anastomosis* will cease to represent a just idea if, by anastomosis, we understand the fusion of two nerves into a single one; there is no true fusion in the case. I had heretofore thought the contrary, but my last experiments force me to modify my views. The branches of the fifth pair, and of the seventh, which we find united, have their fibres associated in such a manner that each fibre preserves the respective properties of the nerve from which it emanates. Thus the fibres of the fifth pair remain sensitive fibres; the fibres of the seventh pair remain motive fibres. The trunk which they constitute by their association is thus a trunk composed of motive and of sensitive fibres, and not of a single order of fibres sensitive and motive at the same time. Cut the nerve of the fifth pair; there remain only motive fibres, and all sensibility disappears. If then I avail myself indiscriminately of the words *association, anastomosis,* you will know what signification I give them. (*Leçons, &c.*, t. ii, p. 185.) We know now what is to be understood by anastomosis between a nerve of sensibility and a nerve of movement. The sensitive fibres associate themselves with the motive fibres, and wherever the former exist you meet with sensibility. The association of two nerves is attended with no confusion of their properties; they are exercised conjointly in the same nervous trunk, but they remain independent, since we can isolate them. (*Ibid*, p. 191.) The seventh pair receives from the fifth, not sensibility, but sensitive fibres. Its own fibres form a nerve exclusively motive. (*Ibid*, p. 209.)

Cerebro-spinal liquid.—As early as 1825 M. Magendie had read to the academy a memoir on this subject; he read a second in 1826, a third in 1828. Long before, however, in 1769, the celebrated Italian physician Cotugno had recognized and described this liquid; but what Cotugno had said was altogether forgotten when M. Magendie in turn occupied himself with this important subject.*

* *Circa medullam, per spinam descendentem, &c.*—"Around the medulla, as it descends through the spine, there is a considerable space; but this extra space is not wholly free. Along it descends the *dura mater*, which, formed into a tube at its exit from the great occip-

What the Italian did not see or saw but incompletely, and M. Magendie has placed beyond doubt, is: 1st, that this liquid exists in all ages; 2dly, in the normal as well as disordered state; 3dly, that it is deposited not in the cavity of the arachnoid, but below that membrane, around the encephalon and the medulla; 4thly, that the *pia mater* is its secreting organ; 5thly, that there is a particular point, called by M. Magendie the entrance of the ventricles, by which the liquid penetrates from the exterior of the brain into the cavities, into the *ventricles*, the *conduits* of that organ.

"Is it not remarkable," he says on this occasion, "that the parts of the brain named by the ancient anatomists *valvula, aquæ-ductus, pons*, have precisely the use which their name indicates? The *valvula* of Viensses, or the great valve of the brain, fulfils, beyond doubt, the functions of a *valve*, since it resists the egress of the liquid which traverses or which fills the fourth ventricle. No part could better merit its name than the *aqueduct* of Sylvius, since, according to the experiments which I have reported, this canal conveys sometimes the water of the ventricles towards the spine, and sometimes from the spine towards the head. Lastly, what is called the *pons* is, in effect, a great medullary arcade, inverted and placed below the current of the liquid which traverses the aqueduct."—(*Journal de Physiologie*, t. vii, p. 29.)

A great anatomist of recent times, Sœmmering, even places the *seat of the soul* in the *liquid* of the *ventricles of the brain:* "If it be permitted us to assign a peculiar organ to the common sensorium, or if there be a proper seat for the common sensorium in the brain, it is not without some show of reason to be sought for in this liquid."*

APPENDIX II.

ANALYSIS OF THE RESEARCHES OF SIR CHARLES BELL ON THE NERVOUS SYSTEM.†

In order to convey a clear idea of the merits of M. Bell, and the particular character of his new views on the nervous system, it is necessary to say a word respecting the doctrines which prevailed, previous to his time, on that important subject.

"If we pass in review," says M. Bell himself, "the opinions respecting the brain and nerves which have been held at different times, we find one theory which had been maintained from the Greek authors down to the age of Willis, and which has been transmitted, with few changes, to modern authors." Now, this theory which had passed from the Greeks to Willis, and from Willis to

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ital foramen, encloses the spinal marrow, like a sheath, to the termination of the *os sacrum*. Yet this tube of the *dura mater* is not so wide as to reach everywhere the circuit of the cavity of the spine, nor so narrow as to embrace closely the included medulla; but is somewhat distant from the cavity of the spine, chiefly behind and at the place of the apophysis, and is at the same time separated by a considerable space from the periphery of the medulla which it encloses. Now, this space between the vagina furnished by the *dura mater* and the marrow, is always *full*, not with medullary matter, nor, as some learned men in so obscure a subject have conjectured, with a vaporous exhalation, but with *water*, similar to that which the pericardium confines around the heart, which fills the hollow spaces of the ventricles of the brain, the labyrinth of the ear, and, in short, all the cavities of the body from which the free air is to be excluded, &c. (Dominici Cotunni: *De Ischiade Nervosa Commentarius*, Vienna, 1770.)

* Peculiare organum sensorii communis si ponere fas est, vel si propria sedes sensorio com muni in cerebro est, haud sine veri quadam specie hoc in humore quæri debet.—(*De Corporis Humani Fabrica*, vol. iv, p. 69, 1798.)

† In this analysis of the researches of Bell there will doubtless be some repetition of what has been said in the Memoir or Notes, but it is not the less needed in order to present the complete filiation of ideas.

modern authors, was the theory of *spirits* derived from Galen. Galen, that great theoretical cultivator of ancient physiology, had imagined *spirits* of three kinds: the *natural*, which were formed in the liver; the *vital*, which were formed in the left ventricle of the heart; and the *animal* or *cerebral*, which were formed in the brain. Willis very much simplifies matters; with him, all the spirits are formed in the brain: *Statuimus hos spiritus solummodo in cerebro et cerebello procreari;* all, it seems, are formed in the brain, and with not much difficulty, as we shall see. It is, in fact, merely a chemical operation: *velut in opus chimicum.** The blood is carried into the brain and the cerebellum by the narrow vessels of the plexus, as by the flexuous tubes of an alembic, *veluti per serpentinos alembici canales;* and then, in the substance of these two organs, *intra utriusque substantiam*, the finest, most volatile and most subtle particles are evolved from the blood; these particles are the *spirits*.

But do these *spirits* really exist? This is the only question which Willis forgets to propound; for, in other respects, there are not a few which he proceeds to ask. He inquires why it is that these spirits, being so thin, so subtle, so fitted for escaping—*particulæ ad avolandum aptæ*—do not in effect escape; and he answers that they are confined or *cohibited*, to use his own expression, by the membranes of the brain, as by the cap of an alembic—*velut alembici obductione.* He even asks what they are—*quid sint?*—and he avows that he cannot venture to compare them too closely with the *spirits of wine* or of *terebinth*, seeing that these may be poured from one vessel into another, or even be distilled, without being lost; while, as for the *animal spirits*, they are so subtle that after the death of the animal no traces of them are to be found.

These *animal spirits* were the mysterious resource of all modern physiology, from Descartes to Borden.† Borden having skilfully turned them into ridicule, less use was made of them. Eventually, in the latter years of the last century, a new term was substituted for the old one; the *animal spirits* became the *nervous fluid.* M. Cuvier still wrote in 1817: "It seems to us that an explanation may be rendered of all the phenomena of physical life by the simple admission of a fluid such as we have just defined;"‡ that is to say, the *nervous fluid.*

Things were at this point, or nearly so, at the time when M. Bell began to think for himself on the nervous system. "It was supposed," he says, "that the brain secreted a *nervous fluid*, that the nerves were the channels which conveyed it, and that all were endowed with the same properties. With this apparent simplicity of doctrine," he adds, "never has there been presented so great a number of errors in the history of any science."

The basis of this doctrine, so *simple*, as M. Bell justly remarks—for the whole is here reduced to a single fluid—was the *uniformity*, the *homogeneousness* of action. For homogeneousness of action M. Bell substituted *speciality* of action, and this was the first, the fundamental feature which separates his theory from the old one. "The key," he says, "of my whole system will be found in this single proposition: each filament of nervous matter is endowed with a peculiar property independent of that of other filaments which occur along with it, and this it preserves throughout its whole extent." (*Natural System of the Nerves of the Human Body.*)

The whole work of M. Bell is an analysis. From the date of his first *essay*, in 1811,§ he separates and distinguishes the functions of the *posterior roots* of the nerves from the functions of the *anterior roots.* This essay, too far in advance of the physiologists of that day, attracted little or no notice; the author, through discouragement, maintained silence for ten years. In 1821 he resumed

* *Cerebri Anatome, cui Accessit Nervorum Descrip. et Usus.* London, 1664.

† For the refutation of *animal spirits* by Borden, see my work entitled *De la Vie et de l'Intelligence*, p. 43, &c. Paris, 1858.

‡ *Le Regne Animal*, &c., t. 1, p. 35, first edition.

§ *Idea of a New Anatomy of the Brain; submitted for the observations of his friends.*

the subject, and presented to the Royal Society of London his *Natural System of the Nerves,* a new analysis, almost as ingenious and subtle as the first, and which gives him his three classes of distinct nerves: the nerves of *sensation,* those of *voluntary movement,* and those of the *respiratory movement.*

I shall divide the labors of M. Bell as he has divided them himself; I shall distribute them into two epochs. The first will be that in which he distinguishes the functions of the *roots* of the nerves; the second, that in which he discriminates the different *classes* of nerves from one another. I shall afterwards say something of his discussion with M. Magendie.

FIRST EPOCH.—*Distinct functions of the roots of the nerves.*—How did M. Bell arrive at this great and fundamental distinction? He has himself told us: "I owe my first ideas to the inductions which I drew from the anatomical structure, and the small number of my experiments had for their object only the verification of these first ideas." He adds: "When, in making researches on a subject of this nature, we follow the natural order of ideas and a philosophical method, when we examine with care the facts which anatomy presents, every experiment is decisive, and truth shows itself so evident and simple that nothing can be more satisfactory to the observer." This is well said, and discloses the process of the author; he proceeds from anatomy to experiment, but to experiment only in his own defence. What, then, did anatomy yield him?

In observing with close and constant attention the disposition, distribution, origin, in a word, the *anatomy* of the nerves of the spinal marrow and of the encephalon, what especially struck him—and the more forcibly the more he considered it—was the perfect regularity of the former and the extreme irregularity of the latter. All the nerves of the spinal marrow are perfectly regular; they all spring from two series of roots pertaining, these to the posterior region, those to the anterior region of this medullary column. The nerves of the encephalon, on the other hand, are all of a surprising irregularity; some spring from a single anterior root; others from a single posterior root; others, again, from a single lateral root. One only, that of the fifth pair, springs from two distinct roots—one posterior and the other anterior—like the nerves of the spine.

Whence this diversity of origin of the nerves of the encephalon? Wherefore two sources for the nerves of the spinal marrow? Would not this diversity of origin indicate a diversity of functions? Thus the leading idea—the idea of diversity of function suggested by diversity of origin—was grasped; and thus far the genius of the discoverer stood as the sole intermediary between the idea and anatomy. But it remained to retrieve this idea, so novel and withal so true, from the category of mere supposition or conjecture, and for this anatomy did not suffice. When Harvey—that perpetual model for all who propose to study the obscure mechanism of living beings—had satisfied himself of the arrangement of the valves of the veins, disposed in such a manner as to favor the course of the blood from the members to the heart, and obstruct it from the heart to the members, he made a very simple but decisive experiment. He tied a vein, and saw it swell on the side of the members, and not on the side of the heart. The blood therefore flowed, in the veins, from the members to the heart, and not from the heart to the members, and the circulation of the blood was demonstrated.*

M. Bell did as Harvey had done. After having long meditated on the anatomical disposition of the *origin* or *roots* of the nerves, he devised an experiment which should likewise be decisive, and executed it. "It was first necessary," he says, "to ascertain whether the phenomena, developed after the lesion of the different roots of the nerves of the spine, were found to correspond with what anatomy would indicate. Hesitating for some time on account of the nature of

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* See the *Histoire de la Decouverte de la Circulation du Sang*, by M. Flourens, p. 41, (second edition.)

the operation, I at length proceeded to open the cavity of the spine in a rabbit, and I cut the posterior roots of the nerves of the inferior extremity; the animal could still move itself, but the cruelty of this dissection prevented me from repeating the experiment. I thought it would suffice to make it on an animal newly stunned and insensible. I therefore struck a rabbit behind the ear, so as to deprive it of sensibility by the shock, and then laid bare the marrow of the spine. On irritating the posterior roots of the nerve, I could perceive no consequent movement in the muscular tissue; but when I proceeded to irritate the anterior roots, there ensued, whenever the instrument touched them, a corresponding movement in the muscles to which the nerve was distributed. These experiments proved that the different roots, and the different columns whence those roots proceed, are destined to different functions, and that the indications furnished by anatomy were correct."

It would be puerile and unworthy to dwell too much upon the faults of this experiment, ever memorable as it is, as being the first. The author says that "the cruelty of the experiment prevented him from repeating it." If he had observed more closely what took place when he cut the posterior root, (and he might have confined himself to irritating it, which would have been still less cruel than cutting it,) he would have seen the animal give signs of distress; the indications of sensibility, of pain, would have manifested themselves, as they always do in such cases, and, in this respect, everything would have been determined. In the second place, to begin by stupefying the animal, by rendering it *insensible*, when *sensibility* was precisely one of the phenomena to be verified, was no doubt the way to render the experiment less painful, but it was also, with deliberate purpose, to render it insufficient and incomplete. It was to instigate more complete ones, which would be made by others, and in effect to multiply the chances of *cruelty* in place of diminishing them.

And this was, in fact, what happened. The experiments of M. Bell were everywhere repeated, and no one was more gratified thereat than himself. "I have only to add," said he in 1825, (fifteen years after his first *Essay*,) "that it has redounded to the satisfaction of all Europe, that these opinions and experiments have been followed up. It has been recognized that the anterior roots of the nerves of the spine excite muscular movement, and the posterior roots sensibility. When, in an experiment, the anterior roots of the nerves of the leg are cut, the animal loses all power over the leg, although the member preserves its sensibility. But if, on the other hand, the posterior roots are cut, the faculty of movement will still exist, although the sensibility be lost. If the posterior column of the spinal marrow in an animal be irritated, it appears sensitive to the pain, but no similar effect is observed when the anterior column is touched."

SECOND EPOCH.—*System or classification of the nerves.*—In 1821 M. Bell read to the Royal Society his first memoir on what he calls the *natural system of the nerves*. His *Essay* of 1811 had passed almost without notice, as has been just said. The present memoir produced a profound impression. It was because the author at last addressed himself to the true judges, to a learned company, and because by the very reading of his work he stimulated it to reflection. It was, besides, the first time that the light of physiology had penetrated into the study, until then purely anatomical, and until then wholly sterile also, of the *nerves properly so called.* What had the anatomists of all countries been doing, before M. Bell, in their researches on the different nerves? They had been emulously striving who should find some new ganglion or some unknown nervous filament, without, however, attaching the least idea to that ganglion or that filament.

"As long as it is supposed," says M. Bell, "that the nerves proceed from a common centre, that they have the same structure and the same functions, that they are all sensitive, and that they all contribute to convey what is vaguely called the *nervous current*, these discoveries of new nerves and new ganglions will not only be useless, but prejudicial; they will serve only to increase the confusion."

A single philosophical, but just and precise idea sufficed M. Bell for the removal of this confusion. He sees two distinct nerves distribute their branches to all parts of the face; three nerves of different origin proceeding to the tongue, four to the throat; the nerves of the neck presenting a most complicated arrangement; and he inquires why is this? Why this multiplication of nerves—why different nerves for the same part? Why, for example, two nerves for the face? "Here," says M. Bell, "we naturally ask, whether these nerves fulfil the same function—whether they contribute a double *contingent* of the same property or *faculty*, or whether they do not fulfil different functions? After having called to our aid," he continues, "all the information furnished by the structure of the human body and by comparative anatomy, we proceed to decide the question by experiments." Thus we see, it is always anatomy which proposes the question, which suggests the idea; but, as we further see, it is always experiment, and experiment inevitably, which gives the answer.*

M. Bell therefore divides, in an animal, the seventh pair, that is to say one of the two pairs between which a decision is to be pronounced, and the animal loses only the *respiratory movement* of the face, simply the *dilatation of the nostrils.* He divides the fifth pair in another animal; this loses the movement of the lips which serves for manducation, a *voluntary movement*; but it preserves the movement of the nostrils, a *respiratory movement.* The former no longer respires by its *nostrils*, yet uses its lips to collect its food; the latter respires by its *nostrils* as usual, but can no longer use its lips to collect its food and eat. Nor is this all: when the seventh pair is cut, the animal does not suffer; and when the fifth pair is cut, it experiences acute pain. Again, the seventh pair being cut, the face preserves all its sensibility, while, the fifth pair being cut, all the sensibility of the face is lost. A difference of properties, then, of the most express nature separates the seventh pair from the fifth. The former is *insensible*, is purely *motive*, and serves only for the *respiratory movement* of the face; the latter is at once *sensible and motive*, and serves at the same time for the *sensibility* of the face and its *voluntary movement.*

There are nerves, therefore, which are only *motors*, like the seventh pair; others which are at once *motors* and *sensitive*, like the fifth; and others still which are only sensitive, like all the nerves, for example, of the *special senses.* And, as regards each nerve, its property, its function depends on its origin. The nerve of the fifth pair springs from two roots, the one *anterior*, the other *posterior*; it derives from the former its *motive* faculty, and from the latter its *sensitive* faculty; it is a *double* nerve, a nerve composed of two nerves, one anterior, the other posterior—one for *movement*, the other for *sensation.* Though springing from the encephalon, it is, from its two-fold origin and its two-fold function, a true nerve of the spine.† The nerve of the seventh pair, on the contrary, is a *single* nerve; it has but one origin, and that is anterior; it is purely *motive.* Lastly, the nerves of the senses have also but one origin, but this is posterior, and they are purely *sensitive.*

Upon this principle of their radication, then, (and the great result of the researches of M. Bell is to have taught us that it is by their radicles that we must

* "Experiments," says M. Bell, "have never led to discoveries." (*Natural System*, &c., p. 251.) This is not wisely said; it is to make experimental art bear the whole burden of the ill-humor entertained towards the experiments of M. Magendie. "Let the physiologists of France," M. Bell further says, "borrow from us and follow up our opinions with experiments," (p. 257.) This *troublesome* M. Magendie has cost us many a sarcasm.

† Add to this that, like the nerves of the spine, the posterior root, that namely of *sensation*, is provided with a *ganglion.* It was supposed, previous to M. Bell, that the office of the *ganglions* was to arrest sensibility. It has been found, by experiment, that the *posterior*, that is the *sensitive* nerves are precisely those which have a ganglion, and they are the only ones which have it.

be guided,) there are three classes of nerves; nerves purely *motive*, nerves purely *sensitive*, and nerves at once *motive* and *sensitive*: the first with a single and *anterior* root, the second with a single and *posterior* root, the third with roots both *anterior* and *posterior*. Never had a more vivid light been shed upon a subject more obscure, confused, and, to use the received term, more *inextricable*.

The nerves, then, are at length classified, and their classification may be called, as M. Bell in fact calls it, a *natural* classification; for it is given at once by the *origin* and the *function*—that is to say, by the two characters which are the most sure, whether regarded in the light of anatomy or physiology.

I have counted but three classes of nerves; M. Bell makes four: it is because I deliberately omit the *great sympathetic*, upon which he has bestowed too little attention to make it worth mentioning here.* It has been seen, moreover, that, besides the two columns of the spinal marrow, the *anterior* and *posterior*, M. Bell admits a third, which he calls the *lateral column*. This third column, which appeared to him so important, is at present, and with reason, regarded as being at least very problematical. Of the five nerves which M. Bell considers as springing from it, and which are the five *respiratory* nerves—the *pathetic*, the *facial*, the *spinal*, the *pneumogastric* and the *glaso-pharyngian*—the first three spring from the anterior column and are exclusively *motors*; the fourth springs from two roots and is *motive* and *sensitive*; the fifth springs from the posterior column and is exclusively *sensitive*.

To consider, then, only what is clear, established, and will be permanent in the researches of M. Bell: the spinal marrow (and I do not here separate from it the *medulla oblongata*) is composed of two columns, the one *sensitive*, the other *motive*. All the nerves which spring from the sensitive column are *sensitive*; all those which spring from the motive column are *motors*. From the *origin* may always be inferred the *function*; from the *function* may always be inferred the *origin*; and M. Bell was not deceived when, from the simple anatomical diversity of *origin*, he had foreseen the physiological diversity of *function*.

THIRD EPOCH.—*Discussion of M. Bell with M. Magendie.*—This discussion relates to two points: first, the discovery of the proper functions of the roots of the nerves; and, secondly, the discrimination of the special functions of the seventh and fifth pairs.

In reference to the first point, M. Magendie read to the Academy, July, 1822, a memoir in which he announced that having cut the posterior root of a nerve in an animal, he had only abolished *sensation*, and that having cut the anterior root, he had only abolished *movement*. This result was received with the admiration merited; but some months later M. Magendie read a second memoir, in which he retracted all that he had said and discredited all that he had done. Each root was no longer exclusively *sensitive* or *motive*; each was both one and the other: only, the anterior root was more motive than sensitive, and the posterior more sensitive than motive.

There could scarcely have been a greater advantage offered to M. Bell. Hence, in a *note* which he attributes to a pupil, a pretence which could not but render recrimination more easy and acrimonious, M. Bell incessantly presses his adversary with this dilemma: Either, he says to him, you adhere to your former memoir, and then I have over you a priority of at least ten years; or you abide by the second, and there is then nothing in common between us, no longer any subject of debate; I maintain the exclusiveness of action of each root, and you return to the old idea of the accumulation of two actions in each. What reply could there be to this reasoning? As a man of sense, M. Magendie did not reply at all; he did much better; he made a new discovery, and one fully as difficult at least as the first: he discovered the *recurrent sensibility*.

* He regarded it as the *lien* "which seems to unite the three other orders of nerves (those of *sensation*, those of *voluntary movement*, and those of the *respiratory movement*) and the body itself in one whole." (*Natural System*, &c., p. 5.)

I proceed to the second point of the discussion: the special functions of the fifth and seventh pair. M. Bell had contented himself with cutting the fifth pair at its exit from the *foramina* of the face; M. Magendie boldly divided it within the skull. The result, from this circumstance alone, was more striking; but from this result, imposing as it was, how many inadmissible consequences did he draw? He pronounced the fifth pair to be not only the nerve of general sensibility, of the *tactile* sensibility of the face, but also the nerve of all the proper organs of the senses. It was the nerve of *olfaction*, of *audition*, of *vision;* he took certain simple effects of a subsequent alteration for *direct* and *immediate* effects.

M. Bell, more practiced than he in analysis, pointed out to him that the phenomena in question are more complicated than he supposed. In the phenomenon of *olfaction*, for instance, there is the sense of smell, the act of scenting, and the tactile sensibility; the first depends on the olfactory nerve, the second on the seventh pair, and the last on the fifth pair. "Having caused a dog," says M. Bell, "to respire ammonia, after the seventh pair had been cut, it experienced at first no effects of irritation of the pituitary membrane. This was because it no longer possessed the power of snuffing up, or drawing in with force, the ammoniacal vapor. * * * If I had not paid attention to these circumstances, I might have believed, in view of what I saw, that the nerve of the seventh pair was the nerve of smell, as a well-known French physiologist has done, who has concluded too precipitately that he had discovered the nerve of vision and of smell in the fifth pair."

It is not difficult to see the different turns of mind in the two men whom I compare: the one was more meditative, more thoughtful—the other more a man of action than of thought; the one might be said to have placed no sufficient value on experiments, the other no sufficient value on ideas; the one would never probably have made experiments had he not begun by having ideas; the other might perhaps have had but few ideas had he not begun by multiplying experiments.

Charles Bell (born 1774, died 1842) was the fourth son of a poor Presbyterian minister of Scotland. The second of his brothers, John Bell, having become professor of surgery in the University of Edinburgh, invited him thither, and initiated him in anatomical studies and instruction. First impressions are strongest; a single good indication given in time furnishes sometimes the opening for a career. Mr. John Shaw, in a prefatory account of Mr. Bell, * tells us that "he commenced his public labors as assistant of his brother, John Bell, who relinquished to him the part of the course of anatomy which treats of the nerves, and counselled him to study the brain by its base, by its relations with the spinal marrow, instead of cutting it by horizontal sections. The intelligent student soon perceived many things which he has developed in this volume, and which might perhaps have always escaped him, had he not then formed the habit of constantly considering, under this point of view, the relations of the brain with the rest of the nervous system."

Arrived at the age of thirty years, Charles Bell felt ill at ease in a dependent situation. Quitting his brother, and removing to London, he there long endured isolation and poverty. In 1805 appeared his first work, the *Anatomy of Expression.* In 1811 he produced his *Sketch of a New Anatomy of the Brain,* the small success of which was the source, as has been already said, of much discouragement. Having been appointed surgeon of the hospital of Middlesex, he seemed wholly devoted to instruction and practice; yet we find, in one of his letters of that date, this passage: "Under a semblance of forgetfulness I am always brooding over my grand idea; it occupies me unceasingly."

Some years later, on quitting a session of the Royal Society, in which he had read his first and successful memoir, he remarked: "At last I may assure myself that I am no visionary. My discovery will place me by the side of Har-

* The Nervous System of the Human Body, as explained in a series of papers read before the Royal Society of London, with an appendix of cases and consultations on nervous diseases. Edinburgh, 1836.

vey. This affair of the nerves may require much time to become what it ought to be, but I indulge myself with the idea that I have made a discovery not surpassed by any in anatomy; nor am I yet at the end of it." This, however, was but a passing gleam of satisfaction; ambitious and delicate minds are always vacillating between exaltation and distrust. He wrote shortly afterwards: "The satisfaction which I have enjoyed in my researches has been very great; the reception which science has given them has been the reverse of what I expected. The earliest announcement of my discoveries obtained from professional men not a word of encouragement. When, subsequently, the publication of my memoirs by the Royal Society rendered it impossible that some attention should not be paid to them, the interest which they excited inured only to the advantage of those who contradicted them, or those who pretended to have anticipated me. It has become for me an affair of but little importance." He was mistaken; it was to him *the important affair;* he would have preferred, however, that it should have been a little more so that of all the world, and that the attention at first bestowed should not so soon have grown languid. He did not recollect Fontenelle's saying: "The admiration of mankind is a sentiment which only craves leave to terminate."

The memorials of our savant have been collected, as I said just now, by his brother-in-law, John Shaw. The warm controversy raised by the new views which he had introduced into science was distasteful to his meditative and serious temper, as well as to his sense of personal dignity. He stood aloof from it, and returned into Scotland, almost disgusted with ambition. "My dear friend," said to him the aged Professor Lynn, "you will never change. If you live to be old, you will be the same child, but with crutches." After his poetical manner, he writes in his private journal, at his departure from London: "I find myself to-day like a bird whose nest is in the cap of a school-boy."

A sojourn in his country wrought in him no change of mood. After filling for some years, at Edinburgh, the chair which his brother had occupied, he travelled on the continent. Artistic Italy atracted the greatest share of his admiration. The following is his judgment, evincing at least profound feeling, respecting Michael Angelo. It should not be forgotten that he himself possessed a superior, an exquisite talent, as designer and painter, and that he has written a book, equally esteemed by the man of science and the artist, on the *anatomy of expression.*

"In these statues," he says, speaking of two statues of Day and Night at Florence, "Michael Angelo has displayed great perception of art and a genius of the first order: the combination of anatomical science and of ideal beauty, or, rather, grandeur. It has been often related of him that he studied the torso of the Belvedere, and had it constantly before his eyes. This fine model of antique art may well have been the authority on which he relied for his great development of the human muscles; but the torso could not have taught him the effects which he produced by the magnificent and gigantic members of these statues. Who is the artist, modern or ancient, that would have voluntarily consented thus to demonstrate the difficulties of the art, and to place the human body in this position? Who that would have thrown the shoulder into this violent contraction, and have preserved, nevertheless, with so learned an exactness, the relations of the parts among themselves, the harmony of the bones and muscles? This great man shows us how genius submits to labor in order to attain perfection. It was necessary to have passed through the severe labors of the anatomist to acquire that power of designing which it was scarcely to be hoped would meet with appreciation either then or afterwards."

On his return from Italy, Mr. Bell, having gone to pass some days at the country seat of one of his friends in the neighborhood of Worcester, was there overtaken by sudden death.*

* See, in the *British Review,* (October 10, 1845,) an article full of interest, by M. Pichot, on Charles Bell. From this I have drawn some of the traits of his familiar life.

ON THE SENSES.

3.—THE SENSE OF TASTE

Translated by C. A. Alexander for the Smithsonian Institution from the German periodical "Aus der Natur, u. s. w."—Leipzig.

"*De gustibus non est disputandum*" is an old and well known commonplace, to which, while prefixing it as a motto, we might give a rendering somewhat different from the usual one, and say: "About taste there is little to be said." No doubt upon such a theme many pages might be easily filled, were we minded to enter the arena of æsthetics and to pursue the chameleon-like idea into all its ramifications; but a sober exposition of the physiological conditions of the sense is so unpromising a subject that we could not help fearing an exposure of the poverty of science, if we did not here, as in our consideration of the sense of smell, propose rather to clear up some of the inveterate errors prevalent among the uninformed than to furnish as thorough an investigation as might be desirable. We reckon herein but little on the interest of the gourmands who may suppose that physiology can be advanced by their manifold experiences in regard to this their favorite sense, and who will, therefore, learn with the deeper indignation that physiology has not only not known how to profit by their researches, but even regards them as the chief sources of the common and widespread errors just alluded to. The fleshy and wonderful member which, as such persons would seem to opine, has been placed at the very inlet of the alimentary canal with scarcely any other view than to preside over gustatory pleasures, offers to our keenest inquiries just as profound an enigma as the organ of smell. Very far are we from being able to explain how that specific sensation, which, without any further power of description, we denominate taste, arises; what is the essential difference between a sensation of sweet and sour; what the real principle which resides in any substance so as to occasion our applying to it one or the other epithet, to say nothing of any possible explication, by the aid of science, of the different *shadings*, as it were, of sapidity cognizable by a nice palate, or of the jargon by which we attempt to characterize them. Here common nerve fibres, between which and those of other organs of sense the closest inspection fails to detect the least difference, serve to interpret to the sensorium, by means of their extremities in the mucous coat of the tongue, the endless variety of so-called flavors of sapid substances; but we seek in vain, either at the outer or inner extremities of these conducting fibres, for any specific apparatus which might enlighten us as to their operations. However indispensable it be, as was shown in our introductory discussion on the sense of feeling, to suppose such an apparatus, yet has all endeavor hitherto to detect it by means of the microscope been baffled; nor, indeed, in the present state of knowledge, were successful research to disclose to us the ends of the nerves of taste provided with minute vesicles or filaments, or any other elementary form, should we understand them any more than we do the already discovered cells at the cerebral termination of all nerves or the small bulbs at the outer extremity of the nerves of touch. It would probably fare with us as with some ancient Roman raised from the grave and suddenly placed in presence of a telegraphic apparatus or steam engine. In the mean time it would seem the next most important problem of science in this connection to study, with more exactness than heretofore, the terminal points of the nerves of taste in the mucous membrane, in order to discover here, as has very recently been effected in the nerves of the other senses, the elementary form of the nerve ends by which the sensation in question is communicated; whereby we may hope to approach, in the future, somewhat nearer to a solution of the mysterious relation which exists between the contact of sapid substances with the tongue and the peculiar sensations which they are destined to excite.

If we bring into contact with the nerves of sight or of feeling a solution of sugar, or of some intensely bitter substance, such as quinine, they remain undisturbed; neither class of nerves is roused to that state of activity by virtue of which a "current" is hurried to the brain, there to call forth an appropriate sensation. We may conclude then with absolute certainty that there is present in the tongue a mechanism, a contrivance which sugar and quinine excite into different specific kinds of action; that this action is suited to evoke in the nerve filaments peculiar modifications of the beforementioned current; and that this current again must find at the cerebral end of the nerves of taste a special apparatus which so operates upon what we call the soul that, as the final result, a perception of sweetness or bitterness arises. These successive preliminaries to a sensation of taste are at present beyond our scrutiny; it will be well if means are at our disposal to resolve, step by step, with clear physiological facts and inductions, the questions which present themselves in connection with each of them. Nor shall we repine that such problems lie before us. The novice errs if he thinks that a science affords to its votaries a higher satisfaction; the greater the completeness with which it presents itself, the more exactly finished the edifice into which it introduces them. A satisfaction of a different and perhaps superior kind is afforded by science through its very gaps and obscure spaces, but only to him who loyally loves it for its own sake; to accompany a science in its development, to contribute, though it were but a small, inconspicuous stone, to the growing structure, affords a more vivid pleasure than being admitted to a participation in what is already accomplished. There are, it is true, self-styled savants to whom also the deficiencies in a science are more welcome than the completed structure, because those deficiencies are regarded not in a spirit of earnest inquiry, but as affording opportunities for the acquisition of easy notoriety and applause. Shunning the toils of research, they find it more convenient to guess at the future revelations of science than acknowledge their own ignorance, and, overweaving all chasms with flimsy theories, they vaunt the result as knowledge converted into luxury and amusement. With these self-satisfied sciolists, who but too often and easily gain access to the public ear, the detail of facts and laws is derided as pedantry; the slow and cautious processes of inquiry as unworthy drudgery; the reserve and modesty of true science as tame mediocrity or affected obscurity. Let the judicious reader be warned against their empty pretensions. If we once more mark with our reprobation this form of charlatanism, it is because no occasion seemed more proper than when attempting to adapt to popular comprehension, without dissembling difficulties or courting effect, one of the most obscure departments of physiology. We return to our subject.

It is not improbable that the first inquiry we have to propose will at the first glance appear to our readers superfluous, and surprise will scarcely fail to be awakened when it is shown how much obscurity really rests upon the seemingly simple question, *What is the seat of taste, and what organs produce the sensation?* With the common and general answer, *the tongue*, unfortunately we cannot rest satisfied, for it remains further to be asked, first, what part of the tongue receives the peculiar impression which we call taste; secondly, whether, besides the tongue, other parts of the cavity of the mouth are endowed with this faculty; thirdly, to what structural arrangements is it due? Strangely enough the answers to the three questions are still in dispute, and in part at least are extremely defective. It would seem, on the whole, highly probable that to a very small tract has been assigned the sense of taste, for with more exact exploration its boundaries have been found to be more and more circumscribed. While the older physiologists acquiesced in the popular notion that the tongue is the organ of taste, and ascribed to the whole mucous coat of the mouth, and even of the jaws and pharynx, the faculty of exciting impressions of taste, most of the modern school are unanimous in the opinion that only a small portion of the dorsum of the tongue, and that indeed the hindmost and nearest to the throat, exercises the prerogative of deciding on the sapid properties of transmitted substances. It is true that there are those even now who regard the whole surface of the tongue, including its under side, and even the mucous membranes which line the orifice of the throat, as embraced within the province of this sense, but in our opinion improperly. No one, however, disputes that the hinder part of the dorsum of the tongue is endowed with at least a finer sense of taste; a conclusion which receives countenance from the commonly observed fact that whenever it is proposed to submit any substance to a closer scrutiny of the sense in question, we instinctively transfer it to that portion of the mouth, where the hard palate also lends its mechanical aid to the investigation; while it never occurs to any one to test the properties of taste with the point of the tongue, or to introduce the object under the lower surface of that member. If it be difficult to prove conclusively that the rest of the mucous regions under review do not possess a sense of taste, it certainly exists there in a much weaker and more obtuse degree. The discrimination is not so easily made as might be supposed; in such inquiries there are not a few incidental sources of error. In the first place, care must be taken for trials of this sort, to choose substances which excite a pure and simple sensation of taste, and not simultaneously a sensation of touch, which may readily be confounded with the former. We ought not, for example, to select substances having what is called a sharp, hot, or rough flavor, for those are qualities of a sensation of touch, as are the pungent odors spoken of in our previous article on the sense of smell. [See Smiths. Report for 1865.] Now, as the sense of touch is unquestionably spread over the entire cavity of the mouth, it will be granted that such substances as the above may readily lead to false conclusions respecting the extent of the gustatory area. In the second place, it is of importance to the proposed investigation that the substance submitted to proof should not be conveyed through the medium of the saliva, in which it is of course dissolved, (for only soluble substances are capable of being tasted,) to other regions of the oral cavity, and so be carried to that hinder part of the tongue, whose especial susceptibility to such impressions is admitted. Nor, for this, is any movement of the saliva necessary; as a lump of sugar placed at the bottom of a vessel of water will, in dissolving, gradually diffuse itself through the whole mass and reach the surface, so may the solution of a similar substance introduced beneath the tongue penetrate the salivary stratum which overlies every part of the cavity, and attain the nerves of taste situated elsewhere. If this be the case, it will of course be difficult to determine the exact seat of the sensation. On the other hand, we are always prone to refer the sensation of taste to the point to which, by virtue of the sense of place, (Ortssinn,) we refer the impression of touch produced by the contact of the substance with the ends of the nerves of feeling. In what way this perception of place which connects itself with the sensation of touch arises, has been discussed at large in our previous essay on the sense of feeling. [Smithsonian Report, 1865.]

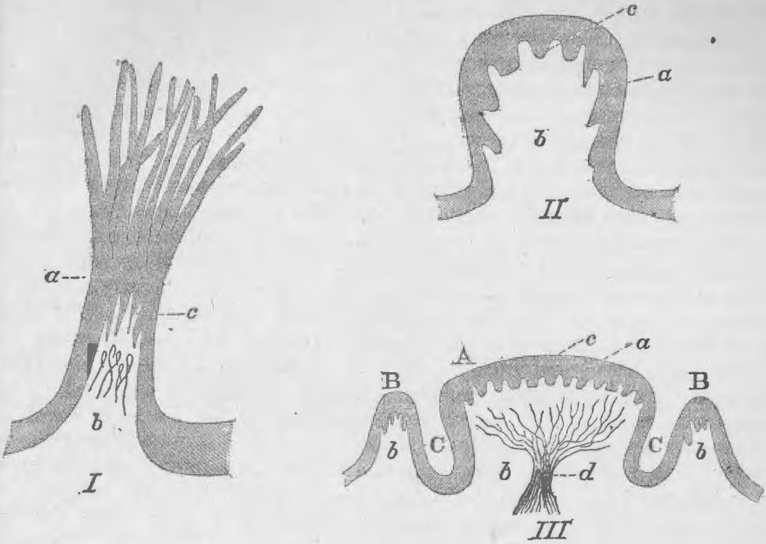
If the proposed inquiry be conducted with scrupulous regard to the precautions indicated, the conclusion, we think, can scarcely fail to be reached that only the hinder surface of the tongue is qualified to communicate sensations of taste. The trial may readily be made by every one for himself. Let a small piece of sugar, for instance, be laid on the end of the outstretched tongue, or, as rubbing will tend greatly to promote the sensation of taste, be rubbed thereon, and no perception of the property of sweetness will ensue. Still more convincing will be the result if an intensely bitter substance be employed. The same negative result is obtained if we make the experiment upon other parts of the cavity of the mouth, and with this view the parts to be examined may be touched with a small and delicate brush dipped in the concentrated solution of a sweet or bitter substance. If, in this way, a little drop of melted sugar be conveyed to the uvula which overhangs the orifice of the throat, no taste of sweetness is perceived, provided we take care, in the first place, that the tongue be not brought, by the involuntary retching which is apt to ensue from touching this part, into contact with the uvula, and, in the second place, wait not so long that the solution shall have time to be diffused gradually with the saliva to the underlying surface of the tongue. If the solution be rubbed in with the brush, the sweet taste will be more quickly produced; but even then it unquestionably arises, not from the uvula, but from the back of the tongue, and the more quickly because diffusion is promoted by the rubbing. We think, in short, that we propose nothing unreasonable to our readers, when we invite them to admit with us the circumscription of the sense of taste to the small tract of surface to which we are disposed to assign it.

The question next occurs, to what arrangements does this region owe its adaptation as an organ of sense? Were there here an apparatus as distinctly destined for the service of the sense as we find for that of sight in the eye, we should be absolved from the necessity of so nice an investigation respecting the local extent of the sense of taste as that just described. But this is not the case; on the contrary, we know not a single peculiarity of the region of the tongue empirically assumed as the province of the sense in question; none at least which can be assigned to it with any such manifest certainty as that with which the dioptrical apparatus of the eye, and the auditory apparatus of the ear, may be assigned to their respective functions. Unfortunately, after long debate, it is not yet quite agreed to which of the special nerves of sense is appropriated the function of taste. No less than three distinct pairs are directed to the tongue, each a fasciculus of those minute fibres which we have heretofore compared to the wires of the telegraph, and each springing right and left from different parts of the brain; these are the lingual branches of the widely distributed fifth pair, the glosso-pharyngeal and the hypo-glossal nerves. From our remarks elsewhere, it is sufficiently clear that neither by microscopic observation nor from any peculiarity can we conjecture the distinctive operation of such a nerve; just as it is impossible for us to infer whether a conducting wire, when we observe it at any intermediate point, is destined to set in motion the hands of a clock or the index of a telegraph. Anatomical research, however carefully it may explore the divarications of each nerve in the tongue, leads us here to no definite decision; as well because the local boundaries of the sense of taste are not established beyond doubt, as because many parts are at the same time supplied by more than one nerve, and because, moreover, that nerve which is directed preferably to the hinder part of the dorsum of the tongue, and therefore has the greater probability in its favor, is distributed also to the entrance and neck of the throat, surfaces to which we know the sense of taste is denied. There remains, then, only the physiological experiment from which to await a decision; and this also has its difficulties, though less in the realization than the interpretation of its results. It consists in dividing one of the three nerves, and in this manner intercepting all communication between its two extremities in the periphery and the brain, thus, of course, as effectually preventing the transmission of any current by which a sensible image may be formed, as the communication between two telegraphic stations would be intercepted by a rupture of the wires. After the division of either of the three nerves in a living animal, it remains to try whether the latter still possesses the power of tasting, for should it appear that, upon the division of any particular nerve, the faculty of the tongue to communicate impressions of taste on being touched with some intensely flavored substance is wholly lost, we shall be justified in concluding that the nerve on which we have operated was the vehicle of those impressions. But here lies the difficulty, for how are we to verify the absence or presence of the sensation in the animal? The animal wants speech to indicate it, and an impression furnishes us with no direct objective token of its existence; resort must, therefore, be had to circuitous procedures, in using which much circumspection is requisite. We should infer the loss of taste, if, after the operation, the animal devours, without signs of repugnance, ill-flavored aliments which in its normal state were constantly refused. On the other hand, if such substances are rejected, we shall be authorized to infer with more certainty the persistence of the sense of taste, when due care has been taken that the nature of the substance is not recalled to the animal by its effects upon some other sense than that which we are investigating. We should, therefore, employ in the trial no substance which, through its characteristic form is familiar to the animal's eye, and as little one which is readily known by its peculiar smell; nor is a substance to be chosen which, through the medium of the nerves of touch in the tongue, produces a sensation of smarting or roughness, so commonly confounded with impressions of taste; finally, we would, in this inquiry, discard anything to which the animal is altogether indifferent. Only such articles, therefore, are free from objection and suitable for a decision of the question as those whose properties can be made sensible to the animal through the sense of taste alone; and none better answers the purpose than a decoction of so bitter and repulsive a substance as colocynth presented in milk. If this mixture be offered to the unwounded dog, allured by the sight of the milk and without suspicion of the foreign substance, he will greedily apply himself to what he supposes to be a well-known dainty; but let a drop once pass the seat of taste, and no sign of disgust will be wanting which a dog can manifest. If, now, we divide one of the three nerves, the glosso-pharyngeal for instance, which chiefly supplies the hinder part of the tongue's surface, and find that the dog devours the milk, notwithstanding its bitter ingredient, with accustomed avidity, we may safely infer that the divided nerve was the channel of the sensation of taste, especially when, after a division of the two other nerves, persistent refusal on the part of the animal has testified a perception of the nauseous admixture.

Now, this has been tried with seemingly decisive results: the most careful experiments have uniformly led to the conclusion that the true impressions of taste can only exist when the glosso-pharyngeal nerves are intact; while on the other hand it has been shown with great certainty that the filaments of the hypo-glossal nerve pass to the muscular fibres of the tongue, and by conveying to these the motive current from the seats of the will in the brain, communicate to the organ the power of motion; that, further, the lingual branch of the fifth pair furnishes to the tongue its sensations of touch, thus notifying us of the form, size, weight, softness, or solidity, as well as the temperature, of whatever is introduced into the mouth, and sometimes, too, giving occasion to those perceptions of mordacity or roughness which we are apt to confound with the genuine impressions of taste. We shall not here enter into a discussion of the importance of the service rendered by the two last nerves; our good readers will, perhaps, take the trouble of reflecting for themselves upon the manifold disadvantages under which we should lie in the affair of eating and drinking; nay, how impossible in some cases it might prove, if our tongue possessed not the faculty of moving voluntarily in all directions, nor its exquisite delicacy as an instrument for examining the quality of all substances introduced into the mouth. We must also be excused for not attempting to give a detailed anatomical description of the true nerves of taste, convinced, as we are, that to the generality of readers such information would be of little avail. We might, indeed, have contented ourselves with assuring them that to a certain nerve, called the glosso-pharyngeal nerve, we owe the sensations of taste, but thought best to present to them an explicit statement of the manner in which this fact has been arrived at, because our object in these essays is rather to interest the laity in the learning of scientific methods than in unprofitable details. The result of these methods in evincing the glosso-pharyngeal nerve to be the vehicle of the impressions of taste, besides the experiments on animals above described, rests also on some scanty observations made upon man himself; for which, but with extreme rarity, a morbid degeneration or even destruction of the nerve in question has afforded opportunity.

The reader will no doubt willingly dispense with a critical discussion of the dissenting opinions of certain physiologists who regard the lingual branches of the fifth pair as partially, if not exclusively, invested with the functions of taste. The fact above mentioned that the glosso-pharyngeal nerve, as its name implies, sends filaments to the pharynx as well as the tongue, neither furnishes an objection to the results obtained by experiment, nor affords a ground for the conclusion that the parts of the mucous membrane of the throat in which its fibres terminate possess also the sense of taste. It may easily be conceived that only those fibres of the nerve passing to the tongue are provided at their inner and terminal extremities with the indispensable apparatus for the reception of the impressions and their conversion into the sensations of taste, while the rest are simply adapted to the transmission of the sensations of touch. We must content ourselves with this as a conjecture, for, unfortunately, it has not been found possible to trace with distinctness the filaments of either of the three nerves to their utmost extremities, and to discover there any special structural arrangement. We shall point out only the following peculiarities of the mucous coat of the tongue. Every one must have observed either in himself or others, or else in the tongue of the ox when it has been prepared for food, that the upper surface is not smooth, but rough; though, in man, the unevenness is so slight, that to the unaided eye this surface presents a velvety appearance, while in the ox it is so rough, both to sight and touch, as to resemble a rasp. Each of the little projections which occupy it appears under the microscope as a very composite organ, on the whole of a quite regular form. We distinguish, according to their outline, three sorts of these projections: the *fungiform*, the *filiform*, and the *circumvallate*, to all of which the general name of papillæ is given. The first are found especially on the upper surface on the point of the tongue; the second are distributed over the whole of the member; the third occur only towards its base, where they are arranged upon its surface to the number of from ten to twelve in the shape of a V. If the tongue be stretched as far as possible out of the mouth this V and the flattened eminences which form it, placed at intervals and surrounded by a sort of foss, may readily be seen. A more exact description of these three sorts of projections will not, we think, be without interest.

As they are merely elevations of the outer coat of the tongue they consist of the same two membranes with that coat, viz: an outer covering, called the epithelium, and the proper mucous membrane. The epithelium consists of cells multifariously superposed on one another, the uppermost layers of which become dried into flat and angular little scales, while the lower are moist and roundish. By the use of the tongue the former are continually worn off, and may be observed with the microscope as a constant ingredient of the saliva, which they render turbid; to repair this loss new cells are always forming below. The layers thus constituted are destined undoubtedly to furnish protection to the underlying mucous tissue which contains the fine and easily wounded vessels and nerves, just as in the external skin the *epidermis*, which is similarly composed, serves as a protective covering to the cutis. The accompanying figures, representing a section of three sorts of papillæ magnified, may serve as the ground-



work of a more exact description of each. In figure I is presented the section of a filiform; in figure II that of a fungiform; and in figure III that of a circumvallate. In all of them the part designated by *a* represents the epithelium, consisting of squamous and cellular layers; that marked *b* the underlying mucous membrane. It will be seen at first sight that the epithelium in neither form invests the subjacent mucous membrane as a glove does the finger, so that the outer surface of the former shall present an exact impression of that of the latter. In the filiform papillæ it will be observed that the epithelium has a number of long antenna-like excrescences, giving the papillæ the appearance of a brush, while in the others this covering is externally smooth, notwithstanding the inequalities of the underlying membrane *b*, whose surface exhibits in each example a number of successive and rather narrow eminences of a sugar-loaf or conical form. What purpose the antennæ of the first form subserve it is impossible to conjecture; but, as regards the regular elevation of the proper mucous membrane, their presence alike in each of the three kinds of papillæ, as well as the circumstance of their existence likewise in the outer skin, leads to the conclusion that wherever they occur, whatever otherwise their diversity, the physiological signification which they bear must be the same. Now, as the microscope shows us, that, as regards the outer skin and in part, at least, the mucous membrane of the tongue, the finest branches of the blood vessels and the terminal extremities of the nerves are found in these little eminences, the inference is obvious that this peculiar formation of the surface is imposed on account of the organic contents of the eminences, providing, as it does, suitable receptacles in the periphery for the ends of the vessels, and especially the nerves, and securing their regular distribution over the surface. We may conceive that it behooved that the ends of vessels and nerves should closely approach the outer surface at a regulated distance from one another, which purpose indeed might have been attained had these minute organs penetrated at certain intervals from the mucous membrane into the loose strata of the epithelium; but then they must have wanted a sufficiently stable involucrum to protect them against the pressure and continual wasting of the shifting elements of that covering. Now, such protection is supplied by the enveloping projections of the mucous membrane which enter into corresponding cavities of the epithelium. We are aware that this teleological mode of considering anatomical arrangements as directed to a pre-determined end, and contrived with a view to attaining that end, will at present be received in many quarters with entire repugnance; wherefore we shall not here further insist upon it. Enough, if the preceding example should serve to show how well this manner of viewing the subject is adapted to lead us to an understanding of such arrangements. The clearest method of explaining natural as well as artificial mechanism is, in our opinion, that which bases itself upon teleological principles; how far the employment of teleology for purposes of demonstration may be justified is another question.

Returning to a consideration of the papillæ we remark that, for a proper conception of them, attention should be especially directed to the delicate structure of the small secondary projections of the mucous membrane, with a view to seeking therein the ends of the nerves and apparatus of sensation. But hitherto all efforts have been fruitless, nor has research in this direction been rewarded with the slightest degree of certainty. Neither in the filiform, nor fungiform, nor circumvallate papillæ has success attended the attempt to explore the ultimate destination of the nerves which direct their minute filaments with extreme intricacy towards the surface, to follow them into the interior of the superficial projections of the mucous membrane, and there detect a difference of arrangements which might guide us to a decision respecting a difference of functions. However probable the supposition that the filiform and fungiform papillæ contain the ends and apparatus of the nerves of touch; that the circumvallate, on the other hand, which occupy the centre of the seat of taste, and are outwardly so strikingly distinguished, contain those of the nerves of taste; yet we are unable to furnish anatomical evidence for that supposition, or indeed to establish it conclusively through physiological experiments. Even the peculiarity of external form of these latter papillæ is a riddle; we cannot explain what purpose is subserved by their division into a roundish central elevation A, and an annular periphery B, separated from one another by a distinct foss C. If we ventured to assert that this foss furnishes a sort of reservoir in which the solutions of sapid objects are received in order, by longer contact with the sensitive surface, to take more full effect on the extremities of the nerves, while the passage of those solutions over a free surface might be too rapid for distinct perception, we should only be advancing an empty conjecture for which no proof could be alleged. Nothing remains for us, therefore, but to wait until, by persevering and patient observation, some deeper insight into the secret mechanism of the organs in question shall be vouchsafed, and material be thereby furnished for physiological interpretation.

While it fares so unsatisfactorily with our knowledge of the organs of taste, we are, unfortunately, not much better off as regards our knowledge of the nature and conditions of the external influences which call forth a sensation of taste of whatever quality. We know well what substances produce such sensations, and bear in mind a correct idea of the kind of impression connected with their use, so that we again recognize the substances by that impression, or, even without their presence, easily recall, in imagination, the sour, or sweet, or bitter qualities which distinguish them. But this knowledge, in which the epicures must be recognized as graduates, though the basis of our practical use of the sense of taste, avails us little or nothing to a mastery of the physiological theory. We know even more: we know accurately the physical and chemical properties of the greater number of sapid substances, and are dumb notwithstanding to the capital question, what property is it which makes a substance sapid, and qualifies it for the excitement of such or such a sensation? We know no single property common to all sapid matters which can be even remotely identified as the cause of their operation on the nerves of taste, and in which we can seek the source of their peculiar action. To illustrate our incapacity of arriving by physical or chemical analysis at any decisive results on this subject, we shall cite one or two examples. Sugar may serve as the representative of sweet substances; we know that it is composed of twelve atoms of carbon, hydrogen, and oxygen, and how it is affected by all chemical agents; we know, also, its physical properties, such as solubility in equivalent proportions, requiring here no further discrimination. How is it now that this substance, on its contact, in solution, with the mucous membrane of the tongue, produces an excitation, whether chemical, thermal, electrical, or mechanical, which gives rise, by its action on the ends of the nerves, to that specific modification of the nervous current to which an appropriate sensation of taste instantly responds in the brain? We may well suppose that the sugar does not take effect on the nerves hidden under the epithelium until imbibed by the latter, and thus brought into proximity with their extremities; but even if it penetrated into immediate contact with them, in what respect is the solution of the question advanced? If we bring dissolved sugar into contact with the nerves of the muscles not a fibre stirs; a sufficient proof that sugar is in itself no "adequate" irritant of the nerves. What, then, qualifies the sugar for excitation of the nerves of taste? The obvious answer is, the presence of some special arrangement at the ends of these nerves. But of what sort is it? How and by what means does the sugar operate upon it? How, through those means, is a nervous excitation produced? This is the riddle to be solved, but it offers at present so much resistance to the sharpest powers of divination that we decline any attempt to meddle with it. The matter becomes still more inexplicable when we consider that, in the first place, sugar is not the only substance that tastes sweet, but that a multitude of other substances, in no respect resembling it either physically or chemically, produces the same sensation in the organs of taste; and, in the second place, that still other substances, which are most nearly related to sugar, having even the same composition, generate either no sensation at all or produce one of a wholly different character. What, for instance, have sugar and a combination of lead with acetic acid in common that both should taste sweet? What sulphate of magnesia and quinine that both should taste bitter? What distinguishes sugar from starch that the former should produce a sweet taste, the latter no taste at all? No answer can be given, and we should but freight our page with useless verbiage did we give a list of all the substances having properties thus comparable. As regards other senses, that of sight, for example, we are in a better position. True, that even here we do not know in what consists the operation of light on the extremities of the optic nerve, whence result their excitation and mediately the sensation of light. But we do know, in the first place, that at those extremities there exists a wonderfully complicated apparatus in which we can recognize the intermediary between light and nerve, through whose agency the former becomes an adequate irritant of the latter; and we know, in the second place, by what momentum of the outward agent, light, the quality of the sensation is determined; it is practicable to state mathematically the degree of momentum adapted to each sensation. The undulatory movements of the luminous ether produce the excitation of the nerves of sight; the velocity of those movements qualifies the sensation so as to produce the colors which we see. A definite amount of this velocity is universally necessary for the production of the excitation; we are able to specify the smallest number of oscillations in a second of time which cannot be diminished, the largest number which cannot be increased, without producing in either case an extinction of the effect of the waves of light on the visual nerves; we are able, also, to state with entire certainty the unalterable number of oscillations respectively pertaining to the perceptions to which we give such names as red, green, yellow, or blue. So it is, also, with regard to the sense of hearing; what has just been said of the waves of light and their relations to the organs of vision and impressions of color may be transferred, with only verbal changes. to the waves of sound which produce the excitation of the auditory nerves. Both these latter senses we propose to examine as exactly as possible hereafter, when the advantages which we possess for their analytical study in comparison with the sense of taste, and which are here only intimated, will be more thoroughly considered, and, we hope, more clearly evinced.

However unknown to us the properties which give to substances the power of producing sensations of taste and the way in which they operate on the nerves of that sense, it is, at least, some compensation that we know to a certain extent the conditions under which a substance empirically recognized as an object of taste exerts its specific power, and the circumstances on which the intensity of the impression depends. First stands the fact that a substance can produce no such impression unless it reaches the seat of this sense in a fluid form; if of a solid nature it must at least be first dissolved in the saliva. This condition results from the necessity, which was above provisionally assumed, that the sapid substance shall penetrate through the coat of epithelium to the mucous membrane. A solid body, even reduced to the finest powder, could not make its way through the closely compacted cells of which the covering of the tongue consists; but in solution it will be imbibed first by one and then by another stratum of these cells, until it finally reaches the underlying and sensitive surface. Now these successive imbibitions may be supposed to imply the lapse of a sensible interval of time, and yet experience teaches us that no interval can be appreciated between the moment of contact with the tongue and that of the ensuing sensation. Still, and in spite of this apparent contradiction, we must insist that no alternative remains for us but to accept this theory of a gradual transmission, seeing that it is supported by the admitted necessity of solution for producing sensations of taste, and no other supposition is possible. Moreover, we have no knowledge of the rapidity with which such transmission is accomplished, so that there is at least no proof that it may not be sufficiently great to render the interval in question quite inappreciable.

From what has been said it results that a body which is insoluble in water, and cannot therefore fulfil the above condition, is incapable of yielding a sensation of taste. We must take care, however, not to reason conversely that solubility in water is the actual quality which in itself fits a substance for the excitation of the gustatory nerves. That this is not the case obviously follows from the fact that many substances readily soluble in water are tasteless—nay, that water itself is so. Were solubility as directly the condition of taste as the undulations of the luminous ether are the condition of sight, a substance to be more sapid—that is, capable in smaller quantity of conveying a more intense sensation—should be the more soluble; but this common experience refutes. Extremely soluble bodies sometimes convey a very faint taste; others, soluble only with difficulty, an intense one. The degree of concentration of the solution requisite in general to produce a sensation of taste is very different with different substances; while some act in a very diluted state, others require a concentration nearly equivalent to saturation. It has been elsewhere stated that as regards odorous substances, a scale has been established by experiment showing the degree of impregnation of inhaled air, requisite in the case of different articles for the production of a sensation of smell; and in like manner it has been attempted experimentally to assign values to the different degrees of dilution which different objects of taste will bear, without thereby losing their property of gustatory excitation. The numbers thus found afford of course no absolutely exact valuation, but as a general appreciation they furnish in this respect a sufficiently accurate comparison of the different substances submitted to trial. We will not withhold from our readers some of the results. It would perhaps be scarcely supposed that sugar rates unfavorably in this scale of values—indeed, the most unfavorably of the compared substances as regards their relative capacity for dilution without loss of taste. Solutions of sugar which contain less than one part of sugar to 99 parts of water, no longer excite any perceptible taste of sweetness. Common salt bears a somewhat greater degree of dilution; the universal remedy for intermittent fever, quinine, remains perceptible under a much greater degree, while sulphuric acid (oil of vitriol) takes rank before all; water, in which only $\frac{1}{10000}$ part of sulphuric acid is dissolved, still tastes perceptibly sour. Acid substances, as many readers both male and female will be able to certify from experiences gained in cookery, most readily impart their sapid qualities. On what conditions these surprising diversities depend, it is beyond our power to explain.

With one and the same body, the *intensity* of the sensation of taste which it excites increases with the progressive concentration of the solution; a law which needs no other proof than daily experience. As regards the comparison of intensities, it will be well to remember what was elsewhere said on sensations in general, and especially on those of touch; we have no measure by which we can estimate an impression of taste, so as to give it an exact numerical expression. If we test, one after the other, two solutions of sugar of different concentration, we cannot, if the difference is slight, decide which is the sweeter; we cannot say that one tastes half, twice, thrice, or tenfold as sweet as another with which it is compared. Though this by the way: the intensity of the sensation depends not alone on the degree of concentration of the solution, but on several other circumstances, whose agency is in part susceptible of explanation, in part not so. It is easily understood that the strength of the sensation increases in proportion as the surface is larger on which the solution operates, and the time longer during which it is in contact with that surface. Just as of two bodies of equal weight that seems to us to be heavier which presses on a larger surface of the skin, and thus gives excitation to a larger number of the nerves of touch, so the same solution of sugar will seem sweeter, if diffused over the whole area endowed with the sense of taste, than when merely a drop of it touches a circumscribed part of the mucous membrane. Thus the intensity of the sensation increases with the sum of the nerve fibres simultaneously excited, because the single impressions seem, in conception at least, to be in some measure combined with one another. The more intense effect produced by prolonged contact of the object with the tongue may be inferred to result from the penetration of greater quantities of the substance to the nerve extremities of the mucous membrane; and for this, too, an analogy may be found in the operations of the sense of touch. A hot surface seems less hot on transient than prolonged contact with the skin, from the circumstance, in the first instance at least, that time is wanting to conduct the particles of caloric in sufficient quantity through the cuticle to the ends of the underlying nerves. One of the most essential conditions for increasing the intensity of a sensation of taste consists in the friction of the solution against the surface of the tongue, or of the latter with the solution against the hard palate. This resource comes into play habitually in the act of taking either solid or liquid food, but we more particularly avail ourselves of it in tasting, with a view to a more accurate judgment of the flavor of any substance. In this case we do not content ourselves with laying the object of which we would form a judgment upon the tongue and quietly awaiting the sensation, but bring it by an incipient movement of deglutition to the hinder part of the tongue, and press this in turn against the hard palate. It is at the moment of this contact, produced by a movement which is indispensable in the act of swallowing, that the most vivid sensation arises, as every one may convince himself who pays attention to what occurs during the process of taking food. For further confirmation of the fact the following experiment may be practiced: Let the tongue be stretched out, with the mouth widely opened, so that the former can no where strike against the roof of the cavity, and a drop of dissolved sugar or vinegar be placed upon its surface, only a very weak sensation will ensue, with an instinctive effort to press the tongue against the palate, because we are unaccustomed, except by this means, to conduct the operation of tasting. The sensation will instantly become stronger if, with the finger or a brush, we rub the solution upon the mucous surface of the tongue. Here, also, we are at a loss for any certain explanation; we can only remotely conjecture how the means spoken of serve to promote and intensify the excitation of the nerves of taste. It may be that the pressure and rubbing facilitate the intermixture of the solution with the salivary stratum which covers the membrane and its penetration among the cells of the epithelium, or that the pressure applied to the extremities of the nerves renders these more sensitive and excitable. This latter conjecture, we confess, though received with favor by some, has, in our opinion, little probability. So long as we know nothing of the means or the mode by which substances communicate sensations to the nerves of taste, all hypotheses on this subject must be air-built speculations. How a mechanical force, a compression of the nerves, can heighten their excitability, must be mere matter of conjecture, and finds for its support no analogy in the action of other nerves. We must be content, therefore, to consign this, with the many other questions which we have been obliged to leave unanswered, to the category of riddles, for whose exact solution we can only look to a perhaps distant future.

We know from experience that the sense of taste not only possesses a very different degree of delicacy in different individuals, but that, even in the same individual, its delicacy is sometimes more, sometimes less exquisite. The causes of this difference are not, in all cases, easily shown. It cannot be denied that a different degree of functional activity of the organs of taste may be innate; but to what parts of the organ the difference is attributable and what properties adapt this or that part to the reception of a keener impression, and its conversion into a more distinct sensation of taste, cannot in the present state of our knowledge be determined. It is but seldom that we observe in the organ of taste, the tongue, those gross and obvious defects which might lead us beforehand to infer an obtuseness of the sense, such, for instance, as a thick and corneous condition of the epithelium, perceptible rugosities, &c. In most cases of difference of fineness of taste in different persons, the cause is to be sought, not so much in congenital or casual deviations of the apparatus, as in diversities of cultivation, of education of the sense. It is a remarkable fact that, through judicious practice directed to that end, the operations of all our senses may be improved to an extraordinary extent, and the way in which this refinement is brought about by diligent and heedful usage, presents one of the most interesting questions of physiology. Is the mechanism of the organs of sense improved by use, or is it only that, with an unchanged condition of the original mechanism, more facility is acquired in understanding their operations? In other words, are material changes produced by use in the apparatus of the senses, or is it the immaterial principle of our nature which derives advantage from that use? Probably, both suppositions are, in a certain degree, true. It would be difficult to adduce direct proof of the improvement of the proper apparatus of the senses, but analogies, drawn from other physiological apparatus, give countenance to its possibility. A muscle becomes so changed through strenuous use, that not only are its powers of performance increased, but a higher activity of nutrition supervenes, and the number of its fibres, on which depends its strength, is also increased. The blood secretes more material applicable to the formation of such fibres in employed than in unemployed muscles; a muscle, indeed, precluded from all activity, ceases to receive nourishment and perishes through inanition. Perhaps also the *quality* of the nutriment supplied is improved by the exercise of the muscle, so that, without actual increase of the fibres, those already existing may be qualified for greater manifestation of power. Daily experience affords proof of the above. It needs but to compare the firm and prominent muscle in the arm of a smith with the flaccid, scarcely developed muscle in that of a recluse, the muscular leg of a chamois hunter with that of a sedentary artisan. But there is often to be seen also, in individuals of spare habit, in whom there is outwardly no striking muscular development, a surprising strength and energy of the muscles, which would indicate the enhanced nutrition of those organs arising from continued use. The muscles, then, being susceptible of this improvement by use, what should forbid our inferring that the working capacity of the apparatus of the senses may, in like manner, be nourished to a higher degree of perfection by habitual exercise? But whether the whole result should be referred to habitual use and exercise, is extremely doubtful; for those, at least, to whom soul and nerve apparatus are not identical. While it may well be believed that an organ of sense may be improved to such a point by material treatment as to be more sensitive to related impressions, it would seem impossible to explain by a simply improved mechanism the vastly increased delicacy of discrimination acquired by practice in distinguishing different impressions or different degrees of the same quality of impressions. If we consider the soul as an immaterial principle, which is indeed consigned to the material organs for all its intercourse with the outward world, and yet is so far independent as to be capable of a different cultivation through the employment of the same unchanged organs, we may also assume that it first gradually learns to interpret and discriminate the permanent processes in the sensitive apparatus, and will of course make the greater progress therein, the greater its practice. We are here venturing, however, into a province where no ordinary sagacity is requisite to find a sure route, and to pursue it without stumbling upon prejudices or deviating into by-paths. We return, therefore, to our immediate subject.

Examples of the degree of delicacy to which the sense of taste may be trained by use need not detain us. Every one may find them among his acquaintances, and few probably of my readers are without a knowledge of some connoisseur in wines, who, from the taste of the precious juice, will undertake to pronounce the year and vineyard which were privileged with its production. Though in such cases there may be often pretence or self-deception, yet it is certain that the sense of smell and even that of touch afford, for these trials, a degree of succor to the sense of taste; even common parlance recognizes the "bloom," the "bouquet," or again the "harshness," of certain objects submitted to the scrutiny of the latter; still there remains for the share of the sense in question enough to secure respect for its educational capacity. It is not, however, in different individuals only that striking differences in the delicacy of the sense of taste occur; they present themselves from time to time in one and the same individual, as the experience of every one will testify. Thus it happens, not unfrequently, that a severe catarrh migrates from the pituitary tunic of the nose to the mucous membrane of the mouth, and occupies the latter to such an extent that, in a scarcely exaggerated sense, all taste may be said to be lost. In this uncomfortable state, food excites only the impressions of touch, often improperly confounded with what is called a "clammy taste," though in truth no genuine sensation of taste exists; with the exception, perhaps, of that generated by intensely sour or bitter objects, which still is scarcely more than a counterfeit of the sensation excited by the same objects in a healthy condition of the organ. In the same way loss or depravation of taste is a very regular concomitant of many disorders of the stomach and bowels, manifested in some cases by hebetation of the organ, in others by perverted sensations, as when, for instance, all substances leave a bitter after-taste. We may remark, finally, that very intense impressions of taste blunt the susceptibility for succeeding and weaker impressions of the same quality, whence after partaking of a sour salad, the wine bibber may smack his lips over the sourest products from the vineyards of Grüneberg; while, on the other hand, the sensibility to impressions of another quality seems to be exalted, so that after the enjoyment of a sweet confection, the least acidity causes the best wine to taste like vinegar. For these facts we can propose only superficial and fragmentary explanations. As regards the bodily ailments above spoken of, it is safe to make of the more or less thickened and furred envelope of the tongue a scape-goat on which we may cast, in the first instance, the blame of the defective or vitiated sensibility to objects of taste. We may suppose, moreover, that in catarrh, exudations from the mucous membrane clog its tissues, and thus impair the irritability of the extremities of the nerves, or in some way embarrass the yet unexplained action of sapid substances on those nerves. But such an explanation or supposition does not carry us far. What really gives rise to the incidental perversion of the sensations in question, can scarcely be the subject of even plausible conjecture, so long as we know not the nature of that excitation which different substances produce in the nerves, so long as we are ignorant how a sweet or bitter taste comes to be generated. That after a sour substance, a sweet one tastes doubly sweet, the reader may be disposed to explain by the effect of contrast. This explanation we shall not in general contest, but most earnestly protest against the commonly received notion that a sweet and a sour taste are opposites of one another. Between two sensations of different quality, whether they belong to the same or different spheres of sense, there exists no *tertium comparationis*, and so nothing which will authorize us to regard them as opposites. It is just as unreasonable to consider a sour taste the opposite of a sweet one, as to attribute opposition to the sensations of a red and green color, or a shrill and a deep tone, as if the one stood in diametric relation to the other as a north or south pole. Could the two sensations be even assigned to external causes essentially antithetical, we should not be justified in transferring this character to the sensations themselves. These are incapable of all description; they admit not of measurement or division, and can as little be brought into the relationship of opposition as of numerical value.

We must not pass without at least a brief notice the *after-taste* or *tang*, although in truth its nature and causes are as obscure as those of the proper sensations of taste. This after-taste exists in a twofold manner. With some substances, the original sensation which they create continues of the same unchanged quality for a long time after they have been swallowed and been displaced on the tongue by other substances. With others, again, after the retreat of the sensation originally excited, another supervenes of quite different quality. Thus there are bitter substances whose taste is not to be got rid of, as it is commonly expressed; and on the other hand, sweet substances which create a bitter after-taste. When this after-taste is of like quality with the original sensation, it is not easy to decide whether it is of a purely objective nature, or in part, at least, subjective; that is to say, whether the action of the substance which generates the primary sensation itself persists as long as the after-taste endures, or whether, for some reason, the excitation of the nerves, or the activity of that central apparatus which operates directly on the sensorium and determines the nature of the sensation, continues even after the disappearance of the substance from the environs of the nerve extremities. Of the last there is no existing proof; the objective nature of the after-taste is, on the other hand, readily conceivable and capable of explanation in different ways. It is very possible that certain substances, having once penetrated into the mucous membrane, cling more pertinaciously than others to its structural tissues, without being removed, by absorption or chemical decomposition, from the vicinity of the nerve extremities on which, therefore, they continue to operate. The probabilities in favor of this origin of the after-taste are greater, the smaller the quantities of the substance necessary to produce the sensation. In the second place, it may well be supposed that there are substances capable of affecting the nerves of taste through the blood; indeed, it is certain that many "subjective" sensations of taste, which, from time to time, present themselves without any external cause, are in reality objective in the sense that substances conveyed from the bowels into the blood have been in such cases carried by the latter to the mucous membrane of the tongue, and thus reach the sensitive extremities of the nerves of taste. The proof of this lies in the often-observed fact that, when intensely bitter substances have been swallowed in the shape of pills so well enveloped that they pass over the tongue without exciting the characteristic taste, that taste will nevertheless arise in the mouth after a certain time. As regards the second sort of after-taste, where its quality differs from that of the primary taste, we are again left in doubt how far it is objective, how far subjective. It is very possible that certain objects of taste suffer, within the mucous tissues, a rapid change or chemical decomposition, and in this altered condition affect the nerves of taste in a different manner than at first. But it is also conceivable that the different after-taste results from a spontaneous reaction of the nerve apparatus; a supposition, however, which admits neither of proof nor closer examination.

In like manner with the impressions of touch and smell, of which we have elsewhere treated, those of taste are regularly and unavoidably associated with *ideas* which mankind in general are apt erroneously to identify with the impressions themselves. On this head, our previous discussions authorize us to be brief. With the impressions of taste is immediately connected the idea of an external *object* as cause of the sensation, and with so little consciousness of the distinction between the sensation and the idea that we believe ourselves to be directly tasting the object as such, and impute to it the qualities of the sensation as a property. Thus every one speaks of sweet and sour substances as if the sweetness or sourness were a property of the substance itself, just as we speak of red and blue objects in the meaning that the color is a property of the object. Most probably we arrive at the idea of an external object as cause of the sensation of taste not through this sensation itself, but through the accompanying impressions of touch, which simultaneously enable us to form the idea of the *place* at which the gustatory impression takes effect. With sensations of taste are also connected ideas of pleasantness, unpleasantness, nauseousness, &c., which are in like manner erroneously confounded with the sensations. An impression on the organs of sense cannot in itself be pleasant or unpleasant. Further, it is often not the immediate gustatory impression which calls up the idea, for this idea may take its shape from mental associations previously established, or in part from simultaneous sensations of taste or smell.

The above is all that we have felt authorized by the present state of our knowledge respecting the physiology of the sense of taste to impart to the reader as worthy of his confidence. We might here desist, but it is, perhaps, proper to offer a few remarks in regard to the practical uses to which the sense of taste is adapted, even if the first considerations which present themselves, as was the case when we formerly treated of the sense of smell, may seem calculated to disparage its value as an endowment of the human or animal economy. In this point of view, it is a matter of indifference whether we ask, of what advantage is the sense of taste? or what was the design in endowing the organism with that sense? The answer to both forms of inquiry rests essentially on the same principles. It is customary to regard the sense in question as a *sentinel*, so to speak, of the digestion, stationed at the portal of the alimentary passages, to challenge the various articles which present themselves for admission, and to enable us to discriminate the hurtful from the wholesome. Nor is this view incorrect, only it is necessary, first, to assign the limits within which this sentinel may be trusted; and, secondly, to inquire how far its function is an independent one. It would certainly be disastrous for nutrition and health if we committed ourselves blindly to such a monitor as this; death from inanition or poison must lie everywhere in wait for us, if we resigned ourselves without reserve to its indications. The sense of taste is itself indeed no sentinel; the quality of a sensation of taste can, of itself, in nowise disclose to us whether a substance is wholesome or unwholesome, seeing that there are many tasteless substances which are the former, and not a few of the latter which are well flavored. The sense of taste is in this respect only like the indications furnished by a clock; we must understand the signification of the numbers of the dial plate and the movement of the hands if we would derive from the clock information as to time; so we must learn by circuitous experiences to interpret the different qualities of the sensations of taste, if we would avail ourselves of them in the diagnosis of food and poison. It is difficult to say how we are determined to the choice of our first nourishment; an impulse wholly enigmatical urges the new-born child to its mother's breast, nor must we imagine that we make any progress towards an explanation when we call this impulse an instinct; that is only a word by which we supply our want of a clear idea. But whatever the nature of the impulse, a sensation of peculiar quality is imparted by the milk of which we are thus led to partake, and this quality, impressed upon the memory, serves us ever after for the recognition of that particular nutriment. And so with all other means of nutrition, whether instinct or experience may have taught us to recognize them as such; the sensations excited by them impress themselves upon us, and, with each, an idea of the nature of the corresponding object. Thus we learn by the sensation to distinguish the objects, but in nowise do we learn from the sensations the significancy of those objects as regards life and health. When we partake of food, the previously instructed sense informs us of the kind and quantity of its sapid ingredients; it is upon our own experience or that of others that we found our judgment of their nutritious or noxious character. Unfortunately for the epicure, the antagonist ideas of pleasant and distasteful can in no manner be brought to conform absolutely with the two categories of food and poison; many a culinary preparation of simplest flavor is an excellent nutriment; many a delicacy, which tickles the palate to an exquisite degree, is either innutritious or positively hurtful. Finally, it is to be remembered that among the safest viands as the subtlest poisons there are some which are quite insipid, to which, therefore, the vigilance of the sentinel palate can by no possibility extend. These patent truths it is unnecessary to re-enforce by multiplied examples. What would it avail, indeed, to prove that a plain milk-porridge as much surpasses a truffle-pastry in nutritious properties as it falls behind it in savoriness? We should make thereby as few proselytes as did the barbarous reformer who once set a whole community in ferment with the words, "Beer is poison."

IV.—THE SENSE OF HEARING.

We enter now upon a sphere of sensation in which we feel that we have a surer footing than when dealing with the enigmatical senses of smell and taste, because here we encounter an external excitant accurately known as regards its nature and laws, and this nature and these laws are already more or less familiar to the contemplation of a large number of readers. What perception of the senses is there which takes a deeper and stronger hold upon our inmost being than sound in its endless modifications and harmonious combinations, which, in its inexhaustible interchange of tones, is capable, if we may be allowed the expression, of more delicate and diversified shadings, especially when it reaches us in the living form of speech? Even that small and unhappy number of human beings in whose inelastic natures music "finds no echo, strikes no responsive string"—even these are the willing slaves of sound, without whose availability as speech the intellectual commerce of mankind would indeed be reduced to narrow bounds and wretched expedients. If we consider the animal world, we recognize the same wonderful force, the same essential office of sound in hundreds of instances. The tones produced by the small fibrous tendons of the throat thrown into vibration by the breath, whether it be in the unmodulated neigh of the horse or the resonant song of the nightingale—these tones, which, in some respects analogous to the speech of man, afford means for a certain degree of intelligent intercourse between animals, are utterances to which we often assign no meaning, or, in our narrow prejudice, altogether disregard as the expression of a psychical emotion. We are very far from intending to write a panegyric on the power of sounds, or to analyze with poetic circumstantiality the susceptibility of sensitive minds to the language of music; there needs but brief exposition to secure for the sense of hearing that high interest which naturally pertains to it. For without the sense of hearing, there is no sound, as without the sense of sight, no color.

We may safely assume that at least every educated individual has formed for himself some idea, however superficial, of the causes of sound; has learned, for instance, to consider the mighty tones of the organ as proceeding from the regular and tremulous pulsations of the air-columns enclosed in the pipes, which pulsations arise when the air compressed in the bellows is directed in a current towards the openings of the pipes; that the penetrating tones of the violin result from the vibrations communicated to the stretched strings by the bow, as similar vibrations are produced in the strings of the piano when struck by the hammer; that the tuning-fork resounds when its elastic branches of steel are thrown into vibratory motion by a blow against some resisting surface, &c. These visible vibrations, as the source of sound, are too obvious to allow us to suppose that there is any one not familiar with the fundamental facts of the generation of sounds. But though our task of exposition as regards the objective excitant of the nerves of hearing be on that account materially lightened, and we may limit ourselves to a discussion of the special relations of certain properties and modifications of this excitant to the auditory organ, yet we believe we do not err in imputing to many and perhaps most of the laity * a radical misconception in which they are only the more fortified by the received explanations on the nature and causes of sounds. This is a misconception so nearly unavoidable, so entwined with all our ideas, and, indeed, so much sanctioned by the language even of science, that it is a difficult matter for physiology to evince it to be such and to establish right conceptions in its place. Many will be able to surmise what misconception we mean, since we have already had occasion, in treating of other senses, to enter the lists against it; but, at the risk of being counted tedious, we must here combat it anew, and much fear that when we come to consider the sense of sight we shall not be able to spare our readers a repetition of our efforts to eradicate it. Every one says, and most believe, that the vibrating string of the violin or the pulsating air-column of the organ pipe itself resounds; this is a radical error. When we said above that without the sense of hearing there could be no sound, we did not mean that if no ear were present an externally existing sound would not be perceived, but, in the strictest sense of the word, that without the ear, without the excitable nerves of hearing, no sound at all would exist. The string only vibrates, it sounds not; the sound is not conveyed through the air from the string to our ear, and through this to our consciousness; *the sound originates in ourselves, is the sensation,* and hence an action of our own soul, which is the result of the vibrations of the string, but has nothing whatever in common with them. The string, by its oscillations, throws the adjacent particles of air into a corresponding oscillatory movement, this movement is propagated in the form of a wave from particle to particle, and when the particles next to the ear receive the undulatory impulse and are set in motion, they communicate that motion to what is called the tympanum of the ear, which is thus in turn made to vibrate; the vibrations of the membrane just mentioned are now transmitted through certain bones of the ear

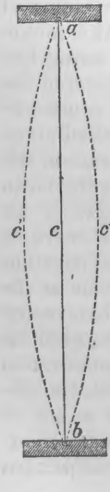
* A term used by some European writers, French as well as German, and seeming to imply that the profession of science is regarded, in Fourierite point of view, as a sort of priesthood. Being convenient, it is here retained. T. R.

to a small reservoir of fluid, in which fluid again the undulatory movement is reproduced, and that peculiar excitation of the extremities of the auditory nerve bathed by this fluid is exerted, which hurries to the brain the mysterious current so often spoken of, and there, through the terminal apparatus of the nerve, gives rise to the phenomenon recognized as a perception of sound. Thus long is the chain of successive movements between their origin in the vibration of the string and their termination in the inscrutable process by which the result is communicated to the mind. As far as the nerves we can analyze these movements through their entire course: up to the tympanum with great exactness, inside of the ear with less precision; but the movement within the nerve and its result, the sensation, are two riddles, of which only the first affords some glimpses towards a solution, the second is, perhaps for all time, shrouded for us in impenetrable obscurity. We are just as little in a position to define the nature of a sensation of hearing, or any intrinsic and essential characteristic pertaining to it, as we are in regard to an impression of any of the senses; the utmost we can do is to designate by commonly received names the qualities of the sensations generated by certain causes, upon the supposition that the sensation itself is known to every one from experience and an idea of its quality impressed upon the memory. If we would more exactly describe the sort of sensation which we call "high" or "shrill," instead of any distinctive token of the sensation itself, we assign the nature and specific properties of the external cause which excites it; we designate a sound as of certain "pitch," according to the number of vibrations of the body producing it, or as of a certain tone, according to the kind and peculiarities of the object or instrument from which it proceeds, because we are, for the most part, incapable of giving an accurate physical account of the forms of the undulations on which the modifications of the sound are conditioned. The same holds good respecting the "strength" of the sensation; the sensation itself admits not of measurement, but we can measure the magnitude of the vibrations or the force of a mechanical arrangement for producing them. In general, and in relation to each of these qualities, the sound evades every attempt to analyze it, however distinctly the sensation may present itself to our consciousness, however vividly we may be able to represent it to ourselves without the presence of its exciting objective cause, however accurately a practiced ear may seize the finest shadings of the height, the tone and the strength of different sounds. It was necessary to premise these explanations in order to reduce what is proposed by of the following exposition to a proper standard. It is undoubtedly a legitimate problem for physiology to investigate the nature and origin of sounds in the exact sense which we have indicated, but, for the solution of this problem, it is wholly unprepared; the physiology of the sense of hearing, of which we shall endeavor to give a brief and intelligible sketch, can teach us at present nothing further than the ways by which, and the forms in which, those motions of matter, which physical science has shown to be the cause of the perception of sound, penetrate to the auditory nerves; the acoustical significancy of the complicated apparatus with which nature has endowed our organism for receiving the impressions corresponding to those motions; and, lastly, the relation between the qualities of these external movements and the qualities of the sensations as indicated by certain epithets applied to them. We proceed to our task.

Deep within the skull, enclosed on each side by the so-called petrous bone, the auditory nerve spreads its peripheral extremities, secluded from all immediate contact with the air, which brings to us, in propagated waves, the sound-producing vibrations of bodies whether near or distant. Of possible ways in which pulsations of the surrounding air might be conveyed to the auditory nerve there are many; but one is suited beyond all others to this office, and alone affords, by an arrangement pre-eminently adapted to the conveyance of sound, an adequate guarantee for the arrival of even very weak undulations at the percipient apparatus of the ear Air-waves may communicate their impulse to all bodies, whether solid or fluid, on which they strike, producing in these bodies vibratory movements more or less intense according to the degree of their elasticity and other properties. They communicate themselves therefore to the external skin of the body, and might be transmitted from this to the underlying soft parts; thence again to the bones, and through these be finally propagated to the extremities of the nerves of hearing But this mode is so imperfect that, were the air-waves to reach their destination only in this way, our perceptions of sound would be limited to cases of violent atmospheric concussion. These waves are not only transmitted with difficulty and greatly enfeebled in solid bodies not rendered elastic through tension, but, if not of such excessive intensity as to overcome all hindrances, their progression, in such bad conductors as the soft parts of the body, would gradually fail, like that of water in sand, before reaching the hidden recesses of the nerves. It would be otherwise, indeed, if we lived in water instead of air. The waves of sound propagated in water are much more easily transmitted to solid bodies, and must, in fact, pass through the corporeal integument of all aquatic animals endowed with the sense of hearing. It would also be quite otherwise if the vibrations of sound-producing bodies were conveyed to us through the medium of solid substances instead of the air. If we stop our ears closely, so as to obstruct in a great degree the passage, and thus oppose to the air-waves a resistance like that which would proceed from a corporeal surface similarly interposed, we shall hear no sound from a tuning-fork thrown into vibration at some little distance, even when the force of the undulations is increased by placing it on a resonant support. On the other hand, if we apply the vibrating tuning-fork to one of our teeth, we hear the sound in its full intensity, as probably most of our readers have experienced. The reason of this is clear: while in the former case the vibrations communicated by the tuning-fork to the air lose so much of their force by afterwards striking against a badly conducting surface as to be unable to reach the sensorium, in the latter, being communicated immediately to the tooth, they are readily transmitted by this to the surrounding elastic solids and bones of the head, by which last they are directly conveyed to the nerves of hearing. This transmission of sound through the solids and bones of the head even surpasses, under certain conditions, that which takes place in the ordinary manner by air-waves entering the ear, but only in the case when the distance between the ear and the vibrating object exceeds a certain magnitude, not, as some physiologists assert, under all circumstances. We hear a vibrating tuning-fork more forcibly if applied to the teeth than if held in the air at the distance of a yard from the ear, but the result is different if the instrument be held immediately before the entrance of the ear, as is strikingly shown by the following very simple and easily repeated experiment: If, after striking the tuning-fork, we bring it into contact with one of the incisors and wait until the sound, growing constantly weaker, is no longer perceptible, we shall hear it again if we remove the instrument from the tooth and hold it close beside the ear. But, apart from this, it is apparent that the transmission of vibrations through the bones of the head to the auditory nerve, would, of necessity, labor under so many embarrassments and be so limited in its employment, that, were this the only instrumentality through which we received the sensations of sound, our sense of hearing would lose all its importance as a means of intercourse between the mind and the outward world. Let us reflect how seldom it would be practicable, and in these rare cases how awkward, if we were obliged to place ourselves in immediate connection with any vibrating body by means of some solid conductor, a metallic rod or other; if, in order to convey the sounds of a violin, or the human voice, to our auditory nerve, a conducting apparatus must first be interposed between the sounding-board of the one or organs of the other and the solid parts of our own cranium. That we are almost exclusively limited to the air-waves as a conducting vehicle between distant sounds and the nerve needs no special proof; wherefore we conclude that, of all conceivable passages by which sound-producing vibrations penetrate to the sensorium, that alone deserves physiological consideration through which undulations of slight intensity, or proceeding from distant objects, attain with facility the nerve of hearing.

It is this one passage, therefore—the passage through the outer ear or auricle, the auditory canal, the tympanum, the ossicles or small movable bones, the labyrinth, both osseous and membranous—to which we shall turn our special attention, while we accompany the vibratory movement communicated by the air-waves through all these several stations in the order in which we have named them. We trust that it may be in our power to convey a clear comprehension of the apparatus, in part so greatly complicated, which the vibrations have thus to traverse. In beginning, however, we are somewhat embarrassed by the doubt as to how far we may venture to presume our readers already acquainted with the nature and laws of those periodic movements outside of our organs of hearing, designated as vibrations and waves—an embarrassment which recurs in a different form with the popular exposition of each of our physiological divisions, since physiology in all its parts presupposes a knowledge of physics and chemistry as its basis. We have before expressed our belief that, of all departments of physics, acoustics is that respecting which some degree of information is most widely diffused; in declining, therefore, the long digression which any adequate acoustical introduction would render necessary, and confining ourselves to the physiological view of our subject, we shall hope to be excused if we refer any reader who may happen to be little familiarized with it to some popular treatise for the doctrine of sounds. For the sake of simplicity and clearness in our statement, we shall substitute, in place of an analysis of all possible sources of sound, a single concrete example of a *string* vibrating under conditions which give rise to the undulations whose destination in the organ of hearing it is our purpose to follow. We take for this purpose a stretched string, supposed to be at a distance of some feet from the ear and of such a degree of tension that, when made to vibrate by a smart blow, it executes in a second four hundred entire vibrations—that is to say, four hundred forward and as many backward movements, with a constantly diminishing magnitude of the path described by its several constituent particles, until it returns to a state of rest. In such a string, the movement of the individual particles lying behind one another in the direction of its length is of very different magnitude; those at either end, *a b*, by which the string is

Fig. 1.



fixed, are entirely motionless; those next to them towards the middle have a certain, but very small movement, being restrained by their fixed neighbors, and the amount of movement increases as the middle of the string is approached, until the maximum of movement is reached in the particle *c* situated midway between *a* and *b*. If, therefore, we set the string in vibration by pulling it by the middle as far as *c'* and then releasing it, it will oscillate between the two limitary positions indicated by the dotted lines *a c' b* and *a c'' b* and its position of equilibrium *a c b*, which it passes at each successive oscillation. The curvature which the figure presents on both sides of the central line is the necessary result of the differences in the movement of the particles as just described. With the continuance of the oscillations, the number occurring in a second of time does not change, depending as this number does only on the length and tension of the string; but it is otherwise with the magnitude of the oscillations; as these proceed, the particle *c* deviates less and less from the position which it occupied when at rest, the curvature becomes constantly weaker, and the string finally forms a right line, as before the impulse. Did there exist now no material medium between the string and our ear, were this space wholly devoid of air, the vibrations of the string would have no relation to our auditory organ, could yield no sound; we should see, but not hear them. The sound, indeed, as we before remarked, proceeds not as such from the string, but is produced indirectly by the string through the action of its vibrations upon the organism of the ear; and for this mediate or indirect action there is needed a material medium with movable particles, a conductor capable of propagating the vibrations of the string by corresponding movements of its own constituent parts. How this propagation is effected by the air is easily shown. We must conceive the air, surrounding the string and filling the space between that and our ear, to be composed of infinitely small particles movable among one another, and we will suppose the ear to be placed on the side next to *c'*. If the string, now, be supposed to oscillate from the position *a c'' b* towards *a c' b*, it drives before it the particles of air next to it; it is evident, too, that, were the air a firm and cohesive body, the string could not execute its movement unless it propelled in a mass the whole of the air lying in its passage. But as the air is an elastic compressible body, the string, instead of driving before it at once the entire mass, presses upon and condenses only the parts immediately next to it, these in turn press upon the neighboring parts, and thus the impulse is progressively propagated from one portion of air to another. When the string has arrived at its limitary position *a c' b*, the displacement of successive portions of air and the compression which attends it have been propagated to a certain distance, so that the air to that extent is in a compressed state, forming a *wave of condensation.* Did the string remain motionless at *a c' b*, then, while the movement of propagation was still continued among the particles at the further end of the wave, those nearest to the string would come to a state of rest, then the next, and so on, till finally all the particles in the first wave would cease to move, the last of them precisely at the moment when the condensation on the further side has reached the limit to which it was carried during the forward movement of the string—that is to say, when the wave of condensation has reached its full length. But the string stops not at its greatest convexity; it springs back by virtue of its elasticity, and again passing the line of equilibrium, forms just such a curvature as before, occupying the second limitary position *a c'' b* During this recoil there occurs in the stratum of air lying to the left of the string, and which had been condensed by its forward movement, the opposite condition of rarefaction, because a void space is left behind the returning string which the air must occupy. The several particles of air follow the string, first, those immediately contiguous to it, then in succession those more remote, so that, as is readily seen, the direction of the movement which the particles now execute is the opposite of that which had taken place on the advance of the string, while the direction in which this movement is propagated from particle to particle is the same. When the string has reached the position *a c'' b*, the propagated displacement of the particles and the rarefaction attending it have extended through a certain tract on the same side with the previous condensation; the air within this tract is in a rarefied state, forming a *wave of rarefaction*, which, succeeding the wave of condensation, advances, like that, from space to space, and to the same distance. In the mean time, the string again springs forward, again compresses the particles of air before it, and in this way the wave of rarefaction is replaced by a new wave of condensation. Thus these waves proceed from the string to the ear in constant succession and regular interchange, alternating as often in a definite time as the string vibrates from side to side; and a particular particle of air, whatever its distance from the string, continues, as long as the string vibrates, to oscillate forward and backward according as it happens to be in a wave of condensation or one of rarefaction. We shall only add that the velocity with which the displacement of the particles of air is propagated always remains the same, no matter how great the number of the vibrations of the string, or how wide its excursions; and that the magnitude of the displacement of each several particle depends on two circumstances: first, on the width of those excursions; and secondly, on the distance of the particle from the string.

The wider the vibrations of the string or the departure of c' and c'' from c, so much greater, of course, is the displacement of the nearest particles of air, and consequently of those which follow in succession. As regards the diminution of the displacement in proportion to the distance from the string, an example drawn from a different quarter will afford a simple and intelligible illustration. If a stone be thrown into the water, there arises a circular wave, which continues to recede on all sides from the centre of disturbance, and in this wave the elevation and depression follow one another in regular succession, like the condensation and rarefaction in the sound-producing waves of the air; but whereas, in the immediate neighborhood of the place where the stone fell, the elevations are high, the depressions deep, the former, though the width of the wave remains unchanged, lose their height, the latter their depth as they recede, until both are at length lost in the level expanse. Such is the case, too, with the air-waves; the magnitude of the displacement of the several particles diminishes in definite proportion with the increasing distance from the source of the sound, and at a certain distance, which is greater as the movement nearest the source, or, in the present case, the excursion of the string is more considerable, the displacement wholly ceases. If our ear be beyond this limit, it is not reached by the air-wave, and can therefore not perceive the vibrations of the string as sound; probably, indeed, the perceptibility of the vibrations ceases within that limit, for it is little likely that, when the movement of the particles of air has become very slight, it should any longer have sufficient intensity to excite the terminal apparatus of the organ of hearing. At the distance of a few feet supposed, in our example, to intervene between the ear and the string, even weak vibrations reach the ear with sufficient intensity, the particles of air next the organ driven at the assumed rate of 400 forward and 400 backward oscillations in a second, forming, with the requisite distinctness, the alternating waves of condensation and rarefaction. Let us now inquire the operation and destiny of these waves in the organ itself.

The part of the ear which is first struck by the air-wave is the auricle or pinna, constituting the external organ, in regard to whose structure and arrangement every one may easily satisfy himself. This external portion presents a sort of hollow shell with several eminences and channel-like depressions, the base of the shell containing a funnel-shaped cavity which opens immediately into the inner passages of the organ. In the lower animals the whole ear has, for the most part, more of the funnel, or, at least, spoon-shape, and is in general much more movable than in man. The human ear is, however, by no means immovable; the reader will scarcely fail to have met, in the course of his life, with some wonder-worker, who, with more or less skill, pretends to the power of wagging his ears. This egregious accomplishment, be it known, is cheap; the ear of every one is provided with several muscles, which proceed in different directions from its base to the surface of the skull, and which, by their contraction, may change the position of the auricle; to these muscles are distributed nerves, whose relations to the brain and the will are like those of the innumerable other nerves, and which are capable, therefore, of communicating motion to the muscles. But just as the deaf-mute possesses the same muscles and the same nerves of volition with him who articulates, and has only not learned to use them because for him the natural instructor, the sense of hearing, is deficient, so it is no absence of the original power but a neglect to use the muscles of the ear in early life, which, in most men, leaves that member incapable of voluntary motion. It may sound strangely, but still is true, that for this neglect the blame lies in the tasteless as well as injudicious custom of muffling the heads of infants in caps, by which the ears are so long kept immovably bound to the head, and precisely at the time when the soul is getting acquainted with its bodily machine, and learning to use it. Crying, speaking, handling, the child learns, not experimentally and with any purpose of the will, by setting in motion the muscles of the throat and thorax, the arm and the hand, to try the effect and benefit which may result. The soul never arrives at a direct perception of these special members, not to say that the knowledge of their existence and of the means of setting them in motion would be the prerequisite incitement to their use. That primary incitement consists much more in the activity of the muscles themselves; the child cries at first mechanically—that is to say, its vocal muscles are thrown into action in a reflex way, the muscular irritation being the effect of the irritation of the nerves of sensation in the brain produced by an external object. With this activity is connected a determinate, and for every muscle a specific feeling, a so-called muscular feeling, of which the child becomes conscious, as it does at the same time of the sensation of sound which is the result of the activity of the vocal muscles. Thus the child first learns that this specific muscular feeling always coincides with that sensation of sound, or, again, that with the muscular feeling which the movement of the arm produces, is associated a sensation of pressure when the limb encounters an external object, which object may at the same time give rise to a visual sensation, &c. The education of the soul thus proceeds step by step, and in this way it very gradually succeeds in obtaining an indelible idea of all possible combinations of the muscular feelings, and in understanding their signification from the consequences experienced, so that it knows exactly what sort and degree of effort the will must make, in order to call forth any definite muscular feeling, and attain the results known to be associated with it. The same circuitous process would lead to the conscious use of the muscles of the ear if their unconscious reflex activity were at first possible, for this would be our instructor by means of the associated muscular feeling and the knowledge of its consequences. Respecting this, however, it is enough to say that the detriment occasioned by the defect of motion is not great, since the human head is in the highest degree movable in all directions, and hence we can well dispense with any movement of the ears. The purposes connected with a change of position of the ears will presently be noticed.

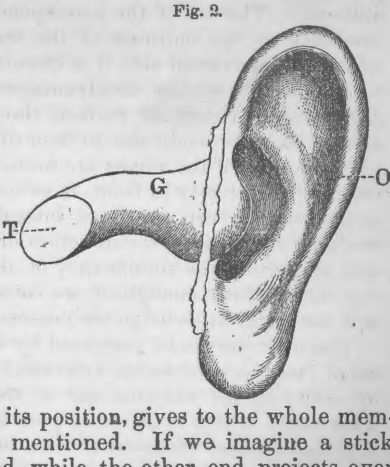
If we ask now what function the outer ear fulfils, in what acoustical relation it stands to the air-wave, the common answer is at hand: to intercept and receive it; but this is by no means an exhaustive explanation. It is true, indeed, that this member is an auxiliary but not an altogether indispensable part of the auditory apparatus, since it is well known that, where it is wanting, not only does the perception of sound exist, but that often it is not sensibly weakened or affected. The same is the case if the external ear be occluded by filling its cavity with some pulpy inelastic substance, while the auditory canal, and that alone, is kept open for the transmission of sound by the insertion of a small tube. But although the auricle is not indispensable, it is not in vain that it occupies its place. Its design may be sought in a two-fold relation to the sound-producing waves of the air: these may rebound against it, as the light is reflected from a mirror, and by means of its different eminences and depressions be thrown in greater quantity into the interior passages; or it may serve as a sort of sounding-board, or, more correctly, as the bridge of a violin—that is to say, receiving the wave of sound and, being a rigid, elastic, outstretched cartilage, it may itself be thrown into vibration, and through the walls of the auditory canal communicate that vibration to the tympanum, as the bridge of the violin transmits the vibrations of the strings to the sounding-board. In both cases the advantage would consist in economizing a part of the air-wave which would otherwise be lost. It was usual, formerly, to seek in the action first mentioned, the reflection, namely, of the wave into the auditory passage, the service of the outer ear. By more careful consideration, however, of the directions in which, according to physical laws, waves of sound, striking in different lines upon its differently shaped parts, must be reflected, the auricle is shown to be very little fitted for this purpose, since it gives no direction to the incident waves by which any important portion of them can be thrown into the auditory channel. If we would convert the auricle into a reflector, we must resort to a very usual means of overcoming any difficulty of hearing by bending forward its free cartilage with the hand, while at the same time the hollow palm of the latter is extended to increase the capacity of the cavity. In this way the auricle with the hand acts as an ear-trumpet in collecting the waves of sound and throwing them thus condensed into the ear. In its usual posture, on the other hand, the chief service of the auricle is to be sought in the second of the above-named relations: on the impact of the air-waves, it is itself fitted to take up the vibrations and convey them through the intermediate structures to the tympanum. The communicated vibrations will, from physical laws, be stronger in proportion as the direction in which the air-waves strike is more perpendicular, but as the auricle is not a level surface, and, at different points, is struck by the same air-wave in different directions, it is more exact to say that its conducting capacity will be greater, the larger the portion of its surface opposed perpendicularly to the air-wave. If the source of the sound is in front of us, so that the waves are directed perpendicularly towards the face, they will evidently strike the right and left ear under equal conditions. In this case it is plain that the ears are not in the most favorable position for the entry of the air-wave into the auditory passages, since, on each side, these cross the direction of the wave at right angles, and there is, besides, a projecting lobe in front which forms a sort of screen to the entrance; still the auricles are not unfavorably situated as regards the direction of the waves, since a large portion of their cavity is turned almost perpendicularly to that direction, at least in persons whose ears stand out somewhat from the head and do not form too small an angle with its side-walls. In this way we may account for the conclusion experimentally arrived at by one of the older physiologists, that the perception of sounds is most distinct if the auricle forms an angle of 40° with the lateral surface of the head; the most favorable angle for all directions of sound there are no means of determining. If the source of sound, instead of being before, is on one side of us and in the direct prolongation of one or other of the ear-passages, the conditions are of course different. The ear of the corresponding side has then evidently the advantage, inasmuch as the entrance of the wave into the auditory canal is here direct, while on the averted side it is circuitous; nor does the auricle, in this case, present itself in a wholly disadvantageous position to the undulations, since no inconsiderable part of its surface, though a different one from the former, is now more or less perpendicular to their direction. That, on the whole, the conditions for the access of the waves are more favorable when they strike the ear laterally rather than directly in front, is evinced by the fact that, in earnest listening, we unconsciously turn the head laterally, so as to bring the axis of one of the auditory canals into the direction of the sound. The above is all that can be said respecting the significancy of the outermost structure of the organ of hearing, and is little enough, if we consider how freely it lies open to observation and the exact knowledge we possess of the nature of the undulations.

The next part to be traversed by the sound-producing air-wave is the auditory canal *(meatus auditorius externus)* which leads from the bottom of the concha or cavity of the external ear to the seat of the finer apparatus of the organ. This canal, which is about an inch long, is not straight, but somewhat bent, nor is it everywhere of equal width; its walls are covered with the viscous secretion called ear-wax, and, especially at the entrance, are overgrown with small hairs. The function of this part, in a general point of view, is clear, though some obscurity rests upon certain special details—that is to say, we know that this external meatus, partly through the included air-column, partly through its own elastic environment, conducts the waves of sound to the interior of the organ; that on account of its curvature and varying width the waves must, in their passage, strike against and rebound from its walls, so that none can reach the tympanum in a quite direct and unbroken line; further, we may conjecture that this reflection of the air-waves from the walls gives them additional force, on the same principle that the rebound from a vaulted surface is known to strengthen sounds; but we are not in a position to determine the course of each wave in this tube with so much precision as physics might desire. The hairs at the entrance are an important protection against the intrusion of foreign bodies, especially particles of dust, which, at least to a certain extent, is thereby prevented. Respecting the true use of the ear-wax there is still a question; the covering of the walls with a glutinous substance would be judged *a priori* to be detrimental to the conducting capacity of the tube, as too great a secretion and accumulation of it are in fact found to be. A recital of the conjectures which have been formed on this subject may well be spared.

We come now to a very important part of the apparatus for conveying sound, the *tympanum*, and its connection with the reservoir of the nerves by means of the *ossicles of the tympanum*, a chain of small movable bones. In order to give the reader a correct idea of this mechanism, it is indispensable to convey as clear a conception as possible of the anatomical arrangement, form, position, and adjustment of its several parts; an undertaking which would be easy had we before us, for the purposes of demonstration, either preparations from nature or artificial models, but which presents no inconsiderable difficulty when it is to be accomplished by means of description and mere delineations.

The tympanum is a delicate membrane, which is stretched like the head of a drum over the inner extremity of the auditory canal, and forms a wall of separation between that passage and a small interior cavity, known as the *cavity of the tympanum*. This membrane is not placed perpendicularly, but obliquely, in such manner that its exterior surface next the canal looks obliquely downward, and at the same time somewhat backward, to the floor of the canal, while the interior surface, turned towards the tympanic cavity, looks upward and somewhat forward. The auditory canal is thus obliquely cut off at its inner extremity, its floor or lower wall being longer than its roof or upper wall, the hinder wall somewhat longer than the front one, the cavity of the tympanum lies more above than behind that membrane. The annexed figure represents the outer ear O, the auditory canal G, and tympanum T, of the right side, seen directly in front. In order to place the membrane of the tympanum in open view, the ossicles connected with it are left out, and for the same reason the outline of the cavity is not given. But the membrane of the tympanum forms not a plane surface. It is arched in the direction of the cavity, so that its outer surface is concave, its inner convex, while between the two layers of which it is composed the handle of the malleus, a small bone, to be presently described, descends from the upper edge as far down as the middle, where it is firmly attached, and, by its position, gives to the whole membrane the curvature which has been just mentioned. If we imagine a stick firmly affixed to the centre of a drum-head, while the other end projects over the rim, and that a force is applied to this outer and movable end, so that the skin of the drum is at once stretched and pressed inward, we shall obtain some idea of the arrangement of the malleus and the tympanum.

Fig. 2.



Let us examine now the action of the sound-producing undulations, the before-described waves of condensation and rarefaction on the membrane in question. It is apparent that this will be thrown into vibration, and that its vibrations will exactly accord in intensity and number with those of the air-waves, and mediately also with those of the body by which the latter are set in motion. Each wave of condensation, the several particles of which are pressing forward in the direction of the wave, will communicate to the particles of the tympanum the same movement, and therefore increase its inward curvature, and at the same time its tension; and each wave of rarefaction—the direction of the particles being now opposed to the general movement of the wave and away from the tympanum—draws the latter, in a certain measure, after it, and the vaulted membrane is correspondingly flattened towards the external air passages. It has been a subject of controversy, in a physiological point of view, whether the movement impressed on the tympanum by the waves of sound, be, like that of the string before described, a normal, vibratory movement, or an undulatory one like that produced by alternate impulsion and retrogression in any fluid, as, for instance, the air. Without entering further into this question, the discussion of which would involve an analytical inquiry into the doctrine of undulations, we shall only remark that, in our own opinion, the tympanum of the ear can, under no circumstances, execute any other movement than vibrations of the former order. If we deliver a smart blow on the head of a real drum it is evident that the elastic membrane will be driven inward in a convex or dome-shaped curve, since here, as in the string, the middle particles are most movable; those lying nearer to the rim are less so, while those immediately at the rim, over which the skin is stretched, are immovable, like the points at which the string is attached. By thus bending inwards, the tension of the membrane is necessarily increased, wherefore the inward bending can only proceed to the point at which the elastic force of the increasing tension finds itself in equilibrium with the force which has been applied from without; when the impulse of the latter gives way the elastic force drives the membrane back, as it does the thread, not to its original plane, but with such velocity that it passes that line and is now curved outwardly, until again arrested by the increasing tension and again driven inwardly. Thus the membrane with which the drum is covered continues to oscillate inwards and outwards with diminishing curvatures, until the whole movement subsides to rest. Now, the membranous tympanum of the ear is placed under precisely similar conditions, and must therefore execute the same sort of vibrations; its particles are not at all movable in an equal degree, so that each, as is the case with the particles of the air, can, in like degree, follow the impulse of the air-wave, the movement of each being exclusively dependent on the direction and force of the impelling particles of that wave; on the contrary, in consequence of the adhesion of the membrane throughout its whole circuit to the walls of the auditory canal, the particles next to the adherent edge are, as in the artificial drum-head, immovable; those at the centre the most movable; the intermediate, movable in a progressive ratio from the periphery to the centre. The magnitude of the movement of each particle depends, therefore, not merely on the impulsive force of the air-waves, but also on its position and consequent mobility. If we represent to ourselves an air-wave of condensation as striking perpendicularly upon the tympanum and simultaneously exerting an equal force upon every particle of the membrane, it is evident, from the different degree of resistance opposed by the several particles—a resistance which increases according to the distance of each particle from the centre—that the result of the impulse will be a convex flexion of the membrane towards the interior cavity. Upon the same principle the succeeding wave of rarefaction will be followed by a similar flexion in the opposite direction. If no wave of rarefaction followed the wave of condensation, the tympanum, when the impulse of the latter was withdrawn, would still spring back through its own elasticity, and undergo, like the drum skin, after one blow, a series of alternating vibrations around its line of equilibrium, the velocity of which would depend alone on the size and tension of the membrane. But with alternate waves of condensation and rarefaction, the tympanum vibrates only under their influence, and as these vibrations will be exactly synchronous with those of the string, which, in our previous example, we supposed to be the source of sound, their number in a second of time will, in like manner, be four hundred inwards and four hundred outwards. The form of the vibrations remains the same; whether the magnitude of the displacements of the particles of air in contact with the tympanum be great or small, nothing changes with that magnitude but the depth of the resulting curvatures. If the air-wave does not strike the whole surface of the tympanum perpendicularly and at the same time, but impinges obliquely by striking first a part near the edge, a convex curvature ensues, but its form will be somewhat different. Though this be commonly the case on account of the oblique position of the tympanum to the auditory canal, we will not enter into a closer analysis of the different forms of curvature which would result from this circumstance, since nothing is thereby essentially changed, and the following explanations will be rendered more generally intelligible by being made in reference only to the simplest form of vibrations.

If we ask now to what end a tense and elastic membrane is interposed in the passage of the waves of sound, why these are not propagated as air-waves to the perceptive apparatus of the auditory nerves, the answer is simply as follows: The ends of these nerves are suspended in a fluid, and for obvious reasons must be so; hence the question first concerns the communication of the waves to this fluid. Did the air-wave immediately strike the fluid the transmitted impression would be extremely feeble, so that weak or remote vibrations, which, under the existing arrangement we hear distinctly, would be no longer perceptible. Besides, it is not easy to see how such a direct transmission of the waves of sound from the air to the fluid could be effected, since a free surface accessible to the external air would be incompatible with the safety of the tender nerves. It is imperative, therefore, that the fluid be enclosed within solid walls; but walls of osseous structure take up the vibrations of air with as much difficulty as fluids, and would transmit them in the same weakened condition; hence there was needed some other and suitable medium between the air and the aqueous secretion. Now this purpose is fulfilled in the most perfect manner by the system of small bones appertaining to the tympanum. Air-waves are with difficulty transmitted immediately to solid bodies, but easily and with intensity to stretched membrane; and when, in this way, the membrane has been thrown into oscillatory movements, as above described, these movements are readily communicable to solid bodies—a fact of which every one may satisfy himself by the following simple experiments: Over the opening of a glass or a tube let a dry membrane be stretched, whether it be of bladder, leather, or mere paper, and let this membrane be strewn with fine sand; if a tuning fork, which has been made to vibrate by a strong blow, be held at a little distance above the membrane, the lively vibrations into which the membrane is thrown by the air waves proceeding from the fork will be manifested by a saltatory movement of the grains of sand as they continue to bound upwards and fall again on the vibrating surface. To show the facility with which vibrations of the membrane are transmitted to solid bodies, let a metallic ring, such, for instance, as is used for keys, be clasped round with the thumb and forefinger of the left hand, while, with the right a strongly vibrating tuning fork is held close above it. With the simple ring not the slightest motion will be felt, but if a membrane be previously stretched upon it and the experiment be repeated in the same way, the ring will be felt to vibrate in the most sensible manner as long as the vibrating fork continues in its vicinity.

We now know how the air-waves are received by the membrane of the tympanum; let us next see how this membrane communicates its vibrations to the chain of small bones behind it, and in what way these in turn set in undulatory motion the fluid contained in the so-called labyrinth. It is no very easy matter to make the structure, position and connections of the system of small bony levers interposed between the tympanum and the labyrinth perfectly clear; we shall endeavor first to exhibit the principle of the arrangement by means of some illustrative delineations: Fig. 3 represents the simplified form of the apparatus seen in perspective; Fig. 4, an ideal vertical section thereof. A little rod of some length *a b*, which bears at its upper extremity a knob *b*, is by its lower half *a c* attached to the stretched membrane of the tympanum T *r*, in such manner that it reaches from the upper edge directly to the middle of that membrane, thus dividing the upper half of it into two equal lateral portions, while the upper half of the rod with the knob projects freely into the cavity of the tympanum. Through the middle *c* of the rod, and crossing it at right angles, (in Fig. 4 supposed to be perpendicular to the surface of the paper,) passes a solid transverse rod *d e*, both ends of which are so secured by ligaments to the bony wall of the cavity near the border of the tympanum, that this transverse rod can turn upon its own longitudinal axis, but not be otherwise displaced. We may imagine both ends surrounded by annular ligaments which are tightly fastened to the wall of the cavity. The lower end *a* of the longer rod bears a short arm *a f*, with which it forms a right angle, and which is perpendicular to the surface of the membrane of the tympanum. The free end *f* of this arm bears finally a stirrup-shaped appendage *g*, firmly articulated with it, whose foot-plate is fitted like the piston of a syringe into an oval window-like opening of the reservoir L, (the labyrinth,) containing a fluid; with the edges of this reservoir the appendage *g* is united by a membranous and very elastic border. The action of the vibrations of the tympanic membrane on this apparatus must necessarily be as follows: If the membrane be driven inwards by a wave of condensation so that its curvature, as shown in Fig. 4, is increased and its centre *a* be thus brought nearer to the reservoir L, the rod *a b* must follow the movement and turn around the transverse rod *d e* (which serves as its axis of revolution) in such manner that the part *c a* below the fulcrum *c* moves inwards and upwards with the tympanum, while the upper part *c b*, projecting into the cavity, moves outwards and downwards, as is indicated by the directions of the arrows in Fig. 4. The rod *a f*, attached rectangularly to the former, will thus be lifted up in the direction of the arrow proceeding from *a*, whereby its stirrup-like appendage *g* will be thrust deeper into the reservoir, and the elastic border by which it is united to the edge of the opening will undergo extension. The fluid in L receives this external pressure and is propelled before it. Were the walls of this reservoir rigid and unyielding throughout its whole circuit the fluid could not give way, and, since a fluid is compressible only by strong force, while that of the air-wave is relatively small, the resistance thus opposed would render the entrance of the appendage impossible, and mediately also the whole movement of the apparatus and the tympanum. In order, therefore, that the external

Fig. 3.

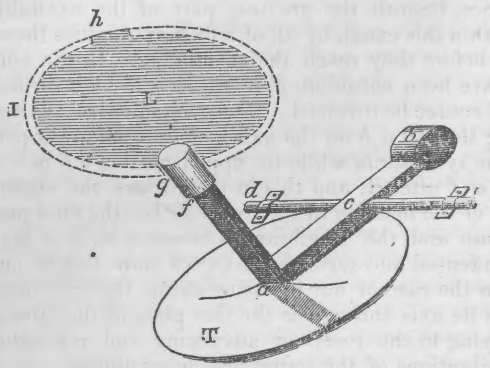
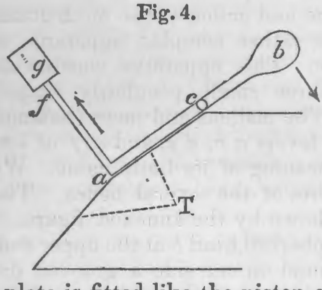


Fig. 4.



action may take effect, the reservoir is provided with a second opening *h*, which is closed with an elastic membrane, (*membrana tympani secondaria*.) In proportion as the stirrup-plate is pressed deeper into its opening, the yielding fluid drives the membrane *h* before it, bending and stretching it outwards. We shall see hereafter that, in nature, this reservoir of fluid consists of a sac-shaped space in which is the opening for the stirrup or *stapes*, and of a long spirally convolved canal; the other opening *h* lies at the extreme end of this canal, so that the movement of the yielding fluid, propagated from particle to particle, must traverse the whole canal before it arrives at the elastic membrane. This extensive circuit is of the utmost importance, because the greatest part of the excitable nerves have their extremities within this canal, by all of which extremities therefore the liquid waves must pass before they reach the membrane. If the condensed wave, whose effect we have been considering, is succeeded by a rarified wave, all the movements will of course be reversed. The tympanum recoils and oscillates outwards, thus turning the rod *a b* on the axis *d* so that the lower portion *a c* moves outwards with the tympanum while its upper portion *c b* moves inwards; the portion *c a* draws *a f* after it, and this in turn draws the stirrup *g* in a corresponding degree out of the opening of the reservoir L; the fluid pursues the withdrawing stirrup-plate and the membrane *h* becomes level or may even be drawn inwards. If condensed and rarified air-waves now follow one another in rapid succession, as in the case of our vibrating string, the little lever *a b* turns with equal velocity on its axis and moves the foot plate of the stirrup in its opening up and down, causing in the reservoir advancing and retreating waves, equal in number to the vibrations of the sound-producing object.

After this general representation of the scheme and action of the mechanism, it will not be difficult to comprehend that of the rather complex apparatus of the ossicles of the tympanum in its actual state. This apparatus consists of three members connected with one another—three small, peculiarly shaped bones—the *malleus*, the *incus*, and the *stapes*. The malleus and incus (hammer and anvil) together correspond to the system of levers *a b*, *d e*, and *a f* of our scheme, the stapes to our stirrup, which is the meaning of its Latin name. We will first consider the form of the several bones. The malleus consists, as is shown by the annexed figure, of a handle *a b* and of a spherical head *c* at the upper end. Beneath this head is found on one side a grooved depression running obliquely around the neck from above downwards, into which a corresponding surface of the incus is fitted; on the other side proceeds from about the middle of the bone a long and slender outgrowth or projection *e f*, running to a point, corresponding to the portion *d c* of the transverse axis *d e* in the previous figure. The incus much resembles a bicuspid tooth with widely separated roots. It consists of a short, thick body *a*, whose upper surface *b* is of an oval shape and is so channelled out as to be concave in a longitudinal and convex in a transverse direction, fitting accurately into the depression at the neck of the malleus. From the body *a* proceed, nearly at a right angle with one another, two processes, one short and thick, *c*, the other, *d*, long and somewhat bent towards its extremity, on which it bears a small lens-shaped bone *e*. With this terminal bone of the incus articulates the head of the stapes, whose form corresponds to its name; it consists of a head, two divergent branches unequally bowed, *a b*, and a flattened oval-shaped plate *c* which forms the base or foot of the stirrup. The position, ligature, and connection of these bones with each other, the tympanum and

Fig. 5.

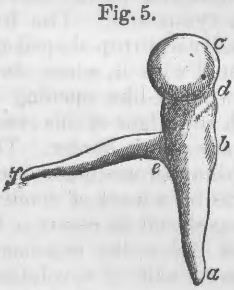


Fig. 6.

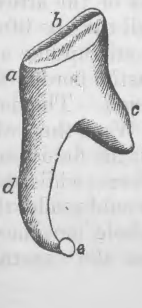
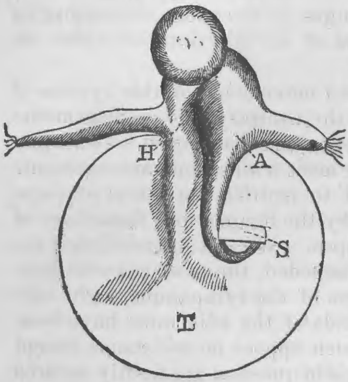


Fig. 7.

the labyrinth, is as follows: If we imagine a human head vertically divided in the middle from front to rear, and from the right half all the parts, both solid and soft, to be removed until we arrive at the cavity of the tympanum and thus bring to view from within the apparatus of the right ear with the membrane of the tympanum, the appearances presented to us, when we look a little obliquely from above, will be those represented in the accompanying figure. T *r* indicates the tympanum, H the malleus, A the incus, S *t* the stapes. The handle of the malleus is firmly united to the membrane of the tympanum, being inserted between the two soft layers of which the latter consists and extending from the upper border to the centre, so that, like the rod *a c* in the schematistic figure, it must necessarily follow the movements of the membrane. The neck and the heavy spherical head of the malleus project freely over the border of the membrane into the cavity of the tympanum; the long projection, on the other hand, runs, at the height of the upper border of the tympanum, forward, and is here tightly fastened, by one of the short, elastic ligaments which spring from its point, to the wall of the cavity. The neck of the malleus articulates with the summit of the incus, the two surfaces being reciprocally adapted to one another, and their edges secured by a capsular ligament which permits but slight displacements. Through this arrangement the short, thick process of the incus is in such a position as to form a right line with the long projection of the malleus, being directed transversely backward as the former forward, and is fastened to the hinder wall of the cavity by a sort of capsule which allows of its turning on its longitudinal axis. The long projection of the malleus and the short process of the incus answer therefore together to the transverse axis marked *d e* in the first delineation, around which turns the little rod marked *a b*. The longer process of the incus runs parallel to the handle *(manubrium)* of the malleus, from which it is separated by a small interval, its course through the cavity being rather more inward and backward. As the membrane of the tympanum lies obliquely this process is consequently placed obliquely; but its lower end, which corresponds to the rod *a f* of the first delineation, bends upwards, so that the foot-plate of the stapes, which is united to it, looks in a certain degree in the same direction. This foot-plate is introduced, like the piston of a syringe, but more loosely, into an oval aperture *(fenestra ovalis)* of the bony wall of the so-called labyrinth; around its edge there extends to the margin of the aperture a small membranous border which renders the aperture water-tight, while, through its elasticity, it permits the stapes to make slight excursions up and down in the receptacle. The opposed opening, *(fenestra rotunda,)* whose necessity was shown above, exists in the labyrinth in the shape of a small round window, closed with an elastic membrane.

Fig. 8.



It will not be difficult for the reader, with the aid of our previous representation of the general scheme, to conceive in what manner this apparatus, under its actual conditions, must operate when the membrane of the tympanum is made to oscillate by the impression of the air-waves. If the membrane of the tympanum be driven inwards, both malleus and incus move as if they were but one piece, though, at first, it is true, only the handle of the malleus is directly moved; but the incus follows the impulse, the adjustment admitting of no counter action. Both turn upon the common axis, which consists of the long projection of the malleus, and the short process of the incus, so that the neck and head of the malleus, and thus the part of the apparatus lying above the axis, describe an arc outwards and downwards, the handle of the malleus, on the other hand, and the long process of the incus describe an arc inwards and upwards, and consequently the stapes fixed at the end of this long process is pressed more deeply into the oval window of the labyrinth. If the membrane of the tympanum bends outwards with the receding wave, the preceding movements are reversed, the part of the apparatus above the axis turns inwards and upwards, the part below the axis outwards and downwards, so that the base of the stapes is proportionably withdrawn from the oval opening. These changes of direction, alternating in rapid succession, produce undulations in the fluid of the labyrinth, of which we shall presently have occasion to speak.

We should by no means think of the described movements of this system of ossicles as being of any considerable extent; on the contrary, the displacements undergone by its several parts from the appulse of even the strongest waves upon the tympanum are excessively slight; indeed, we meet with certain arrangements in the apparatus which are evidently calculated to restrict the extent of these movements. This end is subserved, especially, by the ligamentous fastenings of the axis of movement and of the base of the stapes. Were it intended that the malleus and incus should follow, freely and unimpeded, the most extreme flexions, inwards and outwards, which the membrane of the tympanum might execute under the influence of the air-waves, the ends of the axis must have been inserted, like those of an axletree, in sockets which oppose no resistance except that arising from friction. But the ends of the axis in question are tightly secured by tense ligatures, which are twisted by every turn of the axis, and by virtue of their elasticity offer the more resistance the greater the torsion. In like manner a resistance is opposed to the movements of the apparatus by the nature of the ligature which unites the base of the stapes with the fenestral opening; the small membranous border, before described, very quickly arrests by its tension the intromission and retreat of the foot-plate. It is apparent, also, that the resistance thus offered restricts the oscillations of the tympanum itself, causing them to be of less extent than they would be, under an equal force of the air-waves, were the membrane not united with the ossicles. The object of this restriction is easily conjectured. Though we know not what force of the atmospheric undulations can be borne by the extremities of the nerves suspended in the fluid of the labyrinth, yet we may well infer that they would be injuriously affected by a very violent shock, as the nerve of sight is dazzled by too intense an undulation of the luminous ether. As in the eye protective arrangements are met with which in a certain degree shield the optic nerve from too strong an impression of the light, an analogous intention may, with the greatest probability, be ascribed to the adjustments which in the small bones of the tympanum act in restraint of their movements. This probability almost becomes certainty when we take into view a further arrangement of this apparatus, for which scarcely any other object can be imagined than to prevent too violent a percussion of the fluid of the labyrinth. This arrangement we will now examine somewhat more closely.

Wherefore do we find, in place of an inflexible lever, consisting of one piece, such as is supposed in our representation of the general scheme of operations, and as would have fulfilled all the conditions of those operations, an apparatus composed of two separate parts, malleus and incus, connected with one another by a species of hinge. What are the kind of movements for which this hinge is destined, and what is the signification of this mobility? Why, again, is the stapes a separate bone? To be able to answer these questions, we cannot dispense with bringing to the notice of our reader the existence and mechanical relations of two small muscles, evidently destined for the movement of these bones of the ear, one of them pertaining to the malleus, the other to the stapes. The first is the more important; it springs from about the middle of the skull, passes with its soft and somewhat long fasciculus of fibres, in a channel of its own, to the cavity of the tympanum, into which it penetrates, with its tendinous end, opposite to the tympanic membrane. The tendon goes obliquely through the cavity, and is attached, at a right angle, to the handle of the malleus, near the turning point. We must suppose it to be known what a muscle is, and shall therefore only observe that every muscle has the power of contracting under a certain influence of the nerves, by which means a thickening takes place in the body of the muscle, and its two ends are brought nearer to one another. When the muscle in question, the so-called *tensor tympani*, contracts at the excitation of its nerve, it tends, since its interior point of origin is immovably fixed, to draw its exterior point of insertion, and consequently the handle of the malleus, inwards. But the malleus alone cannot freely follow this traction, first, because it is itself fastened by its long projection to the wall of the cavity of the tympanum; secondly, because it is inserted between the coats of the tympanic membrane; thirdly, because it is connected by articulation with the incus. It is not easy to determine with certainty in what manner this resistance modifies the displacement of the malleus by the muscular traction. Were the malleus and incus united in one piece, the operation of the muscle could be no other than that of a condensed wave of air coming from without; it would turn the lever apparatus in such manner on the above described transverse axis, that the handle of the malleus and the longer process of the incus would move inwards and upwards, and the stapes be thereby pressed deeper into the fenestra ovalis, while the membrane of the tympanum united to the handle must also comply with the traction, be drawn further inwards, and consequently rendered more tense. This is, in fact, the muscular effect which is generally recognized, and has procured for the muscle its name of "tensor tympani." There are circumstances, however, which justify hesitation in admitting that in this simple way everything is explained. The muscle is relatively very long and its tendon attached so near the turning-point of the malleus, that if, through the contraction, it were shortened by a very small part of its length, the result would be a comparatively very large introversion of the malleus. Now, the membrane of the tympanum might indeed, under strong tension, follow with the malleus the traction of the muscle to a considerable extent; but not so the incus, because the penetration of the stapes into the *fenestra ovalis* would encounter insuperable resistance, partly in the ligature of the foot-plate by the narrow border, partly in the fluid which can only yield so far as the opposed opening of the labyrinth, the so-called *fenestra rotunda*, permits. Hence a forcible pressure of the stapes would render the propagation of the sound-producing movement wholly impossible, since no condensed wave would be capable of driving it more deeply inwards, no rarefied wave could move it outwards against the traction of the muscle. The length of the fibres of the muscle shows, on the other hand, that the magnitude of the movement for which it is intended can be no such very slight one, for, throughout our body, the length of the muscular fibres is found to be in direct proportion to the extent of the movement to be accomplished by them. The supposition is therefore forced upon us that the muscle in question can move the malleus alone to a certain extent, without any proportionable movement of the incus; that the malleus, while, with the membrane of the tympanum, it obeys the traction of the muscle, is pressed more firmly against the incus and into the joint which connects both. This hypothesis is the more plausible, because by this alone is explained the presence of the articulation, for which there would otherwise be no apparent reason. We must unfortunately renounce the design of laying before the reader proof of the correctness of the hypothesis drawn from the structure of the articulation, the direction of the muscular traction, &c., for the discussion of these extremely subtle relations would be impossible without the employment of preparations from nature, and would perhaps even then leave the matter in doubt. We can therefore only trust to the reader's faith when we allege that the tensor muscle, in contracting, draws the malleus somewhat inward, thereby occasioning greater tension of the tympanum, but leaves the incus in its natural position, because it turns the malleus less upon the axis common to the two ossicles than upon a vertical axis which passes through the point at which its long projection is attached to the wall of the cavity. Both malleus and incus acquire, through the resulting pressure of their articulating surfaces against each other, a new relative position, but act in this new position, towards the waves of sound arriving from without, as a single lever which propagates their impressions by means of the stapes to the fluid of the labyrinth.

If our readers should perhaps think this explanation of a small and inconspicuous mechanism tiresome and superfluous, while we can take no exception to the first of these epithets, we must protest against the application of the second; the influence of that mechanism on the hearing is, in fact, of the highest importance. In what follows we shall simply regard, as the effect of the muscle in question, an increase of the tension of the membrane of the tympanum, and shall inquire in what consists the utility of that effect. There can be no doubt that it consists in *deadening the effect* of too intense an action of the waves of sound. It is a physical fact that strongly stretched membranes respond to these waves with less intensity, are thrown by them into weaker vibrations, than membranes in a state of less tension; a fact which has been shown by multiplied experiments and may be here taken for granted. As the skin of a drum is pressed inwards by the hand with more difficulty when it is tightly stretched than when relaxed, so the membrane of our ear opposes the greater resistance to the force of the impinging undulations of the air the greater the tension of the membrane. It is a matter of indifference whether the stretching force is exerted around the rim in the plane of the membrane, or, as is the case with the action of the tensor tympani, at the centre of the membrane and perpendicularly to its plane. It has already been seen that the resistance opposed by the membrane to the air-wave increases in proportion to its convex curvature from the increasing force of the elastic reaction; the tensor muscle, which acts like a wave of condensation in producing inward flexion of the membrane and thereby increasing its tension, must in like manner intensify the elastic forces and increase the resistance to the air-waves. Now, if the membrane, in consequence of the increased tension, executes vibrations of less magnitude, in the same degree will the oscillations of the small bones which are moved by it, and finally the intensity of the undulations in the fluid of the labyrinth, be abated; in a word, a given air-wave excites a weaker sensation of sound under strong tension of the tympanum than under slight tension.

This result may at any time be verified in the simplest manner—not, indeed, by a voluntary contraction of the tensor tympani, but by other means which we possess of increasing the tension of the membrane. The cavity of the tympanum is not altogether closed up, for the *Eustachian tube* penetrates into it with a rather long channel, which, at its anterior extremity, opens into the hinder part of the throat near the palate; by this channel the air within the cavity is placed in communication with the outer atmosphere, which, in the act of breathing, passes by the orifice of the tube. By a certain effort, during that act, it is in our power to press the air through this tube into the cavity of the tympanum, and, by another sort of effort, to withdraw it therefrom; the first takes place if, in the act of ordinary expiration, we bar the egress of the air through the mouth and nose, and thus compel its passage into the Eustachian tube; the second, if, with closed mouth and nose, we make an effort at inhalation, and, by thus enlarging the space occupied by air, produce its rarefaction in all parts of the respiratory apparatus, and consequently in the cavity of the tympanum. In both cases the tympanic membrane is more tightly braced; in the former the inhaled air presses the yielding membrane outwards; in the latter the membrane is drawn more strongly inwards in consequence of the rarefaction of the internal air, and the same effect is produced as by a contraction of the tensor muscle. In both cases obtuseness of hearing is the result. If we press air with force into the cavity, there occurs, at the moment when the air enters, a cracking or clashing noise, which is very clearly perceptible to ourselves, and may even be heard by others, if their ear be brought near to ours; during the whole time that air is thus compressed in the cavity there exists, but perceptible only by ourselves, a humming noise, which increases and decreases with the force of the compression. The nature and causes of this singular noise in one's own ear are not determined with certainty. According to an old and widely-received opinion, the cracking sound arises from a sudden and voluntary contraction of the tensor muscle and the consequent sudden stretching of the membrane of the tympanum; but as the noise does not, as the supporters of this opinion believe, occur at the moment when air ceases to be driven into the cavity, but, on the contrary, at the moment when it commences, this view is clearly erroneous. The noise arises upon the sudden flexure outwards of the membrane, and therefore with the tensor muscle relaxed. A conjecture advanced by the author of these lines is to the effect that the phenomenon in question is perhaps to be explained in a manner analogous to the well known cracking which ensues when a finger is suddenly and forcibly pulled, and proceeds from a sudden separation of the closely contiguous surfaces of the articulation between the malleus and incus; it supposes that the abrupt outward flexure of the membrane may produce an instantaneous and audible severance of these surfaces, of which the first must follow the membrane, while the last cannot, on account of the unyielding ligature of the stapes. This conjecture it would be difficult to verify, wherefore we shall not dwell upon it, but return to the dullness produced in the perceptions of the organ by the contraction of the muscle of the malleus.

The intention of this superinduced dullness of hearing is apparent from what has been said above; it consists in preventing a too strong and dangerous concussion of the tender nerve fibres by unduly intense undulations of the medium of sound. That this end should be attained, it is unconditionally necessary that the tensor muscle should, at the right time—that is, when air-waves of a certain intensity enter the organ—be brought into action, and this really takes place, although the "how" has not been indubitably shown. It is thought by many that the will provides for the seasonable interposition of the muscular contraction; that is to say, that, if a very strong sound reaches the sensorium, the will calls into service the conscious reaction of the suppressive mechanism. This is neither proved nor probable, for the will would be a very precarious sentinel. In sleep, or when the attention is wholly diverted from the perceptions of the organ, the will would not fulfil its duty, and the nerves would be abandoned to the mischievous effects of immoderate sound-waves. The security would be much greater if the operation of the muscle were brought about by a simple but sure mechanism, which should be set a going by the air-waves in a degree corresponding to their intensity. And this is doubtless the case; the excitation of the nerves of movement takes place, in all probability, through a reflex action of the nerves, just as it is by this reflex action that the pupil of the eye is narrowed, and the dazzling effect of too intense light-waves thereby diminished. If a very intense wave of sound reaches the ear, so that the auditory nerves are thrown into strong excitement, these immediately transfer in the brain that excitement to the nerve fibres of the tensor muscle with which they are in direct communication by means of a cellular apparatus; the centrifugal excitement thus propagated, (the nervous current), on arriving at the muscle, determines a contraction of its fibres—a contraction which will be more considerable, the stronger the excitement and more forcible the air-wave. In this way, it is true, the tension of the tympanum, which serves for deadening the sound, comes somewhat *post festum*, since already a part of the wave has been conveyed with undiminished intensity to the fluid of the labyrinth and the nerves of hearing; that part, namely, which calls the reflex action of the nervous excitation into existence. But it suffices probably for the protection of the nerves of hearing, if only a long continuance of the violent concussion is spared them; but, from the rapidity with which the nervous current is propagated, and the shortness of the route through a nerve of hearing to the brain, and thence again through the nerves of motion to the tensor muscle, that part of the waves of sound which takes undiminished effect will, in most cases, be excessively small in comparison with that whose intensity is broken by the obtunding apparatus. In the eye it is no better. Here also the path of the reflex action, by which the protective apparatus is brought into service, lies through the nerves of the sense itself: the strong excitement of the optic nerve by dazzling rays of light is transferred in the brain to the motive nerves of the so-called iris, and determines this to a contraction of the orifice by which the rays are admitted. So much concerning this corrective apparatus to the ear. Respecting the operation and uses of the small and short muscle called the stapedius, which springs from the wall of the cavity opposite to the stapes and is attached by its tendon to the neck of the latter, there prevails much greater uncertainty; we shall, therefore, spare our readers a discussion of conjectures which are untenable, or, at least, destitute of proof. Possibly this muscle also is destined to the purpose of lessening the effect of intense air-waves.

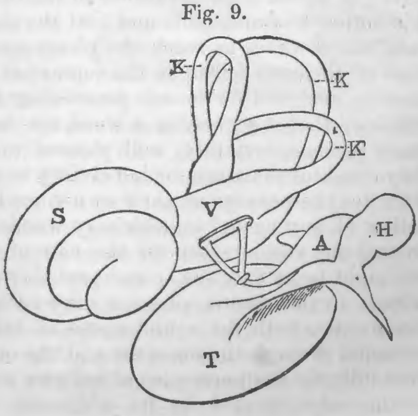
Before we direct our attention to the labyrinth and the apparatus of the nerves of hearing, we should briefly consider some of the relations borne by the *cavity of the tympanum* and the duct called the *Eustachian tube*, by which it is connected with the cavity of the mouth. Although not the slightest doubt can exist as to the immediate signification of these parts, and there are no grounds for supposing this explanation of their existence inconclusive, yet much ingenuity and fruitless pains have been employed in vindicating for them far other and the most singular purposes. Obviously, the cavity and the tube must exist in the ear as the air-space and air-hole in an ordinary drum. The membrane of the tympanum could not vibrate were it not surrounded on both sides by air; the small bones of the ear would be immovable if, instead of being in air, they were fixed in some mass of solids. The air in the cavity, since it would be compressed by the inward curvatures of the membrane, would oppose resistance to the movements of the latter if it did not find in the tube a ready issue when the membrane is driven inwards, and ready entrance when the membrane again swings outwards. This is so simple and clear that all further explanation would seem unnecessary. We shall. however, briefly notice some of the hypotheses advanced on this subject, and hope to show, even to the uninitiated reader, how untenable they are. It has been surmised, for instance, that the waves of sound which the vibrating membrane of the tympanum must communicate as well to the air contained in the cavity as to the ossicles cannot properly be supposed to be lost, but must also be available for the use of the organ of hearing, just as if it were a criminal prodigality of nature if every one of these waves which impart their effect with such constant and beneficent impartiality to all that can vibrate did not reach the auditory nerves of man or beast. The ossicles of the tympanum are so apposite, certain, and sensitive an apparatus for the propagation of these sound-producing undulations, that there needs no second route for conducting them to the nerve; but were such an one really necessary, nature could have chosen none more unsuitable than that through the air of the tympanic cavity and the Eustachian tube. For these self-propagating waves of the air contained in the cavity, there is but one conceivable manner in which they could reach the nerves; no one would think of asserting that they could be transmitted to the osseous wall of the labyrinth, from this to the enclosed fluid, and from this to the nerves; but the membrane which has been mentioned above as closing the *fenestra rotunda*, the opposed opening of the labyrinth, and which is pressed on one side by the air of the cavity, on the other by the fluid of the labyrinth, has been seized on as furnishing the requisite mechanism. It has been contended that the air-waves of the cavity must throw this membrane into vibration, just as the waves in the external meatus of the ear do the tympanic membrane, and that the vibrations of the former in the fluid of the labyrinth would be as capable of creating waves for the excitation of the nerves as the movements of the stapes in *the fenestra ovalis.* In itself this last position is not to be controverted; but it is to be inquired, first, whether the membrane is really thrown into sensible vibration; and secondly, whether its oscillations can take effect upon the nerves simultaneously with those propagated through the ossicles of the tympanum. We do not hesitate to answer both questions in the negative. To be thrown into vibration by air-waves, a membrane must receive their impact, as far as possible, in a perpendicular direction. But this membrane of the fenestra rotunda lies as unfavorably as possible in regard to waves proceeding from the tympanum, inasmuch as it is turned away from the tympanum and towards the Eustachian tube, so that the waves can as little strike it directly as, in the dark, one could strike a disk whose edge instead of its flat surface was presented.

Now, that it is not inaccessible to the waves is certainly true, since its surface fronts directly on the air chamber; but its position would much sooner suggest the idea that it is, as far as possible, sheltered from the effect of these waves than that it is designed for their reception and transmission. But even if we admit that it is, in a slight degree, movable by the waves of the tympanic cavity, it is not difficult to show that these movements must be overborne and annulled by the opposed movements simultaneously imparted to it by the fluid waves proceeding from the stapes. When an external wave of condensation drives the membrane of the tympanum inwards, the stapes is driven deeper into the fenestra ovalis and the fluid of the labyrinth is propelled before it; but this propulsion is only possible, as has been shown, through the simultaneous outward curvature of the opposite membrane, that, namely, of the fenestra rotunda. Now, the same inward movement of the tympanum generates simultaneously a condensed wave in the air of the tympanic cavity, and if this wave strikes the membrane of the fenestra rotunda it cannot do otherwise than drive this membrane inwards, and therefore in a direction opposite to the former. But as the waves of the fluid in the labyrinth will, no doubt, act with more force on the membrane than the air-waves of the cavity, the former must prevail and the membrane curve outwards in spite of the latter force. Were the preponderance reversed, it is manifest that any movement of the fluid by the stapes would be impossible, and the whole system of ossicles would be brought to a stand. Hence the sheltered and most unfavorable position of our membrane as regards the reception of the air-waves would seem to have been wisely ordered, and to be an indispensable condition for the propagation of the sound through the small bones of the ear to the labyrinth.

Other hypotheses respecting the intent of the cavity of the tympanum and the Eustachian tube seem to have as little foundation as the above. It has been thought by some that the tube was established for the purpose of conveying waves of sound from the throat to the organ of hearing. When convinced that this notion was untenable and that the tube could be of no avail in conducting such sound-waves as reach the pharyngeal cavity from the outer air, the same class of theorists fell upon the somewhat *naive* idea that the tube in question must be designed for sounds proceeding from the organs of speech in its close vicinity; that it serves, in a word, for *hearing our own voice.* Now there are many persons, certainly, well pleased to hear themselves speak, who would be duly thankful to nature for her civility in establishing quite a special and private route for the passage of their own voice besides the common one into which the bellow of any casual bullock may wander. But, in the first place, nature has created our voice rather for the ears of others than our own, and, except that we might learn to speak, cared probably little about its being audible to ourselves; in the second place, it may be shown that, setting aside any possible concurrence with the sound-waves of bellowing bullocks, the common channel of sound through the outer air and the outer ear to the tympanum not only suffices fully for the perception of our own voice, but, under ordinary circumstances, is the only channel for its admission. The Eustachian tube is not by any means always thoroughly open, but usually its walls lie close to one another, although so loosely that every inward curvature of the tympanum readily separates them for the expulsion of a portion of the air within the cavity. It opens not so readily to the air of the pharynx; a peculiar effort is necessary to effect an entrance of the air in the above mentioned experiment. In ordinary speaking and singing this effort is never used; the air coming from the lungs chooses the broad and unobstructed outlet of the mouth or nostril, and passes by the orifice of both ear-tubes without opening them; consequently, no sound-producing air-waves come ordinarily by this channel to the organs of hearing. That happens only when, in speaking or singing, we make a peculiar effort, and then indeed we suddenly hear our voice, which before sounded as coming from without, with deafening force and as if it originated in the ear itself. This last circumstance is of particular significance; we shall see further on that, in idea, we then only judge the source, which we regard as cause of a sound, to be *without* us, when the waves thereby generated reach our organ of hearing through the external meatus of the ear; but whenever it happens that this is impervious to the air-waves, we seek the source of the sound *within* ourselves, even when it is really without. There can, therefore, be no question of hearing our own voice through the Eustachian tube, not to say of its destination for this purpose. We could submit more of such hypotheses to criticism were it worth while. Much has been idly said respecting a strengthening of the waves of sound in the cavity of the tympanum through *resonance*, but a closer examination of these suppositions would require a special investigation of the physical nature of resonance, its conditions and laws, which would lead us too far, and, candidly to speak, be difficult of explanation to novices in this kind of knowledge.

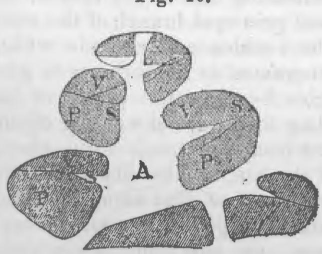
We leave now the outworks of the organ of hearing and enter upon the proper citadel, the recess in which the ramifications of the auditory nerve are distributed for the reception of the waves of sound, for whose conveyance the apparatus we have been considering is destined. This recess, to which the name of *labyrinth* has been given, is, as that name implies, a wonderfully complicated structure, in which, though we need no thread of Ariadne to lead us aright, we everywhere encounter mysterious arrangements, whose full acoustical and physiological understanding is reserved for future times. The labyrinth is a cavity surrounded with walls of bone and filled with a fluid, having two openings, of which one (the *fenestra ovalis*) is closed by the stapes and its annular ligament, the other (the *fenestra rotunda*) by a membrane; this cavity has not, however, the simple shape of a sack, but consists of many compartments and canals in a winding form. If the collective labyrinth be carved from the so-called petrous bone, out of the substance of which it is channelled, it presents, when seen from without, the appearance of the accompanying figure. It consists of two chief divisions, the *vestibule* V with the *semicircular canals* K, and the *cochlea* S. We will consider first the vestibule and the canals. The proper vestibule, which forms the middle part of the whole labyrinth, is ovoidal in shape, and measures about one-fifth of an inch from before backwards and a little less from above downwards; its wall exhibits eight openings. One of these is the already mentioned *fenestra ovalis*, which leads into the cavity of the tympanum and receives the foot-plate of the stapes; a second leads immediately into the spiral canal of the cochlea; a third passes to a narrow channel bearing the

Fig. 9.



name of *aqueductus vestibuli;* the remaining five openings lead into the three semicircular canals, while each of these canals has its opening orifice in the vestibule and runs back with a terminal orifice into the same; but as one of these apertures is common to two of the canals, there are, in effect, only five of them instead of six. The vestibule itself shelters in its water-filled cavity two little membranaceous sacs, also filled with fluid and imbedded in separate depressions *(foveæ)* of its wall, being channels for the nerves, of which we shall hereafter speak. The semicircular canals are narrow bony tubes, bent somewhat into the shape of a horseshoe, enlarged at their commencement and termination, and from their position and direction are distinguished as the superior, posterior and horizontal canal. As nothing results from their special position, we will not detain the reader with the pedantry of a scrupulous anatomical description of them; the above outline conveys an adequate idea of their form and direction. In each of these canals is fixed a membranous tube, which has the same form as the canal, as if it were a reduced cast of it, and dilating therefore at the beginning and end of the canal into a small roundish cavity called the *ampulla.* The interspace between the membranous tube and the osseous wall of the canal, as well as the tube itself, is filled with a watery secretion, which is in immediate communication with that of the vestibule. The sack-shaped origin of each membranous tube is a conduit for the nerves. The cochlea well deserves its name; the best idea of its form may be acquired by observing the shell of the common garden snail. As in this shell a spiral and constantly diminishing channel winds, from the base to the apex, around a central spindle, so the interior chamber of the cochlea consists of a similar screw-shaped canal, which describes, with a like progressive diminution, two and a half turns around the *modiolus* or central axis, terminating, not in a point, but in a funnel-shaped dilatation. A more striking difference between the shell of a snail and the cochlea consists in this: that the canal of the latter is not single, but is divided, throughout its length, by a transverse partition into two passages, one of which runs above, the other below the dividing wall, but which communicate with one another in the funnel-shaped expansion; resembling in some sort a winding staircase by which one might ascend to the top, and, stepping over again, arrive at the bottom. An idea of this arrangement may be conveyed by the annexed figure, which represents a central longitudinal section of the cochlea from its base to its apex. A denotes the central axis, S the partition wall running around it, V and P the winding passages separated from one another by this wall. These passages bear the name of *scalæ* (stairs); that above the partition, marked V, being called the *scala vestibuli,* that below, marked P, the *scala tympani.* They owe these names respectively to the circumstance that the former communicates, by an oval aperture, with the vestibule, while the latter is closed below by the membrane of the *fenestra rotunda,* which faces the cavity of the tympanum.

Fig. 10.



Both passages are filled with fluid which, at the entrance of the scala vestibuli, is in immediate communication with the fluid of the vestibule. The partition S, which divides the canal into two passages, is constituted, as the figure represents, by a thin lamina winding spirally around the axis, to which it is attached by its inner edge, while its outer edge adheres to the outer wall of the cochlea. It consists of two *zones,* as they are called, united with one another, the one osseous, the other membranous, which together extend quite across the canal; that part of it which lies contiguous to the axis being composed of two thin lamellæ of bone and reaching as far as the middle, while the outer part, by which it is continued to the opposite wall, consists of a soft membranous substance, serving, as we shall see, to support the tender apparatus of the nerves.

The description in its several parts of the auditory nerve, which distributes filaments in the cavity of the labyrinth for the reception of the advancing waves of sound, is rather a precarious task, which we may facilitate however, without any real loss to the reader, by forbearing to enter into those subtle details which, if a novice, he would scarcely understand. Nor will this be wholly a sin of omission, for, truth to say, the wonderful apparatus here displayed to us by the microscope is, in its physiological signification, not much less than altogether unknown to the physiologists themselves. It is only to-day, so to speak, that the coryphœus of our science has succeeded in detecting the true anatomical constitution of the extremities of the nerves of hearing in the cochlea and vestibule, so difficult is it to submit uninjured to observation these infinitely delicate structures, hidden deeply in the skull and shut up within osseous walls, and to grope one's way in the astonishing complexity of the elementary tissues which the microscope reveals in them. The auditory nerve, running back from its origin at the base of the brain, issues, after a short course through several openings in the petrous bone, into the labyrinth, and here divides into two principal branches destined for different parts. Each penetrates by a separate aperture, the one into the vestibule, the other into the cochlea. The former again divides into numerous filaments which direct their course partly to the two membranous sacs of the vestibule, partly to the membranous *ampulla* situated at the beginning of the semicircular canals, where their extremities are distributed something after the manner of a delicate brush. These minute fibres, each of which forms an isolated conductor of the nervous current, terminate finally in delicate bulbs or nerve cells. It is a remarkable peculiarity that at the terminal expansion of the nerves of hearing a finely grained calcareous substance is interspersed through the delicate fibrous tissue of the vestibular sacs, consisting, as is clearly apparent under the microscope, of minute but distinctly formed crystals. Chemical investigation has shown that this *otolith* or "sand of the ear," consists of a compound widely dispersed through animate and inanimate nature, the same which, under the name of chalk or marble, forms mountains and rocks, and which, as shell, protects from outward injury the embryo of the feathered tribe; in a word, of *carbonate of lime*. It may be mentioned by the way that this calcareous matter is not peculiar to man, but occurs also in the organ of most animals, partly as a finely crystallized mass, partly in larger or smaller crystals; its significancy as respects the hearing has as yet resisted all efforts of conjecture and experiment. The second principal branch of the auditory nerve, the cochlear, passes to the base of the cochlea and ascends within its axis to the apex, becoming more and more attenuated as it continues to give out filaments which proceed from it at right angles between the plates of the bony lamina, to their outer border. Corresponding to the spiral winding of this septum or partition wall, the filamentary offshoots from the trunk form also a spiral line. At the edge of the bony portion of the lamina these filaments issue upon the membranous portion and pursue their course at first along its under surface and consequently within the scala tympani, but presently pass through different small foramina into the scala vestibuli and now run transversely along the upper surface of the membranous lamina towards the outer wall of the cochlea, in the neighborhood of which they terminate. That part of these filaments which traverses the upper surface of the membranous lamina in the scala vestibuli, is, however, no longer the simple nerve fibre which we observed in the trunk and in its course between the plates of the bony portion of the lamina; it has become a singularly complex structure with cells, partly inserted in its progress, partly as appendages of the stem. It would require no little detail to give an exact account of the microscopic constitution of these nerve extremities, nor would our inquiry even then be much advanced, since physiology is still greatly in the dark as regards this delicate mechanism. The reader will perhaps obtain the best idea thereof if he imagines himself ascending, suppose in the scala vestibuli, by a real winding staircase, the stairs of which consist of many thousand small and closely contiguous steps, each step being represented by the transversely directed terminal part of one of these nerve filaments. It is still in dispute whether this terminal apparatus floats freely in the fluid of the scala vestibuli, or adheres by its outer end to the membranous lamina; whether it is covered over by a second delicate membrane and thus is enclosed between two membranes, &c.

We proceed now to a consideration of the transmission of sound in the labyrinth. We have seen that the waves of sound are communicated to the fluid of the labyrinth by the piston-like movements of the stapes in the fenestra ovalis, and have been led to recognize the corresponding out and inward curvatures of the membrane of the fenestra rotunda as the indispensable condition through which the undulatory movement of the fluid is rendered possible. But how these waves are constituted, how propagated, what their special destinations in the different divisions of the labyrinth, in what manner they operate on the extremities of the nerves in the vestibule and the cochlea, for what reason these extremities present to them so peculiar an apparatus and arrangement, these are questions which press for an answer, and until answered debar us from a solution of the mystery in which the excitation of the nerves by the waves of sound is enveloped; the answers given to them have thus far, however, proved poor and unsatisfactory. The nature of the fluid-waves of the labyrinth is essentially the same with that of the air-waves; according as the stapes presses inwards or is withdrawn, there is a succession of waves of condensation and rarefaction, in which the single particles of the fluid, as they follow one another, act precisely as was above explained in reference to the particles of air. The course of the waves is, at least in its principal features, clearly and definitely prescribed; the waves must first be propagated into the vestibule, whereby the membranous sacs suspended therein, together with the extremities of their nerves, will be agitated. Were the vestibule entirely shut in, while only the fenestra rotunda, closed with its membrane, found a place at a point of the osseous wall, the waves would be restricted in their play, and would everywhere bound against the solid structure and be beaten back from it like the sea from a rocky coast. The repulsed waves would encounter those which were following, and disturb their action by giving to their advancing particles a contrary impulse. As, in this way, the whole effect of the waves in producing a regular excitation of the nerves would be frustrated, it is evidently of the utmost importance that the vestibule should be provided with the numerous openings which lead to the canals filled with fluid; these canals serve as conduits for the waves of the vestibule, which must necessarily traverse them as well as the long passages of the cochlea before reaching the fenestra rotunda, which lies at their further extremity, and which, by the yielding of its elastic membrane, alone renders the existence of the waves possible. The action and course of the wave in the semicircular canals are difficult to ascertain with physical accuracy; but, as regards the cochlea, it is at least clear that, since only the upper scala opens into the vestibule from which it takes its name, the wave must traverse the whole canal by this passage to the apex, then pass over into the scala tympani and descend by the latter, which lies underneath the dividing lamina, until it finally takes effect on the membrane of the fenestra rotunda at the base. During its course through the scala vestibuli it must strike in regular succession upon all the nerve extremities which cross that passage, and exert upon each of them, one after the other, its peculiar excitation. This is but a rough sketch, it is true, of the movement of the waves of the labyrinth, but it is nearly all that we know with certainty. Efforts have not been wanting to penetrate further, and especially to ascertain whether, where, and in what direction the waves are reflected or thrown back, whether this reflection produces an increased degree of undulating intensity through resonance (somewhat in the manner of the reflection of the waves of sound from the walls of a lofty apartment.) Numerous investigations have been instituted by the most ingenious experimentalists to throw light upon these subtle inquiries, but the results have been so scanty and uncertain that we abstain from the discussion of the different conjectures advanced, especially as the possibility of making them sufficiently intelligible must be doubted. It has also been a question, and one which has long been the subject of lively scientific discussion, to what end serve the different arrangements observed in the extension of the terminal portions of the nerves, first in the saccules of the vestibule and semicircular canals, in which the brush-shaped ramifications of the nerve extremities are struck simultaneously by advancing particles of the fluid; secondly, in the cochlea, in which these nerve extremities are so arranged behind one another that the waves must strike them successively one after the other; or, more generally, what special design have the cochlea, the vestibule, the semicircular canals? We might fill pages with the enumeration and criticism of the conjectures in which a solution has been sought, and would only see that most of them were destitute of proof, some even absurd. It has been supposed, for example, that the semicircular canals serve for a knowledge of the direction from which the waves of sound proceed. As if the perception of the direction could at all be the object of an impression of the sense; as if a wave of the fluid could telegraph to the mind, through the nerve which is struck, the point of the compass from which the excitation is directed; as if a different direction of the fluid-waves within the labyrinth could be the herald of a different direction of the air-waves without! We hope to explain presently in what manner we obtain an idea of the position in space of the external sources of sound. In regard to the cochlea, the opinion has long obtained favor that it is intended to conduct to the nerves contained in it the waves of sound directly communicated from the air to the bones of the head and propagated through these, while the vestibule is destined only for the reception of the vibrations transmitted through the auditory canal, the tympanum, and the ossicles. This assumption rests on the early but erroneous opinion that the nerve filaments of the cochlea terminated between the bony plates of the laminar partition. Since it is known, however, that they issue from these and terminate free in the fluid of the cochlear canal, this hypothesis has become untenable, for air-waves (and it is these almost alone which convey a sound to the human ear) are transmitted with so much difficulty and so much enfeebled to the bones of the cranium, and again with such difficulty and feebleness from these to a fluid (the cochlea fluid,) that it is scarcely conceivable that in this way a sound producing vibration could reach the nerves of the cochlea with a strength sufficient for their excitation. In short, the unpalatable but honest confession of ignorance is the only answer which can properly be given to the above questions.

We have finished our explanation of the progress of the auditory excitation in our organ of hearing; we have followed the sound-producing movement from its origin in the outward object (the vibrating string) through the external air, the external ear, the auditory canal, the tympanum, the movable bones, and the fluid of the labyrinth, to the nerves. We come now to the pith of the subject, the problem which it is the part of acoustical physiology to solve, to the question, how does this movement operate upon the nerves when it has reached them? How does it release in them the so-called "nerve current," which, proceeding through their fibres to the brain, there, by a certain apparatus, so works upon the soul that the latter instantaneously conceives a sensation of sound? We need not combat anew the irrational idea that the sound-movement, as such, is propagated in the nerves; that a sound wave, transmitted from particle to particle, operates as such upon the perceptive apparatus in the brain. We have in earlier articles sufficiently established the fact that the process itself in the nerve fibres is totally different from the external irritant of the sense which produces it; that it is a specific movement of the nerve molecules of a yet unexplained nature, and probably essentially the same in all nerves, whether it be generated by waves of light or of sound, by pressure or heat, by objects of taste or of smell. At the peripheral end of all the nerves of sense which we have heretofore considered, we have everywhere found or must suppose an appropriate terminal apparatus, whose function it is so to elaborate the excitation which is received from without, that a movement of the current in the fibres of the nerve shall ensue. This is the case with the nerves of hearing. It is nearly certain that it is not the simple mechanical agitation of the filaments of the auditory nerve which excites them, for this agitation excites no other nerve, yet is the nerve of hearing as like to all others as one copper wire to another; the wave of sound must first call into existence some other action which is capable of stimulating the nerve fibre. Of what nature it is, and how the waves of the fluid of the labyrinth produce it, is wholly unexplained; we can only designate with certainty the seat and instruments of this enabling action; there are here as elsewhere, at the outer ends of the nerves, the minute fibres and bulbs which the microscope discloses to us. Still less have we any intimation how the "current" of the auditory nerves, generated mediately by the undulatory movement, acts upon the brain and the soul, so that the latter forms the specific conception of sound. We can here only refer to the general reasoning by which, in previous essays, it has been shown that it is the quality of the *inner* central and terminal apparatus of the nerve fibres which determines the characteristic effect of the current, the nature of the sensation. Who knows whether, with the sounding plummet of experiment, we shall ever succeed in exploring these deepest mysteries of existence?

The final result of every excitation of the auditory nerve produced by a movement of the kind we have been describing is a spiritual incident, which, in general terms, we denominate a sensation of sound, but of which we distinguish several qualities. We contrast a sensation of tone with mere noise; we divide sounds according to their "*pitch*" and their tone; each of these qualities again is susceptible of an endless gradation of *intensity* from the lightest, only to be perceived by the most strained attention, to that which seems to penetrate us by its intolerable force. The qualities, like the intensity of the sensation, are determined by certain *momenta* referable to the nature of the external excitation; thus the pitch of the sound depends on the number of the waves which in a given time reach the ear; the tone on the form of the waves; the intensity on the magnitude of the displacement which the wave communicates to the particles. The explication of these relations is thus far exclusively the affair of physics; we shall not here enter at large into this branch of learning, because it is at present really not susceptible of a physiological interpretation; that is to say, although we know from experience that a determinate, subjectively recognized sound is causatively conditioned on certain properties of the external movement which are very imperfectly understood, we are unable to explain the *why;* to specify how this or that form of movement acts upon the ear; how it operates upon the nerve; much less can we describe the specific quality of the nervous current, which is its result, or, in the last place, the quality of the sensation. Referring the reader, therefore, to the popular expositions of writers on physics, we shall confine ourselves here to a few observations. A sound of some definite pitch arises when a sound-producing body—a string, for instance—executes a certain number of vibrations in a unit of time, so long as this number is not too small or too great. In other words, the vibrations are only audible, only capable of producing the appropriate excitation of the nerves, within certain limits of frequency. Under favorable circumstances we can hear a sound—the deepest that is possible—when a string, an air-column, &c., executes sixteen vibrations in a second, while with yet slower vibrations the auditory nerve, under all circumstances, remains at rest, be the intensity as considerable as it may. On the other hand, there is also a maximum velocity with which alone the sensation of sound—the highest possible—consists; this upper limit of the scale of sounds is not accurately ascertained, because with different organs of hearing it is now higher, now lower. It is held, however, that a sensation of sound no longer arises when the number of vibrations exceeds 64,000 in a second. This fact is well suited to show that sound exists not without, originates not in, the vibrating string, but in our own sensation; else were it difficult to see why, with a certain lowest and highest number of vibrations, the laws of movement remaining wholly unchanged, this movement should lose the property of sound. Very conceivable is it, on the other hand, that the auditory nerve ceases to react when the vibrations which strike it follow one another too slowly or too rapidly. An exact physiological explanation of this restriction of the irritability of the auditory nerve to certain limits it is not in our power to give; but it is interesting to observe that there is yet another nerve of sense which affords a complete analogy to this state of relations—the nerve, namely, of vision. As the nerve of hearing is excited by the waves of sound, propagated to it from solid and fluid matter, so is the optic nerve excited by the waves of the luminous ether, which are subject to the same general laws of undulatory movement. As waves of sound of different frequency produce an impression of different qualities of the sensation of sound or a different pitch of tone, so waves of light of different frequency give rise to perceptions of different color, but, as in the case of the auditory nerve, only within certain limits of rapidity. If more than 697,000,000,000 or less than 439,000,000,000 of vibrations of the luminous ether follow one another in a second, the optic nerve is insensible of any impression, even when the vibrations undoubtedly reach it. Between the limits above assigned for the sense of hearing lies an endless series of differently qualified sensations, every possible number of vibrations between 16,000 and 64,000 producing a modification of quality—a *sound* of definite pitch. Yet we mean not thereby to say that our faculty of discrimination among these different qualities is itself unlimited, or, in other words, that we are conscious of a difference of quality in the sensation when two undulations of different rapidity strike the ear near or after one another, how small soever the difference may be. On the contrary, it is a fact easily substantiated that most men have originally but "a poor ear," but that, through use, a tolerably high degree of delicacy of hearing is attainable; thus, while many scarcely recognize two sounds as of different pitch, one of which is produced by 800, the other by 810 vibrations in a second, there are musicians who seize the difference with accuracy, and distinguish the higher from the deeper tone when the number of vibrations is 1,200 in the one, 1,201 in the other. The auditory nerve, or, more properly, the organ of sensation on which it operates, is in this respect subject to the same conditions with the rest of the organs of sense, being susceptible, through judicious and attentive use, of improvement in its performance, and specially in its faculty of discriminating between sensations of different quality.

A highly interesting question, but one which unfortunately has set at naught every attempt at explanation, arises out of the fact that we can perceive simultaneously a great number of sounds together, and distinguish the pitch, tone, and strength of each of them. The harmonious association of different sounds may often seem to the uninitiated a chaos, from which he is unable to distinguish the individual members, while to the musically cultivated it is no difficult matter to disentangle from the fullest instrumental or vocal orchestra each sound signalized by a special pitch or tone, to follow the melody through all its inflections, and clearly perceive all the transitions; but even the laity know from experience that it is possible to single out an individual voice from the buzz of a company all speaking together. The art consists only in fixing the attention inflexibly on the tone or modulation which we desire to separate from the rest, and in this respect also use refines and strengthens the discriminative faculty of the sense. At first glance, perhaps, this fact may not seem very singular; not more so than the simultaneous perceptions of other organs—the skin and the eye. Innumerable impressions on the skin are simultaneously perceived, and, in idea, locally separated from one another, if the space between the impressions is sufficient; and when they are situated close to one another the mind takes cognizance of their number, and from that and their position determines the size and form of an external object. So is it likewise with the eye, which can perceive simultaneously so many objects, and direct the attention at will to each of them as sensible and contiguous points on the retina. The analogy of this power of local discrimination in the impressions of touch and sight with the simultaneous hearing of several tones cannot be doubted, but, while for the first the explanation is simple and obvious, an analogous interpretation is not applicable to the sense of hearing. In the organs of touch and sight the severance of the simultaneous impressions rests on the local severance of their external exciting causes; that is to say, on the excitation, through separate impressions, of different conducting nerve fibres isolated from one another—a point which we had occasion to discuss when treating of the sense of touch. Could it be shown that in the ear likewise the undulations pertaining to different sounds acted upon different nerve fibres, a severance of impressions, in conformity with the number of single fibres excited, would be a necessary postulate. But no proof of this is possible; on the contrary, a consideration of the circumstances under which sound is transmitted in the organ of hearing forces us to the conclusion that each wave strikes upon all the nerve fibres, and that a number of undulations, originating near one another, must blend in a single movement long before they reach the reservoirs of the nerves. It is clear that the tympanum cannot possibly vibrate at the same time in conformity with the impulse of several undulations, slowly with the deep, rapidly with the high pitch, with this and that acceleration, according to the character of the tone, in such wise that the simultaneous sounds shall be transmitted, without reciprocal disturbance, from the tympanum, through the intermediate organs, to the labyrinth. Just as a body on which a blow from different directions simultaneously takes effect, cannot proceed in the direction of both at once, but will take a middle course, so must the vibrations of the tympanum take, according to the laws of mechanics, a middle form, resulting from the different concomitant impulsions; and hence only a single undulation, as representative and result of all those generated outside, will be transmitted to the labyrinth. Of any contrivance in the labyrinth by which this compound movement may be again resolved into its original single elements, and each element—that is to say, each undulation belonging to a determinate note or tone—be directed to a special organ, and thus transmitted to the brain, we know nothing and can form no conception. In advance, therefore, it only remains to suppose that this resultant undulation, as such, strikes on its passage and excites each successive nerve, and that it is the soul which first resolves into its constituent elements the compound sensation arising from without; but *how*, is an inexplicable riddle.

We leave the consideration of the pure sensations of hearing, and turn lastly to a brief discussion of the ideas derived from this sense and their mode of origination. As with all the rest of the senses, the sensations first acquire value and serviceableness for the intercourse of the soul with the outer world by their association with certain ideas, which, in the previously educated sense, are so unconsciously and infallibly identified with the sensations, that we take them for the sensations themselves. The sensation of hearing, like every other sensation of sense, is a purely subjective process, a transformation, it might be said, of our consciousness. The cognizance of the existence of an external source of sound as cause of the sensation, the perception of its direction and distance in outer space, pertain to the idea which attaches itself, through a physical operation, so closely to the sensation, and becomes so blended with it, that we seek not only the source of sound outside of our own ear, but the sound—that is, the sensation itself, with all its qualities; that we think the sound penetrates as such from without through the portals of hearing to our soul. The newly-born child hears like the grown man, has the same quality of sensation, but he knows not whence the sound comes, knows not that it comes from without, or, indeed, that there is anything without him. It learns gradually to distinguish outer objects from its *I*, its *Ego*, and thus comes gradually to recognize the cause of the sensation of sound as something existing outside of itself. We have, as a general thesis, discussed in previous essays on the senses the process by which this objectivity is made familiar to the human mind; we shall here, therefore, only consider the method in which the process is specially conducted as regards the sense of hearing. A fact which at once guides us to a right point of view is this: that only under a certain condition do we conceive an outward object to be the cause of the sensation of sound, and when this condition is wanting, seek the source of the sensation within ourselves, even when we know and are directly informed by other senses that it lies without us; that condition is, that the sound shall be conducted to the auditory nerve through the channel which we have been above anatomizing, the outward ear, the tympanum, and the ossicles. If by any means whatever the transmission of the air-waves to the tympanum and the vibration of the latter be prevented, every sound which, being conducted by the ossicles, reaches the auditory nerve with sufficient force, seems to us to originate within ourselves. This is most readily and strikingly shown by immersion in water. According to the researches of one of our most ingenious physiologists, the tympanum under water only so long conveys the waves of sound, or propagates their oscillations to the labyrinth, as the external passage of the ear is filled with air; if it be filled with water, so that the outer surface of the tympanum is bathed by it, the transmission of waves of sound takes place only through the ossicles or movable bones; but with considerable intensity, since the bones, though as solid bodies they take up with difficulty sound-producing undulations of the air, take up with facility sound-producing undulations of the water. If now, while one is immersed, a sound under water be caused by another person, as, for instance, by striking two stones together, this sound, so long as the auditory canal contains air, will be ascribed to an external source, and its direction be correctly determined; but so soon as the passages of the external ear are filled with water, the sound, from whatever direction it may proceed, will seem to originate within the head. The experiment may be variously modified; and since in the form just described it is somewhat inconvenient, the following method may be resorted to: let the inquirer modulate his voice to a tune, and while continuing to do so, suddenly close both ears firmly with his hands; immediately an entirely different sensation will ensue, the tone of his own voice, which, with the ears open sounded as if from without, will on the instant of the closure seem to arise from within. Since it cannot, then, be doubted that the vibrations of the tympanum in some manner determine the objectivity of our impressions of hearing, it is worth inquiring what part that instrumentality plays. The most probable conjecture is the following: the tympanum is very richly provided with nerves of feeling, which, like the nerves of touch of the rest of the surface of the body, are adapted to sensations of touch and of common feeling; these nerves are thrown into excitation when the tympanum vibrates, and generate a sensation of touch, of the quality of which we are not specially conscious, because it is, as it were, dulled by the sensation of hearing which constantly accompanies it, but which, like every impression on the skin, is associated with the idea of an external object as the exciting cause. As we refer this idea obtained through the nerves of touch to the simultaneous auditory sensation, and, indeed, falsely identify it therewith, we arrive at a perception of the objectivity of the source of sound, without being aware of the circuitous manner in which we acquire it. Here is again an example how one sense comes to the aid of another, how two perceptions of sense interlink with one another, in order to make one or the other intelligible; this important help does the sense of touch, in community with the "muscular sense," afford to the sense of hearing, inasmuch as they inform us of the direction in which lies the external source of sound. In the first place, the sensibility of the tympanum, just alluded to, furnishes us the ready means of deciding on which side of us the source of sound is situated, since we naturally seek it on the side of that ear whose tympanum, being more strongly agitated by the wave of sound, communicates to us a more intense sensation of touch. The muscles which move our head convey to us, through the sensations continually connected with their action, accurate ideas of the position of that part of our frame, and consequently of both our organs of hearing, so that we are conscious of the position in space of the more strongly vibrating tympanum. If the difference of these sensations of the tympanum is not sufficiently distinguishable to afford a certain judgment, we turn the head on the cervical axis hither and thither until the sensations of touch and hearing acquire greatest intensity in one of the ears, and then present this ear, whose position the muscular feeling teaches us, in a right line to the source of sound, for we know by experience that in this way a given sound is most distinctly heard. If the sensations be equally strong in both ears, we have no alternative but to seek the intonating body directly before or behind us. Most probably, in discerning whether it is before or behind, above or below us, we are guided by another class of sensations of touch, that, namely, which results from the action of the air-waves on the cartilaginous outer ear or auricle. There are simple experiments which show that this member materially assists in the discernment of the direction from which sounds proceed. If we press both auricles closely to the head, we lose the power of discerning with certainty whether the source is before or behind, deceive ourselves easily, and, if the object is before, seek it behind us. If we firmly compress the auricles, and, moreover, place our hands flat in front of both organs so that they shall stand like the auricles, only on the wrong side, we regularly confound before and behind, on an impartial examination of the impressions received. But for those who would repeat these experiments we must add that prepossession in such trials is not easily avoided; we must eliminate with the utmost care all possible tokens, from which, by the help of the other senses or from experience, we might form a judgment of the position of the object. It is not easy to explain with certainty in what manner the auricle, through its perceptions of touch, leads to a determination of the direction of sounds; but it cannot be doubted that, as regards this function of the organ, we gradually learn to associate the different qualities of the sensations, which is the necessary result of the impact of differently directed waves on different parts of the auricle, with determinate ideas of place, just as we gradually learn by experience to form a correct judgment in respect to all the impressions received through our widely organized sense of touch.

The faculty of estimating the remoteness of a source of sound is also one which we acquire through the indirect process of experience. In many cases it is the sense of sight which enables us to form a judgment of the distance of the object which generates the sound. If we do not see that object, the judgment is only then in some degree exact when it relates to definite sounds or noises of which we have had sufficient experience to be able to estimate the diminution of intensity with the increasing distance. Thus is faithfully inculcated upon our memory the intensity of the impression of a human voice in ordinary speech or loud calling, in immediate proximity and at all distances; hence it is that we are capable of estimating with considerable correctness the remoteness of an individual from the intensity with which his voice reaches us. Various circumstances, such, for instance, as a strong wind opposite in direction to the sound, may, indeed, render our judgment uncertain; yet in this case also we possess to a certain degree experiences respecting the influence of such circumstances, which serve at least for an approximately correct estimate of intervening distances.

We hope to have conveyed to the reader of the above sketch, in as intelligible a form as possible, what is correctly known of the mechanism and action of our organ of hearing. At least, let not the inquirer who turns away unsatisfied from these pages impute it as a fault to the author that he has, so often and upon so many important points, been obliged to accuse science of ignorance. It is, on the whole, well for us whose lives are dedicated to such pursuits that science still hides under a thick veil so many precious treasures to reward the assiduous research and penetration of future explorers.

V.—THE SENSE OF SIGHT.

When his corporeal mechanism was giving way and refused further service to the exalted genius of our greatest poet, its last effort in obedience to the parting spirit was the cry: "Light! more light!" These words are ambiguous and have been differently interpreted; their sense, however, lies more on the surface than that which has been often assigned to them. There was here nothing mystical; no solemn decoration thrown around the close of a glorious life. It was the failing sight—the death of the most precious sense—which, pressing upon the yet lingering consciousness, called forth the wish implied in the expressions we have quoted. To thousands and thousands of the ordinary dwellers upon earth must the hour of death have brought the same mournful perception of withdrawing sensibility, alike whether they were still able to express it in words, or whether the organs of speech no longer obeyed the impulse, or the impulse itself were wanting; alike in whatever form, whatever words, the soul made known its last impressions. The simple expression, so often heard, "It grows dark before the eyes," is in its origin and signification wholly identical with the dying utterance of Goethe.

A glance at the physiological conditions of the sense of sight renders the frequent positive perception of its extinction in the case of the dying fully conceivable, while that of every other sense takes place for the most part without any distinct recognition. No one remarks when the organs of taste and smell resign their activity; few, except under peculiar circumstances, perceive when the auditory nerve loses its susceptibility to the impressions of the waves of sound which reach its extremities; and quite as few are conscious when the skin surrenders its function of recognizing warmth and pressure; but to most, perhaps to all men, whose consciousness is not extinguished before the death of the senses, the perception of a darkening of the organ of vision makes itself so urgently felt as to arouse the already slumbering consciousness to one last, painful effort, and communicates a reaction by the wonted paths to the organs of speech. But whence the perogative of this one sense? The answer is not far to seek. No sense is during our whole life so uninterruptedly active; on none is the attention of the soul so constantly bent; the functions of no other sense supply so habitually and variously the materials of thought and imagination. So long as we are awake the eye conveys to the mind in unbroken succession images upon which it acts, whether the other senses be simultaneously in activity or repose; even while concentrating, for example, with feverish solicitude, our attention on the sensations of the ear, no impression of the sight escapes us, however evanescent may be our consciousness of the perception. While we close our eyes in order to think with less distraction—while we slumber and dream—the activity of the sense of sight continues amidst our thoughts and dreams, the objective perceptions which are wanting being supplied by subjective ones; nor do we lose in dreaming the distinction between darkness and light. What other sense can boast of being equally indispensable? Hours and even days may pass without action on the part of the sense of taste or of smell, and without attention being paid to the absence of their sensations; and although we dream of scents and of tastes, it is not with the same necessity and constancy with which we dream of objects of sight. Even the auditory nerve may long be at rest without consciousness on our part of the stillness, if no sensational momentum summons the attention to watchfulness; nor is it at all different as regards the sense of touch. Hence, because we are accustomed to hold uninterrupted intercourse with the outer world by means of the eye, and constantly to derive from its perceptions the incentives to thought and action, the perception of a failure of the visual power is unavoidable even to the parting consciousness; hence the cry of the poet: "Light! more light!" as the eyes with which he had looked through nature and the human heart were extinguished in death. The construction which I have put on the dying words of Goethe is, I think, clear and consistent, and I have placed it at the head of this essay for the purpose of impressing more deeply on the reader the inestimable value of the sense whose nature and office it will be the aim of the following pages to explain.

There is no one probably who has not, at some time or other, propounded to himself the question, which of the senses he would be most loath to dispense with or to lose. The blind would answer, the sight; the deaf, the hearing; because each knows from experience, and is prone to overrate the disadvantages to which the loss of either sense subjects him; but he who, being possessed of all the senses, seriously asks of what avail is each to him, will unquestionably concur in opinion with the blind. No one would in such case think for a moment of the sense of smell, nor would the most pampered palate be suffered to prefer its pretensions in behalf of that of taste; rather would we claim an exception in favor of the sense of touch. But enough; the eye is pre-eminently the jewel of the organs of sense! Let us seek to penetrate into its wonderful conformation. To lay before the novice an intelligible representation of the office, mechanism, and action of the eye, is, it must be confessed, a peculiarly difficult task, more difficult than a like undertaking with regard to the ear, for the reason, in part, that the learning connected with the proposed discussion is necessarily much more comprehensive than that which regards the other organs. In order to perform satisfactorily what we propose, it will be necessary to premise a careful anatomical analysis of at least the most important parts of the complicated apparatus of the globe of the eye, as well as the physical doctrine of the nature of light; the laws of its propagation, refrangibility, reflection, &c.; the relations of the separate parts of the eye to the light should be specially examined, and finally the origin of the sensations of light and the modifications of vision are to be considered; the whole to be couched in terms and in a form intelligible to the laity. We shall lose no time in a further detail of the difficulties or extent of the undertaking, but it is proper to say that no exhaustive discussion of these subjects is here contemplated; we shall be content to have explained to our readers what is most essential, and to have imparted correct views of the most important particulars.

The eye fully corresponds in its arrangements and its purpose to the so-called *camera obscura*, an instrument which we may safely assume to be generally known, especially in view of its customary employment in the service of photography. This instrument is designed to throw a distinct image of an object situated before it upon a surface placed in its background, whether this surface be a pellucid plate of glass on which we see the image as if it were painted on paper, or a sensitive sheet treated with collodion or iodized silver plate, as is practiced by photographers and daguerreotypists. In this last case, the image is fixed, by the chemical influence of the rays of light which produce the various parts of the figure, on the sensitive substance with which the plate is charged; how this takes place, we need not here explain. Whoever has seen the image of a landscape or person on the glass plate of the camera obscura, will remember that it is distinguished by two circumstances from the object it represents: first, it is considerably smaller than the object; and secondly, it is inverted; the houses in the landscape stand on their roofs, the persons on their heads; whatever in the object is to the right, in the image is to the left, and *vice versa.* For the production of this small inverted image, the glass lens placed in the anterior opening is employed, serving for the transmission of the rays proceeding from the object, but at the same time diverting them from their course in such manner that the rays proceeding from each single point of the object, which in front of the lens diverged from one another, converge behind it, so that at a certain point on the plane of the smooth tablet they again unite. This refraction of the rays of light by the glass lens will be rendered more clearly intelligible through an explanation of the formation of the image in the eye. This organ fully corresponds to such a camera obscura; under this aspect, then, let us consider its arrangement and the signification of its separate parts with the aid of the annexed figure, which represents a horizontal section of the

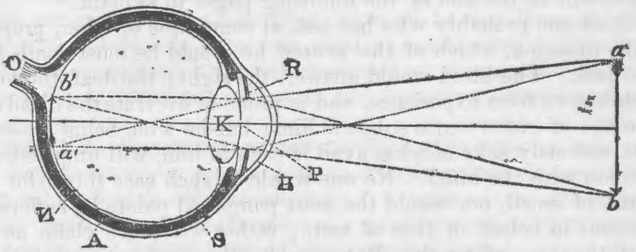


Fig. 1.

eye made directly through its centre. The eyeball is, in general, of globular shape, the optic nerve O being inserted, like a handle, somewhat towards the side of the nose. The outer wall of the globe is, with a partial exception, formed of a hard opaque white tunic S, called the *sclerotic,* (the white of the eye,) which answers to the wooden wall of the camera obscura, and serves as a solid protective covering. On its inner side lies a second tunic or coat, the *choroid,* which is extremely rich in blood-vessels, and is of a dark-brown or chocolate color; it is composed of three layers, the inmost of which consists of small cells, loaded with minute pigmentary granules, and so arranged with regard to one another as to resemble a tesselated pavement. The design of this dark coloring is the same with that of the black coat with which the inner wall of the camera obscura, as well as of the telescope or microscope, is furnished, namely: to prevent the dispersion of the rays of light and consequent disturbance of the perceptions of sight, as will be hereafter explained.

On the front surface of the eyeball we see, as it were inserted into the sclerotic, the transparent cornea H, arched in the manner of a watch-crystal; this forms the foremost part of the wall of the eye, and is distinguished from the rest of the integument by its transparency and greater curvature. Behind the cornea, like the dial-plate behind the watch-crystal, lies a brown or blue or gray-tinted membrane, the *iris* R, with a circular opening in its middle, the *pupil* P. The iris is, in effect, only the forward part of the choroid. The interval between the hinder surface of the cornea and the front surface of the iris is the "anterior chamber" of the eye, filled with a fluid called the *aqueous humor.* Behind the iris, and contiguous to its hinder surface, is the *crystalline lens* K, a lenticular highly transparent body, which turns its front and less curved surface to the cornea, its hinder and more convex surface to the background of the eye. The interval between this lens and the rear of the eye is filled with a perfectly transparent, albuminous fluid, of the consistence of thin jelly, called the *vitreous body.* These cursorily described parts—the cornea, the aqueous humor, the pupil, the crystalline lens, and the vitreous body

—form the different mediums through which are transmitted the rays of light proceeding from external objects to the *retina*. This last is a sensitive membrane which lines the background of the eye, and upon it the rays, after undergoing refraction in their passage through the mediums just mentioned, again unite to form an image of the object from which they issued. The convex cornea and the crystalline lens together correspond to the glass lens in the front opening of the camera obscura. The iris represents a blind or curtain which intercepts all the rays which enter through the lateral parts of the cornea and for certain reasons would disturb the formation of a distinct image, so that only the rays transmitted through the central opening of the cornea—the pupil—are permitted to penetrate to the background of the eye. The pupil, an inconspicuous opening, often scarcely a line in width, is the window through which we observe the world, or, rather, through which the world penetrates to us, since, through this aperture, everything emitting light, from the inconceivably remote to the nearest and most minute, if it falls within the sphere of vision, throws a part, if but a small part of its rays on the delicate organ whose office it is to convert them into perceptions of light and color and of the local relations of visible things. Hapless is he for whom this small window is closed, for whom the transparent lens is clouded; he is indeed despoiled of his most precious heritage.

The *retina*, an extremely delicate, almost transparent membrane, which, as the figure shows, is concentrically outspread upon the inner surface of the choroid nearly as far as the edge of the lens, and which thus lines the cavity filled with the vitreous body, is nothing else than the flattened extension of the end of the optic nerve; but of how wonderfully compounded a structure, of how admirable a distribution and stratified arrangement of the separate and delicate elements. It is impossible, in a rapid sketch, to convey to the novice a complete idea of the mechanism of the retina; we shall give, therefore, but a few outlines. The optic nerve O penetrates, with its vast number of closely compressed filaments, the outer integuments of the eye at the point of entrance, and advances to the cavity of the vitreous body; having reached this, its collective fibres bend around on all sides at right angles, and spread themselves in all directions into a beautiful network of fasciculi or bundles disposed in the plane of the retina, and reaching as far as the anterior margin of that membrane. Everywhere in this course single fibres bend around from this innermost layer of the retina to the external layers, and terminate on the outer surface of the retina in rod-like and flask-shaped or conical organizations, which, as the proper terminal apparatus of the optic nerve, enable, as will hereafter be seen, the waves of light to take effect on its fibres. The external layer of the retina consists of a delicate mosaic of these rods and cones, which are all disposed perpendicularly to the surface; it represents, therefore, a mosaic of the extremities of the optic nerve, of which extremities those situated in the part of the retina directly opposite to the pupil are most closely aggregated with one another; those, on the contrary, situated laterally to the former, stand so much the more widely apart the nearer their position to the anterior margin of the retina, a fact which we shall make use of further on. The retina thus constituted corresponds, as is apparent from the foregoing, to the smooth tablet or photographic collodion plate in the camera obscura.

To examine the texture of the rest of the organizations of the eye would lead us too far: one point, however, must not be passed without notice. Every one has probably observed, or can at any moment satisfy himself, that the pupil, which appears as a round black spot in the middle of the eye, is sometimes wider, sometimes narrower, according as the sight is exerted in obscurity or in light. The iris owes this power of changing the diameter of the pupil to certain muscular fibres, microscopically fine, which, at the excitation of the nerves leading to them, become shorter in their longitudinal diameter, and are capable of being thus contracted to one-sixth of their original length. These fibres, similar in their nature to those of which all muscular flesh consists, pertain, in the iris, to two systems having two different directions. The one system consists of fibres which surround the pupil in circular lines concentric to its margin; the other of fibres perpendicular to the former, which radiate from the margin of the iris to the margin of the pupil. In consequence of this arrangement the operation of the two systems is necessarily as follows: If the marginal or circular fibres contract, the circle formed by them becomes smaller, and consequently the pupil, whose margin is bordered by these circles, narrower; if the radial fibres contract, they draw the margin of the pupil outwards on every side, and therefore widen the pupil. Now it is clear that the narrower the pupil the fewer will be the rays of light which can penetrate within the eye to the retina. The use of a change in the diameter of the pupil is this: if a great deal of light is thrown by very bright objects upon the eye, a part thereof is cut off by the narrowing of the pupil, in order that the sensitive retina may not be unduly excited and dazzled; if we look, on the other hand, at obscure objects, the pupil is widened in order to admit as many rays as possible into the eye, to communicate the strongest excitation to the retina and produce the most vivid sensation possible. It is a circumstance of the greatest interest that this regulation of the intensity of the light arises not through an act of the will, but with invariable certainty and proportionality through a reflex mechanism. It is the light itself which sets the mechanism in motion with a force corresponding to its own strength; the impression produced by the light on the retina and propagated by the optic nerve to the brain calls forth there an activity which, in a reflex manner, is communicated to the muscles of the iris.

Having thus given a general idea of the structure of the eye, we proceed to explain the formation of a small inverted image of the external object on the retina. In the above figure *a b* represents an arrow placed before the eye, which arrow we will imagine to be composed of a number of single luminous points arranged one after the other. Such a luminous point is *a* at one end and *b* at the other end of the arrow. Every such point emits rays of light in all directions, so that the space pervaded by these rays represents a globular sphere, of which the luminous point is the centre. If the eye be within this sphere, a small segment of it, that is to say, a small pencil of rays, penetrates into the eye, namely, those rays which fall within the circle of the pupil, while the rest are intercepted by the opaque parts of the eye. In the figure the diverging, dotted lines proceeding from *a*, and those formed of small strokes proceeding from *b*, show the limits of the pencil of rays (in transverse section) which are capable of entering through the pupil into the eye. Now, we have already mentioned that the crystalline lens and cornea together refract the incident rays from their course in such a manner that they cause the rays which, in front of them, were divergent, to approach one another in their rear, so as to unite again at a point situated behind the lens. How and by what laws this refraction takes place we cannot stop to explain. For simplification let it be supposed that the refraction of the rays is only produced by the lens; the figure will then show the rays proceeding from *a* as united behind the lens at *a'*, and those proceeding from *b* at *b'*; *a'* is therefore the image of the point *a*, as *b'* is of *b*. Between *a'* and *b'* lie the images of all the luminous points of the object between *a* and *b*; *a'*——*b'* then is the image of *a*——*b*. The figure shows further, first, that the image is smaller than the object; and secondly, that it is inverted. In the external arrow, *a* lies to the left, the corresponding image *a'* to the right; *b* lies to the right, *b'* to the left; the relative position of all the intermediate points between *a'* and *b'* will therefore be the same. In this way there is formed on the background of the eye a small inverted image of every external object from which rays pass into that organ, as a similar image is traced on the blank tablet of the camera obscura. The experimental verification of these facts is not difficult. If the eye of some animal be extracted, and both tunics, the sclerotic and choroid, be carefully detached from its posterior hemisphere so that the retina may be seen, the delicate image of any bright object presented to the eye may readily be observed on that membrane. An ingenious method has, moreover, been devised, by means of which we are enabled to see in the living eye the image of a candle-flame, for instance, thrown upon the retina. The instrument by which this is effected has been called "the eye-mirror," *(Augenspiegel)*; to discuss its principle would detain us too long, but its operation is briefly this: the rays of light thrown back from the retina of the observed eye and returning in the same route by which they entered, again unite in the luminous point from which they proceeded, whence they are reflected, by the mirror, on the eye of the observer.

If we observe on the glass tablet of the camera obscura the images of objects situated at different distances from it, we perceive that only the objects which lie at some definite distance throw upon the tablet sharp and distinct images, while the images of objects either nearer or more remote have a confused and faded appearance. If we change the distance between the glass tablet and the lens, or, in other words, shove the lens forwards and backwards, the objects which were at first distinct will become obscure, while the nearer or more remote objects become alternately distinct. In a word, only the images of objects lying at a determinate distance from the lens can be at any one time distinct and well defined; hence the photographer, in order to obtain an exact likeness of a person in front of the camera, must bring the glass tablet or the sensitive collodion plate, introduced in its stead, accurately into the plane on which falls a sharp image of the object. This proceeding is grounded on the following physical facts: If we imagine a luminous point situated at some given distance before a transparent lens, such as is used in the camera obscura, the rays proceeding from that point and striking the lens unite behind the latter at a definite distance; now, if we bring the objective point nearer to the lens, the point of union of its rays will be changed, and, consequently, the position of its image, which will be thrown further back beyond the lens; if, on the contrary, we remove the objective point to a greater distance from the lens, the position of its image will be moved forward and nearer to the lens; if this objective point lies at an immense distance from the lens, like the sun, the position of its image falls upon what is called the focus or burning point of the lens; if the objective point, on the other hand, be placed in the anterior focus of the lens, the position of its image will lie at an infinite distance behind the lens—that is to say, beyond the lens the rays proceed in a parallel direction. Let us suppose now three objective points, *a b c*, placed simultaneously before the lens L at definite distances in a right line one after another; then the corresponding positions of their images *a′ b′ c′* will also lie behind the lens in a right line one after the other, but by no means at a distance from one another corresponding with that of the objective points; the latter, for instance, may be respectively ten, twenty, and thirty feet distant from the lens, the former perhaps only some lines apart. If we suppose that the blank tablet G which intercepts the image occupies the place at which the image of the middle object *b′* is formed this image will be distinctly defined, while the images of the nearer

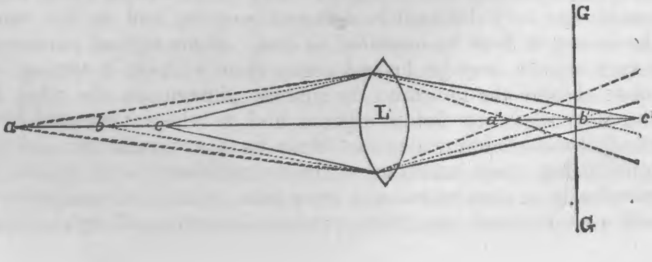


Fig. 2.

and more remote objective points will appear faded and indistinct, for the glass tablet is struck by the rays of the nearer object *c* before their union at the point *c'* by the rays of the more distant object *a*, after their union at the point *a'*, and consequently in both cases by rays diverging from one another. If we move the tablet forward to a' the images of *b* and *c* fall within so-called circles of dispersion, since the rays from both unite at a point behind the tablet. What is here said of a single objective point holds good likewise for larger objects, which are to be considered as composed of a great number of points lying near one another. Wholly conformable to this is the process in the eye: the images of several objects at different distances, simultaneously projected on the retina by the cornea and crystalline lens, fall also behind one another, so that only one of them can be in the plane of the retina, while the others fall either before or behind it, and thus present only the dim images of the dispersion circles. A simple experiment shows this: if we close the left eye and hold before the right at a certain distance a finger so that this finger shall be in a line with a remoter object lying behind it, the window for instance of an opposite house, and then fixedly regard now the finger and now the distant window, it will result that whenever we thus regard the finger the window will appear indistinctly, with dim outlines, and the finger indistinctly when we fix our attention on the window. Still more evident is the following experiment: Pierce with a needle two small openings in a card near one another, hold the openings as closely as possible before the pupil of one eye, while the other eye is closed, and observe through the openings alternately one and the other of two pins set up in front of the eye at some distance from one another; the more distant pin will now appear indistinct and double when the nearer one is attentively regarded, and *vice versa;* the pin to which the sight is directed appearing always well defined and single. The double image of the indistinct pin arises simply from this, that two separate pencils of rays proceed from it through the two apertures of the card into the eye and strike the retina, which is occupied with the pin to which attention is directed, either before their union at a single point or after it in two separate places. The eye-mirror shows in the most direct manner the different distinctness of the images formed on the retina by objects at different distances. From the foregoing experiments we learn, first, that two objects at different distances cannot be seen distinctly at the same time, and secondly, that we have the power to see at will either the nearer or more remote object with distinctness; in other words, that we have the power to adjust our eye to different distances, so that distinct images of objects unequally remote may fall just on the plane of the retina. This power has received the name of the *faculty of ocular accommodation*, and the alteration in question, which we execute for the adjustment of the eye to different distances, is termed *accommodation* or *adaption.* This faculty of accommodation is not unlimited; no eye is capable of being adjusted to all possible distances from the most immediate proximity to infinite remoteness; objects which are too near appear indistinct to all eyes in spite of every effort of accommodation, as do objects removed beyond a certain distance to most eyes; in other words, the images of objects which are too near fall, in the case of every eye, behind the plane of the retina, the images of objects too remote are formed in front of the retina. The limits within which accommodation is possible are very different in different persons; and in the same person may in the course of time be impaired or lost. Short-sighted persons are those to whose eyes objects may be brought very close without becoming indistinct, while remote objects are obscure; far-sighted persons, on the other hand, are those who see clearly very distant objects, and to whom near ones become dim at a relatively considerable distance. Between the normal eye and the object about eight inches may intervene without indistinctness of vision; the near sighted see clearly at four inches and even less. Constant occupation with objects which require close inspection produces shortness of sight; the contrary effect results from habitual occupation with remote objects; hence we find students, engravers, &c, almost always short-sighted; huntsmen, on the other hand, far-sighted; in old age usually every eye loses more or less its capacity of seeing objects which are close at hand. We will now consider in what accommodation consists, and the mechanism by which it is produced.

We have seen in the camera obscura two means of adjustment working to the same end, the forward and backward movement of either the glass lens or the black glass tablet. For the employment of these means in the eye it is necessary either that the screen of the retina which intercepts the image should be movable in the direction of the crystalline lens, or that the lens should be capable of movement forwards and backwards as regards the retina; forwards in the case of near-sight, that the image which would otherwise fall too far behind the retina might be brought forward to the plane of the latter; backwards in the case of far-sight, that the image now formed in front of the retina might be thrown back upon its plane. In the eye the displacement of the retina alone is impossible; it cannot leave the choroid which lies behind it, nor can an apparatus for such a purpose be easily imagined. Mediately, indeed, through a change of form of the whole globe of the eye, the distance of the posterior layer of the retina, which more directly serves for distinct vision, might be changed as regards the lens—that is to say, might be increased by such a lateral compression of the eye that its diameter from before backwards would be augmented at the expense of the transverse diameter, or be diminished by a contraction acting from front to rear. This somewhat rude mechanism of accommodation had, in fact, many advocates in earlier times, who asserted that the compression in question was effected through the small muscles which surround the eye and execute the movements of the ball. This view, nevertheless, must be regarded as wholly untenable, and was indeed contradicted by many insurmountable objections even before the true mechanism of accommodation had been discovered. The second of the above-named means, the movement of the crystalline lens, was accepted by others, though no direct proof of it could be adduced nor an answer given to the question how the fluids before and behind the lens could yield to the displacement—without a possibility, in short, of pointing out any mechanism by which this displacement could be accomplished. At present we know with certainty that in the act of accommodation the lens does not change its place. It follows, therefore, that the adjustment of the eye must take place in a different manner from that of the camera obscura. And in fact a third means remains through which the shifting of the image delineated by a lens may be brought about, a means not indeed applicable to our artificial inflexible glass lenses, but capable of adaptation to the elastic and organized lens of the eye. This means is a *change in the form of the lens.* The physical facts on which this kind of accommodation rests are briefly the following: a lens refracts rays of light more strongly the greater the curvature of its surfaces; if we have therefore a slightly curved and a greatly curved lens formed of the same glass, and an object at a definite and constant distance, the first lens refracts the rays proceeding from this object less than the last lens; the consequence is that the more strongly refracted rays unite sooner than the rays more weakly refracted; hence the point of union, that is, the image of the object, lies in the case of the strongly curved lens nearer behind it than in the case of the weakly curved lens. Thus, if we conceive an object so near the eye that its image with a given distance between the crystalline lens and retina falls behind the plane of the latter, this image will be made to move forward so soon as the surfaces of the lens assume a greater curvature, since the convergence of the rays proceeding from the lens into the vitreous body will be greater. Now this actually takes place in the eye. The eye at rest is adjusted to objects at a certain distance—that is to say, maintains habitually such a curvature that the images of objects at some degree of remoteness fall upon the retina; would we observe objects more closely, the crystalline lens assumes a greater curvature, a curvature so much the stronger in proportion as the object is nearer, the magnitude of the curvature being in every case such as to bring the image which falls too far behind the retina just into the plane of that membrane. The correctness of this statement is ingeniously shown by means which we must be content very briefly to indicate. The surfaces of the transparent structures of the eye, the cornea as well as the lens, do not permit the passage of all the light which falls upon them; a part of it is thrown back or *reflected;* hence if a luminous object is before them, they form a reflex image of it, an image which, from physical laws not necessary to be here dwelt upon, is an exceedingly small one. Further, this little image traced on the outer convex surfaces of the cornea and lens stands upright like the object, and seems to lie behind the reflecting surface, while that of the concave surface behind the lens is inverted and occupies a position before it. The little image of the cornea is easily perceived; the lustrous appearance of the eye consists indeed in the light thus reflected. If we hold a candle flame near our head and observe the eye of another person, we shall readily perceive on its cornea the small sharply defined image of the flame. The images of both surfaces of the lens are seen with more difficulty because they are fainter and are also somewhat eclipsed by the brighter image of the cornea; besides that only a small portion of the reflecting surface is accessible to view through the pupil. Now, it is well known that the reflex image of a curved surface changes in magnitude and position according as the form of the surface is changed, as its curvature is either increased or diminished. On this fact rests the experimental proof of the change of form of the crystalline lens in the act of looking at near and at distant objects. By means of a peculiar telescope adapted to the most exact measurement, the change in position and size of the reflex image traced on the front surface of the lens may be accurately ascertained if the observed eye be alternately fixed on a far and near object at a measured distance without any change of its own position; from these measurements again has been calculated the magnitude of the variation of curvature which the surface in question undergoes in the action of accommodation. That the front surface of the lens becomes more strongly curved when viewing a near object, may be seen even without an instrument, if the eye be observed in profile; the pupil will then be seen to project further beyond the edge of the cornea; it must be understood, however, that the eye which is observed shall not in the least change its place in transferring its contemplation from the remote to the near object.

After having so far considered the eye as an optical instrument, a *camera obscura,* explained the origin of small inverted images of outward objects on its retina, and shown the means by which well defined images of objects at all distances are brought into the plane of this sensitive membrane, let us turn our attention to the proper physiological part of the problem, the explanation of *sight* itself. The existence of the image on the retina is not vision; the soul sits not in the retina, nor receives into itself this image as such without further elaboration; there is no second interior eye through which it observes the small pictures of the retina, as with our visual organ we observe the images of the camera obscura. Between the image and the perception of it there still lies a whole series of processes through which the image operates indirectly on the sensorium, so as to excite sensations of light and color and lead to a recognition of the local relations of external objects. The rays of light, united for the formation of the image, act first on certain apparatus of the retina; this apparatus acts in turn on the fibres of the optic nerve, in which are thus set in motion the mysterious couriers known as "nervous currents;" these currents flow to the brain, and take effect there on other apparatus, which stands in the more immediate service of the soul, and from whose activity the latter derives sensations and ideas. If we would explain this succession of incidents, we must begin with the first member of the series. What, then, is light? What is a ray of light which, through air or glass, proceeds from a distant object to the eye, and is propagated through the cornea, lens, and vitreous body to the retina? It is difficult usually for the laity, without some general previous instruction in physics, to acquire a correct idea of the nature of light, to disengage themselves from the deeply rooted but false notions which from childhood onwards are intimately interwoven with the idea of the luminous element. The sole property of light recognized by the uninformed, as manifested by different colors, is no property at all of light, but, as we have explained in an earlier part of these articles, is a property of the sensation which light awakens, and which has nothing in common with the external exciting cause Without the optic nerve light is still present, but it excites no sensation, imparts no color; this paradox I hope in the following paragraph to unriddle. Light is a movement, the movement, proceeding from its point of excitation, of an ever present and all pervading ether, as it is called, which we must imagine to be composed of countless, infinitely small, inert particles. These particles are thrown into vibrations, which are propagated in the form of waves from particle to particle; such is the nature of light. Luminous bodies, like the sun, constantly set the surrounding particles of the ether in motion, and these propagate their motions from one to another on every side. Everywhere these ethereal undulations impinge upon matter; through a part of this matter, which we term transparent, they pass with more or less obstruction, the motion being transmitted to the particles of the ether contained in such bodies; through another part of matter, untransparent or opaque bodies, they are unable to penetrate, and are either absorbed or reflected as are the waves of sound from solid bodies, being thrown back in the latter case like the waves of the sea from its shores. If the ethereal waves pass from thinner into denser matter, for instance from the air into glass or water, or inversely from a denser into a thinner medium, they pursue their course undisturbed when they strike perpendicularly the surface of the second medium, but, on the other hand, are diverted or refracted from their first direction when they strike that surface at an oblique angle, and in a greater or less degree according to the difference of density of the two mediums. A further discussion of these principles of physics respecting the nature of light is here impossible; of one of the most interesting facts in this connection, the different kinds, namely, of undulatory movement of the luminous ether dependent on the velocity of the waves, we shall have occasion hereafter to speak more at large.

It is these undulations, then, of an inert, matter-penetrating ether, which, proceeding from luminous bodies to our eye, throw the particles of the ether already there into vibration, and are thus finally propagated to the retina. To transform these vibrations into a sensation of light, no apparatus less complicated is requisite than to convert an electrical current into a telegraphic despatch, which has as little in common with the generating current as the sensation of light with the exciting undulations. The sensation to which alone light and color pertain as properties is a wholly specific effect of these waves, which they can only produce by help of the apparatus of the retina and optic nerve, and of certain parts of the brain; if they strike upon wood or metal, the latter become simply expanded, and this expansion bears as much relation to a sensation of light as a telegraphic despatch to an explosion of powder, both of which may be produced by the electric current. Hence we must further inquire, what becomes of the ethereal undulations in the retina; how do they operate upon the elements of that membrane and the fibres of the optic nerve which proceed from it, in order finally to be converted into a sensation of light? Unfortunately very little is known of the chain of processes in question; respecting some links of it we have obscure intimations; respecting others not even so much. In the first place it is certain that the wave is not propagated as such in the optic nerve to the brain, but that in the retina it is converted into that movement of the contents of the nerve molecules which, in our introduction, we designated as a nervous current, and therefore into a movement of a wholly different nature; as different, indeed, as is the electric movement which we excite by friction from the friction itself. How this conversion in the retina is brought about we know not; we can only suppose as a possibility that the waves of the ether produce, in a part of the structure of the retina, a chemical decomposition, and that through this chemical decomposition a chemical irritant is generated which excites the current in the nerve-fibres. For this conjecture nothing further can be alleged than that the undulations of light are well known to produce chemical decompositions, of which the blackening of the silver combination in the sensitive collodion plate of the photographer is an obvious example, and that certain chemical agents are capable of exciting all the nerves. Or it might be conceived that in some way, perhaps, electricity is excited in the retina by means of the undulations, and that this serves as an irritant for the nerves, for which that agent is in general the most potent of stimulants. That electricity excites the optic nerve is a well known fact; if we pass an electric discharge through the eye, a bright flash is instantly perceived. Finally, it might even be supposed that the ether-waves produced an expansion of some structure, and that this mechanically excited the nerves through the consequent pressure. In short, many instrumentalities are conceivable, but none is previously susceptible of proof, nor can one perhaps be considered more probable than another. Only one important discovery has the present age, with all its acknowledged progress, disclosed to us; we know the place of the retina in which the conversion in question is effected; we know the apparatus which is charged with effecting this conversion. It is not the fibres of the optic nerve running on the inner side of the retina, parallel to its surface, which are directly excited by the undulations, though it is upon these that the undulations first strike, before they advance to other elementary portions of the membrane; on the contrary, we know with entire certainty that the waves pass, without resulting effect, not only by these fibres of the optic nerve, but by the layers of the retina lying next behind them, until they have reached the external or hindermost layer bordering immediately on the choroid; their specific effect is therefore first exerted on the so-called columnar layer with its rod-like and cone-shaped organizations. Here the conversion takes place; the rods and cones are the apparatus on which the ether-waves in some manner operate; which apparatus next operates by some unknown agency on the nerve-fibres which bend round so as to advance to this deepest layer. It is impossible to render generally intelligible the demonstration of this statement, interesting as it is in the highest degree; we must confine ourselves to a few cursory indications. That the fibres of the optic nerve in the internal layer of the retina are not directly excited by the vibrations of the ethereal particles, follows necessarily from these among other facts—that, first, those spaces of the membrane where only nerve-fibres are found, namely, at the entrance of the optic nerve, are incapable of vision, to which fact we shall recur in the sequel; and, in the second place, that at that point of the retina with which we see most distinctly, the space just opposite to the pupil, no nerve-fibres exist; from the impossibility, moreover, of reconciling the local perceptions of the organ, hereafter to be spoken of, with the proposition that light directly excites the fibres of the optic nerve. While by these and other considerations it is placed beyond doubt, that light can as little generate a nervous current in the fibres of the optic nerve as in any other nerve-fibre, there has been direct proof adduced that it is precisely the rods and cones before mentioned on which the light operates, that it is through these organizations that the excitation is effected. If, in an obscure apartment, we look fixedly before us, while we move laterally in a circle before the eyes a burning candle, we shall perceive after some time that our field of vision becomes faintly illuminated, and in this bright field a dark arborescently ramified figure makes its appearance. The same phenomenon occurs if we move to and fro, against the bright sky and close before the pupil of the eye, a card pierced with a small aperture. This dark ramified figure is nothing else than the net-work of blood-vessels within the retina of our own eye; hence it has been called the "choroid figure." However incredible it may seem to the laity, that we should thus perceive with the eye objects lying on the background of the eye itself, the fact is not the less true nor the less capable of explanation. It is the *shadows* of the blood-vessels traversing the retina which we see by means of the retina. Why they should seem to be outside of the eye—transferred, that is to say, to external space, like every image of the retina which excites the brain—we shall hereafter see. If we employ the second method for the production of the phenomenon, the explanation will be as follows: the narrow opening of the card presents before the eye a luminous point which sends out rays on every side, and hence a broad pencil of rays through the pupil into the eye. These rays, on account of the close proximity to the eye, are so strongly divergent that both cornea and lens cannot render them convergent by refracting them; they arrive, therefore, still divergent at the retina, and illuminate it to a considerable extent, exciting, wherever they strike, the sensitive apparatus; hence the bright field of vision. Now appear, as stated, the blood-vessels of the retina like dark shadows on a bright ground, a proof that the sensitive layer of the membrane, on which the light takes effect, must lie behind the blood-vessels, since otherwise, these shadows could not be perceived. The vessels themselves run in the layers lying next behind the nervous or internal layer; consequently, the elements of the retina which enable us to perceive the light and also the shadows as an absence of light, must lie still further back. From the degree of displacement which the shadows of the vessels undergo through a shifting of the external source of light, it has been found possible accurately to calculate how far the layer in question must lie behind the vessels, and the distance has in effect been exactly computed which exists between the vessels whose shadows we see and the external or columnar layer of the retina. This is an ingenious and striking proof, which we owe to the same distinguished inquirer who first accurately taught us the structure of the retina, together with the nature and connection of its elements distributed to different layers.

If we now proceed a step further in bridging over the chasm between the luminous wave and the sensation of light, we come to the intermediate member which has been spoken of in our previous discussions respecting each of the other senses—the current, namely, in the excited nerve-fibre, which, having been indirectly set in motion by the wave through the co-operation of the rods and cones in the columnar layer of the retina, flows through the fibrous extremities of the optic nerve in the retina and its remoter channel in the stem of that nerve, to the brain. We have so often inferred from the nature of this movement in the nerve its probable identity in all the nerves; have in so many ways pointed out that this movement in the nerve has nothing at all in common with the external irritant which occasions it—a fact which, of course, holds good with regard to the current of the optic nerve and the undulatory movement of the luminous ether—that we shall spare our readers any unnecessary repetition. We may also abstain from anything more than a simple allusion to the wholly unsolved problem of the further destiny of this current, on its arrival at the brain, in the interior apparatus of the excited nerve. We know only that the current of the optic nerve takes effect on this extreme cerebral apparatus, consisting of the bulbous bodies heretofore described, in a peculiar manner, inducing therein some modification which gives rise in the mind to a sensation of light, just as the corresponding modification in the terminal bulbs of the auditory nerve produces a sensation of sound. Whether any solution of these seemingly impenetrable mysteries is reserved for human ingenuity can only be known to future times.

Before proceeding to treat of this simple sensation of light in its connection with objective ideas, we must consider the different *qualities* of the sensation.

These qualities are the different sensations of color. We distinguish white, red, blue, green, yellow light; we speak of white, red, and other colors of the objects which we see—that is to say, we confer the special property of the sensation which we call color on the external object by which it is generated—with the same incorrectness with which we attribute the properties of light in general to external objects. Now, what this blue or green, this red or yellow color is, can no one say; we can as little describe the qualities of a sensation of light, as the qualities of any impression received through the other senses; and how impossible this is has been sufficiently shown in previous essays. Meanwhile the true image of every sensation of color is indelibly fixed in all memories, so that at any moment we can recall the idea of each several quality, or assign its appropriate color to every object which presents itself; but no one can specify any distinctive sign pertaining to the sensation itself, whether of blue or green; no one knows whether his own perception of blue accords with that of another individual. In common life it is customary to speak of a black color, and even in science it is sometimes doubted whether black should not be called a color; that is to say, a sensation. In my opinion there is no room for such a doubt. Black is nothing but the absence of a perception of light. We call those objects black from which no light proceeds to our eye; we perceive them as gaps in the illuminated field of vision, being conscious, by virtue of the sense of space resident in the retina, of the particles of that membrane which remain in repose and unexcited, between those particles which are struck and irritated by the light. If we exclude all external light from our eyes, the whole field of vision appears black; we are conscious of the repose of all the particles of the retina. But to enter more particularly into the details of this controversy, whether black be a sensation or not, would lead us into too long a digression.

If we turn our consideration to the external causes of the different sensations of color, so indescribable and inexplicable in themselves, we are met by an entirely conclusive and exact physical theory. The different sensations of color stand in relation to their causes in the same category with the sensations of sound of a different pitch; it is the *frequency* of the vibrations of the particles of the luminous ether, the *length of its waves*, by which the quality of the sensation is determined; to each definite color belongs a definite, exactly measured length of wave and frequency of vibration, as to every sound of definite pitch belongs a definite number of vibrations of the body which generates it. Physics has demonstrated that the white light of the sun—that is, the vibrations of the ether proceeding from the sun which taking effect on the retina produce the sensation of a white color—is no uncompounded light, but consists of a great number of the ethereal vibrations differing in the degrees of their frequency; it has further taught us to decompose this compound light into its several constituent parts. If a sunbeam be received through the edge of a three-sided prism of glass, while this edge, for example, is held before a small round opening in an opaque screen upon which the sun shines, there will appear on a surface situated behind the screen not a round white spot corresponding to the opening, but a long bright stripe in which the different colors of the rainbow follow one another in the same order as in the rainbow. One end of this stripe will be occupied by a red portion, followed by an orange, a yellow, a green, a blue, an indigo and a violet colored portion in succession, the latter occupying the other end of the stripe. The origin of this prismatic stripe consists in this: that the waves of different vibratory velocities compounded together in the white light of the sun, on their way through the edge of the prism, are diverted, each in a different degree, from their course, so that those waves in which the ethereal particles vibrate most swiftly are most diverted or *refracted;* those whose particles vibrate most slowly are least refracted; if we imagine, for example, the white solar ray striking horizontally on the prism, all the waves combined therein will, when the edge of the prism is directed upwards, be so refracted as to pass obliquely downwards to the other side, but the slowest with least, the swiftest with most obliquity. It results that the undulations differing in their velocity strike, one after another, on the intercepting screen. The different colors of different portions of the stripe proceeds from this: that the waves reflected from the screen to our eye operate differently on the nervous apparatus of the organ according to the velocity of their vibrations, so that each generates for itself a definite modification of the nervous current, and every such modification a definite quality of sensation; that is, of color. The longest waves with the least vibratory velocity, which reach the eye from the upper end of the stripe, and which hence, in the image on the retina, form, through the inversion of the image, the lower end of the stripe, so operate on the nerve-exciting apparatus that the sensation of a red color is the result; the shortest and swiftest waves from the other end of the stripe (which stripe is called the solar spectrum) produce in the same way the sensation of a violet color; the waves of intermediate length and velocity give rise in succession to the above-named intermediate sensations of color. And the same is the case not merely with the waves directly disengaged from the sun's light, but with those issuing from every visible object. An object appears red when the waves proceeding from it are so constituted that each particle of the ether lying in its course executes only 439 billions of vibrations in a second of time, while each wave is the seventy-thousandth part of a millimetre in length; if the wave be only the sixty-thousandth part of a millimetre long, while, on the other hand, its particles vibrate 697 billion times in a second, the object from which it proceeds will appear of a violet color. The observed object is hence neither red nor violet; it only throws the particles of the ether lying between itself and our eye into vibrations of definite velocity, according to which they excite in our mind, through the medium of the retina and optic nerve, the sensation of red or violet. In like manner we erroneously seek the sound we hear in the vibrating string, while the string simply vibrates; the sound first exists in our sensation, occasioned by the waves of a definite frequency of vibration proceeding from the string. It is, in fact, the general error as regards all perceptions of the senses that we ascribe the quality of the sensation to the exciting external cause; a necessary error, however, through which our sensations become first available for the practical uses of life, as we have repeatedly shown on previous occasions. To return to our theme: Each solar ray, which the prism decomposed into seven undulatory elements distinguishable by their color, comprises other waves besides the colored ones. In the white solar light there are ether waves of yet slower vibration than those which produce the sensation of red, and such as are of yet greater vibratory velocity than those which yield a violet color. In the spectrum, therefore, formed by the prism, there are waves (the longest) which strike beyond the red extremity, as well as others (the most frequent) which strike beyond the violet extremity; but these we do not see, although constituted precisely like the rays of color, except as regards the difference of length. In other words, only those waves of the ether, the number of whose vibrations falls within certain limits, (434–697 billions,) are capable of so affecting the apparatus of the retina as to produce excitation of its nerves, and therefore a sensation; while, on the other hand, as well those whose number of vibrations is too small as those whose number is too great, are not qualified to excite a sensation. Again a complete analogy with the nerves of hearing! If the vibrations of a string are calculated to excite the auditory nerves so as to produce sound, there are equally limits to the number of vibrations which must not be transcended; the string must not oscillate from side to side less than sixteen times, nor more than 64,000 times in a second. If it vibrate less often or more frequently, it sounds no longer. But how, will the reader ask, can it be known that beyond the visible there are present invisible waves of light? How can light be recognized which is not seen? For this there are several excellent means which we will briefly notice. In the first place, all ether waves have the power of giving *warmth*—that is to say, of exciting, when they strike the nerves of touch, the sensation of warmth—of causing matter to expand; in short, of producing all the effects attributable to heat. In other words, heat and light are the same, both consisting in the vibrations of that elastic medium known as the luminous ether. The power of warming is not, however, proper to all vibrations of the ether in an equal degree; the vibrations which are too slow to excite the nerves of vision, and which hence, in the spectrum, fall outside of or beyond the red, possess that power in a high degree; we recognize them by their action upon the apparatus for measuring heat. In the second place, the rays of light have the power of producing, by their action on certain substances, chemical decomposition; for instance, of rendering black certain combinations of silver. This chemical effect pertains in the highest degree to the most rapid vibrations of the ether, and therefore to those invisible vibrations which fall beyond the violet; we recognize their presence by their chemical effect. They are to be recognized, however, in other ways, being capable, for instance, of becoming visible, with a blue color, when, by certain means which we possess, but which we cannot stop to explain, their vibrations are rendered slower. Under favorable circumstances, indeed, when enabled to operate with great intensity on the eye, they become, even without abatement of frequency, directly visible; that is to say, when they take effect very strongly on the optic nerve they are capable of exciting it.

Such, briefly, is the physical theory of the origin of sensations of color. With this are further connected some important physiological considerations. We have proved at large that light and color exist not externally, but are entirely qualities of sensation, conditioned by the nature of the optic nerve and its perceptive apparatus in the brain. A sensation of light, therefore, may be determined not merely by the impact of a wave of the luminous ether on the retina, but by any excitation of the optic nerve, of whatever kind it may be, provided it strike the extremities of the nerve in the retina or its stem and the current be evoked in its fibres. It has been already mentioned in passing that the strongest irritant of all nerves, the electric current, produces, when directed through the eye, intense perceptions of light, or, to speak more exactly, a bright flash at the moment of commencing and ceasing, a radiant yellow and violet colored image during its continuance. Pressure on the nerves of sight or their division, sometimes required by morbid degeneration of the eyeball, also produces a sensation of light. Even mechanical irritation of the retina induces the phenomenon. Every one may recollect from experience that a blow or thrust against the eye is attended with an appearance of sparks, but the fact may be verified at any moment by a more commodious experiment. If the eye be closed and a finger end pressed against the lateral parts of the eyeball, and therefore against the sclerotic coat under which lies the retina, a bright circle will appear in the darkened field of vision; not, however, at the point of pressure, but in the opposite part, as, for instance, towards the nose when the pressure is on the side next the temple, underneath when the pressure is from above. We will inquire further on, why this transposition of the perception should take place. To adduce still another example of these luminous apparitions through mechanical irritation of the optic nerve: When from physical affections, spirituous potations, or morbid excitement a determination of blood to the head takes place, fine luminous specks are apt to glimmer before the eyes, the particles of which float confusedly in the field of vision, and most distinctly when the sight is directed to a bright sky or attention is paid to the phenomenon with the eyes closed. This glimmering arises from over fulness of the blood vessels traversing the retina itself; the loaded vessels and probably the single corpuscles of the blood which permeates them exert a pressure on the elements of the retina lying behind them and thereby produce the excitation of the optic nerve. Once more:

Light and color are specific qualities of sensation, the result of the activity of the nerves, through whatever means that activity is called forth, whether by ethereal undulations, electricity, or mechanical agency. But as the nerves of touch are destined for excitation by pressure, heat, and cold, the auditory nerve for excitation by the waves of sound, and both are arranged with a view to the impression of these agents alone, so the optic nerve is not designed for mechanical or electrical irritation, but is exclusively destined and contrived for excitation by the waves of the luminous ether. Hence that wonderfully perfect optical apparatus before the retina, which conducts the rays of light and disposes them for the formation of exact images; hence the complicated and as yet imperfectly understood apparatus of the retina itself, by which the vibrations of the luminous ether are enabled to transmit their specific excitation to the optic nerve, while for the electric current or for pressure no such enabling apparatus has been provided, but both agents must act immediately on the naked fibres of each nerve in order to set in motion their respective currents.

So much for the origin of a simple sensation of light in general and of its qualities specifically. In relation to the latter, namely, the colors in which observed objects appear to us, we must call attention to some highly interesting but as regards their nature not wholly explained facts, which are known in part to every novice, but often pass without consideration. The same object does not always appear to us of the same color; although the waves of light proceeding from it are the same, the quality of the sensation is in some manner changed in conformity with the color of the objects simultaneously perceived in close proximity with it. If a red wafer on white paper be attentively observed for some time, it will be seen that the paper around the wafer appears no longer of a pure white, but assumes a greenish hue and at length a red tint like the wafer itself. If we look at a white wall through a hole in a green paper it appears at first of a reddish color, but afterwards of a greenish hue like the paper. These experiments may be varied at pleasure. Even black objects, from which no light is emitted into the eye, appear on a colored ground not black, but perceptibly tinted—green, for instance, on a red ground, and green also on a green ground. As already intimated, no certain explanation has been given of these facts.* We know only that the red or green color of the white ground around a green or red wafer is not objective—that is to say, that light-waves of the corresponding velocity do not pass from the paper into our eye, but waves of the same compound (white) light, as when we observe the paper alone. Hence the color of the paper is *subjective*—that is to say, it depends on a peculiar mode of perception by our own mind. The mind conceives white to be green when it appears beside red, to be red when it appears beside green. Red and green form for our mental perception contrasted (complementary) colors; so likewise do blue and yellow, &c. In the white light of the paper, as has been shown, all colors are contained; it may well be, therefore, that when we see red near white we overlook in the compound white the red ingredient and more clearly perceive the complementary color, the green ingredient; single it out in some measure from the mixture and substitute it for the objective white, of which it is a part. This is the commonly received explanation, but it is not full one. It is still more difficult to account for the colors which black objects assume on a colored ground, for here the question no longer relates to the different interpretation of the quality of a definite sensation, but to the perception of color by the retina, when in fact no light falls on it. Black is no color. When we see therefore black objects colored, our perceptive faculty must conceive a sensation of definite quality without any apparent objective cause; how this comes to pass, what, as regards the mind, is the provocative thereto, and why the conceptions of color in this case maintain a constant quality in relation

* See article on Subjective or Accidental Colors.

to the colors of the background is an enigma which it is not at present possible fully to solve.

A second series of interesting facts consists in the so-called *secondary images.* When an impression of light strikes the retina, the sensation thereby generated does not vanish instantaneously with the cessation of the impression from without, but subsists yet a short time longer. A number of familiar phenomena admit of being referred to this fact. When a wheel turns very rapidly we no longer distinguish the single spokes—it appears as a full disk; when we whirl a glowing coal rapidly around we do not see the coal gradually advancing as a bright point; it presents a closed circle of light; that is to say, we see the coal simultaneously at all points of its progress. Whence is this? It results simply from the persistence of the impression. The coal produces at each point of its course a sensation of light which endures for a moment, while the coal itself has already moved forward. If we imagine the coal at the uppermost point of its circuit, the sensation generated at this point still endures, that is, we still see the coal at this uppermost point, while it has already moved a space further, and has generated sensations from the following points, which again continue a moment beyond the time of its presence at any given point. With a certain velocity of rotation, it will follow that the sensation produced at the first point still persists when the coal has performed its whole circuit, again arrived at the uppermost point, and there generated the impression anew; and since the same is the case with each successive point, the image presented to our eye is, of course, that of a circle of light. The same holds good with regard to the revolving wheel. A further interesting example is the following: We have seen that white light is compounded of seven colors—those, namely, of the rainbow; in other words, a ray of light which generates in the eye the sensation of a white color is composed of seven luminous undulations, differing in the vibratory velocity of their ethereal particles, each of which undulations, when it strikes the eye separately, generates the sensation of a peculiar color. Now, if we take a disk, divide its surface from the centre outward into seven compartments, color these successively with the tints of the rainbow, and in the same order, the first red, the second orange, &c., and then cause the disk to revolve very rapidly, it will appear to the eye to be white, or, at least, gray. After what has preceded, the explanation is simple. Through the rapid movement of the disk the several colored rays strike so quickly one after another upon the portion of the retina on which the image is traced, that the impression of the first ray is not yet extinct when the remaining six have already struck upon the same sensitive points, whence the retina is simultaneously excited by the seven rays, and a perception of whiteness is the consequence. From this simple persistence of the original impression we should distinguish the secondary images which sometimes occur in a narrower sense of the word; phenomena which, like the former, are familiar to every one from experience. Who has not looked upon the setting sun, and then perceived the persistent after-images which occupy the field of vision wherever the eyes may be turned, and vanish not even when they are closed; which, on the contrary, then appear vividly and arrayed in different colors, red, blue, &c., while to the open eye, directed, for instance, towards the bright sky, they seem projected as a dark object on the back ground. By attentive observation and some practice many analogous phenomena of great diversity and interest may be observed. We confine our exemplification to an easily repeated experiment. If we place a red wafer (or red paper) on a white ground and observe it attentively for some time, then suddenly turn the sight laterally to the white surface, we shall plainly perceive an after-image of the wafer of a green tint, which follows the eye when directed to different parts of the back ground. If the object thus fixedly observed be blue, the secondary image will be yellow; in a word, it will be always the so-called contrasted or complementary color which presents itself in this image, and always darker than the bright ground on which we observe it. If this ground be colored, the color of the secondary image varies in different ways which we need not specially describe; a blue wafer, for instance, observed on a yellow ground yields, when the eye is turned to a white surface, a yellow image of the wafer on a blue ground.

It has already been said that these images display themselves not only to the open, but to the closed eye, and with the most surprising alternation of colors and brightness, so that, after the observation of some definite object, these repeatedly change, in the secondary image, in a regular series, until, growing weaker and weaker, the image finally disappears. The following is an example of this series of appearances, but it should be premised that by most eyes they are only perfectly recognized after long practice and familiarity; but then so plainly and readily that the secondary images obtrude themselves among the perceptions without particular attention, and often mingle inconveniently and annoyingly with the direct observations of sight. If we observe persistently a bright-colored object—for instance, a bright candle flame through red glass—and then closing the eye, direct our whole attention to the dark field of vision, the secondary images of the red flame will present themselves in the following succession: First, the original appearance subsists, like every impression of the retina, for a very brief moment; next, though so evanescent as to be almost always overlooked, appears a bright green image of the flame on a dark ground; this gives way to a bright red image of the flame, also on a dark ground, to which succeeds a dark green image on a bright ground; then again a bright red on a dark ground; again, a dark green on a bright ground; and this alternation proceeds until the whole phenomenon closes with a faint, dark green image on a bright ground. Thus secondary images of like color and like relations of brightness with the object are interchangeably mingled with those which appear in the complementary colors of the object, and in which what was dark in the object (the ground) is bright; and, inversely, what was bright (the flame) is dark. Secondary images with such inverted relations of brightness are termed negative; those in which these relations are direct are termed positive. Indescribably gorgeous is the play of colors of these images in the closed eye after the contemplation of the sun itself. The positive image appears not white like the sun, but assumes, one after the other, the most various and brilliant colors. First, it appears of a bright blue with orange-colored border, then blue, violet, deep red, with always differently tinted edges. Yet, however beautiful and surprising these images of the sun, we should warn every one against a protracted observation of that luminary; the experiment is extremely hazardous for the eye; the dazzling solar light over excites the retina and paralyzes the vision; the images once seen follow the observer with painful pertinacity. The most striking attestation to the danger of such observations is found in the fact that the inquirer who, with unwearied assiduity, first thoroughly investigated these phenomena, soon grew blind, and has not yet fully recovered his sight. To many of our readers he is not unknown; under the name of "Mises" he has endowed German literature with many highly poetical creations; and whoever, among his attractive writings, has met with the touching poem, "Der Schwarze Vogel," (The Bird of Gloom,) will read it with double interest when he knows that it sprung as a mournful effusion from his heart, when, in consequence of his experiments, the veil of blindness was falling upon his eyes. We renounce any further discussion of the details, as well as an enumeration of the differently modified experiments relating to secondary images, and unfortunately must, at the same time, renounce the purpose of explaining the nature of the phenomenon itself, and the changes which it undergoes under different circumstances. There are theories upon the subject, but none which are entirely satisfactory. So much, indeed, we know, that these images are no dream images, but secondary and necessary effects of the activity of the optic nerve and its terminal apparatus in conformity with natural laws; more than this we shall never be able to establish until we have succeeded in explaining the nature of that nervous activity itself.

We have thus pursued our subject, so far as it relates to the proximate performances of our organ of vision, as far as this is at present practicable; we have seen the undulations of the luminous ether penetrate through the dioptric screen of the eye and the rays of light unite in the surface of the retina into images of outward objects; we have seen, finally, sensations of definite quality arise from those etherial undulations, by means of the apparatus of the retina and the terminal apparatus of the optic nerve. But these simple sensations are by no means visual perceptions, are in themselves no communications to our minds from the outer world of its existence and its action; they are but dead letters, which must first be reduced to living speech. As with all the rest of the senses, so here the naked sensation is but a special accident of our consciousness, which must undergo further elaboration, be interpreted by indirect means, and connected with appropriate ideas, before we can realize its inappreciable value. Only by our learning to refer the sensation of light to the external object which occasions it, thus giving it objectivity, only by our learning to associate with the simple sensations ideas of the local relations of external objects, is sensation transformed into vision. What would it avail us that every object from which a luminous undulation proceeds should cause a sensation which might be of red, or green, or other color, according to the length of the wave, if this sensation remained a purely subjective feeling, disclosed to us nothing respecting the source from which it came; to what purpose the wonderful arrangements which unite the rays of light on the retina into a faithful image of the object whence they proceed, if this image could not act upon the mind in such a way as to make it sensible of the relations of the object in outward space, its form and magnitude, its position and movement? The laity are not generally accustomed to distinguish clearly the sensation from the associated idea, and therefore overlook the wide chasm between a sensation of sight and *seeing*. We will attempt to overbridge this chasm as briefly and intelligibly as possible; and we may the better afford to be brief, since we have already treated at large, in reference to the sense of touch, of the process by which objectivity is conferred on our sensations and of the origin of our local preceptions.

We transfer every impression of the sight to the outer world, even those whose cause exists in the eye itself; we refer none to our organ of vision; the gleam of fire which strikes the eye, as well as the flash occasioned by an electric stream directed through it, seems to us to be outside of the eye. While, as regards the sense of touch, we arrive at a perception of the sensitive surface, and hence of the organ on which the impression takes effect, as a general thing we never attain, in the case of the sense of sight, a consciousness of the perceptive surface of the retina, the organ of this sense; never perceive an image formed on that membrane as cause of the sensation we experience. It was for science first to investigate and verify the existence of such a surface and image; and notwithstanding this, the physiologist himself is as little capable as the novice of transferring his own sensations to the membrane of the retina. And why? There are circuitous means, already discussed in our doctrine of the sense of touch, by which we attain in childhood to the conviction that there is space external to ourselves and outward things as opposed to our own personality; that these outward things are the causes of those conditions of the mind which we call sensations. But while, as regards the sense of touch, we are enabled, through the double sensation which arises from the touch of one part of the surface by another, to distinguish this surface from outward things, no such resource is available as regards the eye, because the means thereto, the power of testing the organ of vision with the organ of vision itself, is wanting; we learn to refer the sensations of sight directly and exclusively to the external objects. This mode of interpretation once established, we are thenceforth its slaves; we can never again free ourselves from its influence nor conceive of a sensation of light as purely subjective, and without the idea of a causative external object to which it is referable.

Yet this reference of the sensations of light to the outward world is still not a complete act of vision; for this there is further required the perception of the local relations of the external things to which we refer the sensations. And here the organ of sight offers a complete analogy with the organ of touch. We must conceive of our retina as a mosaic of sensitive points, just as our outward skin was shown to be; according to the specific coloring which each of these sensitive points imparts to the sensation called forth by it, does the mind recognize the place in outer space at which it is to seek the object of the sensation. In conception the space embraced by the field of view is distributed into just so many compartments as the mosaic of the retina comprises points of sensation; if one of the latter be struck by an impression of light, we refer the sensation to that compartment of the field of vision which corresponds to the point struck. If a series of points of the retina which lie in a right line be struck simultaneously we refer the impressions to those compartments of the ideal mosaic which lie in the corresponding right line. If an image of a sphere, of the moon, for instance, be delineated on the retina, the points of the retina which are struck lie in a spherically bounded surface contiguous to one another, and the same is the case in the field to which our idea transfers the sensations belonging to the several points; hence it is that in the first case we see a right line; in the second, a sphere. If the retina be simultaneously struck by light at two points separated from one another, each of these impressions connects itself with the appropriate idea of locality. We see two distinct points of light and comprehend their severance from one another, since we are conscious of the mosaic particles of the retina remaining unaffected which are situated between the two that are struck; we mentally enumerate these, and from their number form a judgment of the intervening distance. If we observe two objects, one after the other, the image of the first of which covers four, that of the second eight sensitive points on the retina, we estimate the magnitude of each of them and their relative proportion according to the number of sensitive points which are struck, or rather according to the number of the compartments embraced in our ideal field of vision, which in both cases we have filled up with our sensations. In this manner we arrive at the perception of the form and size of observed objects, but always without being conscious of the meditator between the object on the one hand and the sensation and idea on the other, the image, namely, on the retina with its local relations; always without attention to the mental operation by which we have once laboriously learned to clothe the simple sensations with the ideas of form and magnitude.

For a clearer explanation of the local perception, we have called to our aid the hypothesis that the retina represents a mosaic of sensitive points; this hypothesis it is proper that we should now a little more closely examine. Physiology must take it for granted that each of the countless minute filaments of the optic nerve, which, packed together in its stem, are afterwards dispersed over the retina, there terminate in a peculiar apparatus; that the terminal apparatus of all these filaments lie, arranged near one another, in the plane of the retina, like the pieces of a delicate mosaic work; and that each apparatus represents in reality a sensitive point endued with the nature and properties which we have supposed in our explanation of the local perceptions of the organ. If this be the case, everything is easily explained, and the results of the most recent microscopic investigations permit us to conclude with almost entire certainty that the retina is, in fact, such an anatomical mosaic of distinct nerve-ends, that the rod-shaped and conical appendages of the nerves in the hindermost layer of the retina form respectively just such sensitive points as we have supposed. Let it be assumed, then, that every nerve-fibre terminates in a cone, and can, as is really the case, only be excited by the light through this organization; the above explanation may then be interpreted as follows: Each cone elaborates the luminous wave which strikes it into an irritant of the nerve, which sends a current to the brain, and there evokes a perception of light. The current is essentially the same in all the nerve fibres from the similar constitution of the waves, but it is characterized in each fibre by some modification, some *nuance*, however slight it may be, which definitely distinguishes it from the current in all the other fibres; this modification is imparted to it by the appendage in some manner wholly unknown to us. It is this modification of the nervous current, and the sensation thereby generated, which we learn to interpret as a token of the place in the field of vision, to which we must assign the perception; how this is effected has been shown in our remarks on the sense of feeling. The shading of the sensation, which an impression on the central cones situated opposite to the pupil produces, we have learned to connect with the idea that the object to which we refer the sensation lies in the middle of the field of vision, and it is from the same specific shading or modification of the sensation that we determine the relative position of the other points in that field. This brings us to a further interesting question pertaining to this head of our subject. When the novice is informed that all outward objects are portrayed on the retina in an inverted position, it seems to him incomprehensible why we should see the objects upright, as they really are, and not inverted; indeed, up to the latest times there have been physiologists who, in order to clear up this seeming contradiction, have taken great pains to find some anatomical or physical mechanism for once more reversing the image of the retina. This is wholly unnecessary; such a mechanism were only to be thought of, if we perceived the image as such and its inverted position were capable of directly acting upon the sensorium. But that, as we have seen, is not the case. Notwithstanding the inversion of the image, the fact that the object is seen upright admits of the most simple and natural explanation, when we accept the fact that we connect with the excitation of a sensitive point lying to the left in the retina the idea of a cause of the sensation lying to the right in outer space, and have learned in like manner to refer the excitation of a point below to an object situated above. If we consider how ideas of place come to be connected with the sensations of sight, we must see that it cannot be otherwise. An object which we see we can at the same time touch, and from the position of our organ of touch can determine, in the manner formerly pointed out, the position of the object in space. When we know that, in order to reach a point which has evoked a sensation, we must move the touching finger in the direction which we call to the right, we refer the visual sensation also to the right in outer space, unembarrassed by the fact, of which we are wholly unconscious, that the impression of the light has been received on the left and not on the right of the retina. As we formerly saw, it is the appropriate sensations connected with the movements of our limbs, whose surface is sensitive to the touch, from which we form a judgment of the direction of the movement and the position of the objects touched. Now the eyes also are movable; their movements are connected with the feeling of movement like those of our arm, and this feeling we learn to interpret and to use for the formation of ideas. We know, if our sense of vision has once been educated, that an object lies to the right of us, when, in order to perceive it clearly, we must turn our eyes to the right—that is to say, when the movement of the eye creates a muscular feeling, which we have learned to recognize as a movement to the right. These muscular feelings which accompany the movements of our eye are, in yet another relation, highly important auxiliaries to the perceptions of sight, and in a manner wholly analogous to the sense of touch. They instruct us respecting the size, form, and distance of observed objects and the direction of their movements. Not to dwell too long on details, we will illustrate this by an example. Suppose a circular object before us. If it is small, so that we can survey it with the eye unmoved, we at once perceive its form through the sense of relative position exerted by the retina; or, in other words, from the circumstance that the points of the retina occupied by its image lie in a circular line. But if it is so large that we cannot embrace it in one view, we proceed as follows: We so direct our eye that first the image of its uppermost point shall fall upon the central point of the retina; we then move the eye so that the image of the following point shall fall upon the same place, and so on, till one after another the images of all the single and successive points of the ring have made their impression upon the same sensitive portion of the retina. That in this process the impressed place of the retina has remained the same, we learn from the continuance of the local coloring of the sensations; that the object has a circular form, we gather from the muscular feelings which have accompanied the movement of the eye, and which we have learned to refer to a circular movement. Since these muscular feelings inform us also of the magnitude of the movement executed, we form therefrom a judgment respecting the size of the observed circle. If we would estimate the distance of two observed points from one another, we either enumerate, in the manner heretofore explained, the number of the mosaic elements of the retina, which remain unaffected between their images, or we direct our eyes so that first the image of one point shall strike the middle of the retina, and then move the eye so far that the image of the second point shall strike upon the same place, and now compute the interval between the two from the muscular feeling which attended the executed movement and which assists us to form an idea of their direction, size, &c.

We might draw out these remarks to greater length, and initiate our reader, if he had the patience longer to accompany us, still more deeply in the mysteries of the eye; but we limit ourselves to what has been said, because it has seemed to us, under the circumstances, a duty to restrict ourselves to what is of most importance, as well as because many other points would appear to offer too much difficulty to be made either intelligible or interesting to the general reader. There are certain limits beyond which the popular exposition of the natural sciences is impossible, and must partake more or less of the character of charlatanism. To him who has followed us we hope to have sufficiently shown that it needs not a glance towards the inconceivably distant worlds of the nocturnal heavens to be sunk in devout astonishment before the marvels of creation, but that the smallest organ of our bodily frame offers a subject of still more wonder, and, because more accessible to our understanding, of a yet higher degree of interest.

LECTURE ON THE RESULTS

OF

SPECTRUM ANALYSIS APPLIED TO THE HEAVENLY BODIES,

DELIVERED BEFORE THE

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AN important invention or discovery seldom, if ever, remains sterile and alone. It gives birth to other discoveries. The telescope and the microscope have led to remarkable discoveries in astronomy and in minute anatomy and physiology, which would not have been possible without those instruments. The observation that a magnetic body, free to move, arranges itself nearly north and south, has not only contributed immensely to the extension of commerce and of geographical discovery, but also has founded the important science of terrestrial magnetism.

This evening I have to bring before you some additions to our knowledge in the department of astronomy, which have followed from a comparatively recent discovery. The researches of Kirchhoff have placed in the hands of the astronomer a method of analysis which is specially suitable for the examination of the heavenly bodies. So unexpected and important are the results of the application of spectrum analysis to the objects in the heavens that this method of observation may be said to have created a new and distinct branch of astronomical science.

Physical astronomy, the imperishable and ever growing monument to the memory of Newton, may be described as the extension of terrestrial dynamics to the heavens. It seeks to explain the *movements* of the celestial bodies on the supposition of the universality of an attractive force similar to that which exists upon the earth.

The new branch of astronomical science which spectrum analysis may be said to have founded, has for its object to extend the laws of terrestrial physics to the other phenomena of the heavenly bodies, and it rests upon the now established fact that matter of a nature common to that of the earth, and subject to laws similar to those which prevail upon the earth, exists throughout the stellar universe.

The peculiar importance of Kirchhoff's discovery to *astronomy* becomes obvious if we consider the position in which we stand to the heavenly bodies. Gravitation and the laws of our being do not permit us to leave the earth; it is, therefore, by means of *light alone* that we can obtain any knowledge of the grand array of worlds which surround us in cosmical space. The star-lit heavens is the only chart of the universe we have, and in it each twinkling point is the sign of an immensely vast though distant region of activity.

Hitherto the light from the heavenly bodies, even when collected by the largest telescope, has conveyed to us but very meagre information, and in some cases only of their form, their size, and their color. The discovery of Kirchhoff enables us to interpret symbols and indications hidden within the light itself, which furnish trustworthy information of the chemical, and also to some extent of the physical, condition of the excessively remote bodies from which the light has emanated.

We are indebted to Newton for the knowledge that the beautiful tints of the rainbow are the common and necessary ingredients of ordinary light. He found that when white light is made to pass through a prism of glass it is decomposed into the beautiful colors which are seen in the rainbow. These colors when in this way separated from each other form the *spectrum* of the light. Let this white plate represent the transverse section of a beam of white light travelling towards you. Let now a prism be interposed in its path. The beam of white light is not turned aside as a whole, but the colored lights composing it are deflected differently, each in proportion to the rapidity of its vibrations. An obvious consequence will be, that on emerging from the prism the colored lights which formed the white light will separate from each other, and in place of the white light which entered the prism we shall have *its spectrum*—that is, *the colored lights which composed it, in a state of separation from each other.* Wollaston and Fraunhofer discovered that when the light of the sun is decomposed by a prism, the rainbow colors which form its spectrum are not continuous, but are interrupted by a large number of dark lines. These lines of darkness are the symbols which indicate the chemical constitution of the sun. It was not until recently, in the year 1859, that Kirchhoff taught us the true nature of these lines. He himself immediately applied his method of interpretation to the dark lines of the solar spectrum, and was rewarded by the discovery that several of the chemical elements which exist upon the earth are present in the solar atmosphere.

It is my intention to bring before you this evening the results of the extension of this method of analysis to the heavenly bodies other than the sun. These researches have been carried on in my observatory during the last four years. In respect of a large part of these investigations, viz., those of the moon, the planets, and fixed stars, I have had the great pleasure of working conjointly with the very distinguished chemist and philosopher, Dr. Wm. A. Miller. Half a century ago Fraunhofer recognized several of the solar lines in the light of the moon, Venus, and Mars, and also in the spectra of several stars. Recently, Donati, Janssen, Secchi, Rutherford, and the Astronomer Royal have observed lines in the spectra of some stars. Before I describe the results of our observations, I will state, in a few words, the principles of spectrum analysis upon which our interpretation of the phenomena we have observed has been based, and also the method of observing which we have employed.

When light which has emanated from different sources is decomposed by a prism, the spectra which are obtained may differ in several important respects from each other. All the spectra which may present themselves can be conveniently arranged in three general groups. A *spectrum* illustrating each of these three orders is placed upon the diagram:

1. The special character which distinguishes spectra of the *first order* consists in that the continuity of the colored band is unbroken either by dark or bright lines. By means of the electric lamp, Mr. Ladd will throw a spectrum of this order upon the screen. We learn from such a spectrum that the light has been emitted by an opaque body, and almost certainly by matter in the solid or liquid state. A spectrum of this order gives to us no knowledge of the chemical nature of the incandescent body from which light comes. In the present case the light is emitted by the white-hot carbon points of the electric lamp. A spectrum in all respects similar would be formed by the light from incandescent iron, or lime, or magnesia.

2. Spectra of the *second order* are very different. These consist of colored lines of light separated from each other. From such a spectrum we may learn much. It informs us that the luminous matter from which the light has come is in the *state of gas*. It is only when a luminous body is free from the molecular trammels of solidity and liquidity that it can exhibit its own peculiar power of radiating some colored rays alone. Hence substances, *when in a state of gas,*

may be distinguished from each other by their spectra. Each element, and every compound body that can become luminous in the gaseous state without suffering decomposition, is distinguished by a group of lines peculiar to itself. These green lines are produced by silver in a state of gas, and only by silver gas. It is obvious that if the groups of lines characterizing the different terrestrial substances be known, a comparison of these as standard spectra with the spectrum of light from an unknown source will show whether any of these terrestrial substances exist in the source of the light.

3. The *third order* consists of the spectra of incandescent solid or liquid bodies, in which the continuity of the colored light is broken by dark lines. These dark spaces are not produced by the source of the light. They tell us of vapors through which the light has passed on its way, and which have robbed the light, by absorption, of certain definite colors or rates of vibration; such spectra are formed by the light of the sun and stars.

Kirchhoff has shown that if vapors of terrestrial substances come between the eye and an incandescent body, they cause groups of dark lines, and, further, that the *group of dark lines* produced by each vapor is identical in the number of the lines and in their position in the spectrum with the *group of bright lines* of which its light consists when the vapor is luminous.

Mr. Ladd will throw upon the screen the spectrum of incandescent carbon points which contain sodium. Observe in addition to the continuous spectrum of the incandescent carbon a bright yellow band, which indicates the presence of sodium. Now a piece of metallic sodium will be introduced into the lamp. The sodium will be vaporized by the heat, and will fill the lamp with its vapor. This vapor absorbs, quenches the light that it emits when luminous. There will thus be produced a black line exactly in the place where the bright yellow line was seen.

It is evident that Kirchhoff by this discovery has furnished us with the means of interpreting the dark lines of the solar spectrum. For this purpose it is necessary to compare the bright lines in the spectra of the light of terrestrial substances, when *in the state of gas*, with the dark lines in the solar spectrum. When a group of bright lines coincides with a similar group of dark lines, then we know that the terrestrial substance producing the bright lines is present in the atmosphere of the sun; for it is this substance, and this substance alone, which, by its own peculiar power of absorption, can produce that particular group of dark lines. In this way Kirchhoff discovered the presence of several terrestrial elements in the solar atmosphere.

METHODS OF OBSERVATION.

I now pass to the special methods of observation by which, in our investigations, we have applied these principles of spectrum analysis to the light of the heavenly bodies. I may here state that several circumstances unite to make these observations very difficult and very irksome. In our climate, on few only even of those nights in which the stars shine brilliantly to the naked eye, is the air sufficiently steady for these extremely delicate observations. Further, the light of the star is feeble. This difficulty has been met, in some measure, by the employment of a large telescope. The light of a star falling upon the surface of an object-glass of eight inches aperture is gathered up and concentrated at the focus into a minute and brilliant point of light.

Another inconvenience arises from the apparent movement of the stars, caused by the rotation of the earth, which carries the astronomer and his instruments with it. This movement was counteracted by a movement given by clockwork to the telescope in the opposite direction. In practice, however, it is not easy to retain the image of a star for any length of time exactly within the jaws of a slit only the 1-300th of an inch apart. By patient perseverance these difficulties have been overcome, and satisfactory results obtained. We considered that the trustworthiness of our results must rest chiefly upon *direct and simultaneous comparison* of terrestrial spectra with those of celestial objects. For this purpose we contrived the apparatus which is represented in the diagram.

By this outer tube the instrument is adapted to the eye-end of the telescope, and is carried round with it by the clock motion. Within this outer tube a second tube slides, carrying a cylindrical lens. This lens is for the purpose of elongating the round point-like image of the star into a short line of light, which is made to fall exactly within the jaws of a nearly-closed slit. Behind the slit an achromatic lens (and at the distance of its own focal length) causes the pencils to emerge parallel. They then pass into two prisms of dense flint glass. The spectrum which results from the decomposition of the light by the prisms is viewed through a small achromatic telescope. This telescope is provided with a micrometer screw, by which the lines of the spectrum may be measured.

The light of the terrestrial substances which are to be compared with the stellar spectra is admitted into the instrument in the following manner:

Over one-half of the slit is fixed a small prism, which receives the light reflected into it by the movable mirror placed above the tube. The mirror faces a clamp of ebonite, provided with forceps to contain fragments of the metals employed. These metals are rendered luminous in the state of gas by the intense heat of the sparks from a powerful induction coil. The light from the spark reflected into the instrument by means of the mirror and the little prism passes on to the prisms in company with that from the star. In the small telescope the two spectra are viewed in juxtaposition, so that the coincidence and relative positions of the bright lines in the spectrum of the spark with dark lines in the spectrum of the star can be accurately determined.

MOON AND PLANETS.

I now pass to the results of our observations.

I refer in a few words only to the moon and planets. These objects, unlike the stars and nubulæ, are not original sources of light. Since they shine by reflecting the sun's light, their spectra resemble the solar spectrum, and the only indications in their spectra which may become sources of knowledge to us are confined to any modifications which the solar light may have suffered, either in the atmosphere of the planets or by reflection at their surfaces.

Moon.—On the moon the results of our observations have been negative. The spectra of the various parts of the moon's surface, when examined under different conditions of illumination, showed no indication of an atmosphere about the moon. I also watched the spectrum of a star as the dark edge of the moon advanced towards the star and then occulted it. No signs of a lunar atmosphere presented themselves.

Jupiter.—In the spectrum of Jupiter, lines are seen which indicate the existence of an absorptive atmosphere about this planet. In this diagram these lines are presented as they appeared when viewed simultaneously with the spectrum of the sky, which, at the time of observation, reflected the light of the setting sun. One strong band corresponds with some terrestrial atmospheric lines, and probably indicates the presence of vapors similar to those which are about the earth. Another band has no counterpart among the lines of absorption of our atmosphere, and tells us of some gas or vapor which does not exist in the earth's atmosphere.

Saturn.—The spectrum of Saturn is feeble, but lines similar to those which distinguish the spectrum of Jupiter were detected. These lines are less strongly marked in the ansae of the rings, and show that the absorptive power of the atmosphere about the rings is less than that of the atmosphere which surrounds the ball. A distinguished foreigner present at the meeting, Janssen, has quite recently found that several of the atmospheric lines in this part of the spectrum are produced by aqueous vapor. It appears to be very probable that aqueous vapor exists in the atmospheres of Jupiter and Saturn.

Mars.—On one occasion some remarkable groups of lines were seen in the more refrangible part of the spectrum of Mars. These may be connected with the source of the red color which distinguishes this planet.

Venus —Though the spectrum of Venus is brilliant, and the lines of Fraunhofer were well seen, no additional lines affording evidence of an atmosphere about Venus were detected. The absence of lines may be due to the circumstance that the light is probably reflected, not from the planetary surface, but from clouds at some elevation above it. The light which reaches us in this way by reflection from clouds would not have been exposed to the absorbent action of the lower and denser strata of the planet's atmosphere.

THE FIXED STARS.

The fixed stars, though immensely more remote and less conspicuous in brightness than the moon and planets, yet, because they *are original sources of light*, furnish us with fuller indications of their nature.

To each succeeding age the stars have been a beauty and a mystery. Not only children, but the most thoughtful of men, often repeat the sentiment expressed in the well-known lines—

> "Twinkle, twinkle, pretty star;
> How I wonder what you are."

The telescope was appealed to in vain, for in the largest instruments the stars remain diskless—brilliant points merely.

The stars have indeed been represented as suns, each upholding a dependent family of planets. This opinion rested *upon a possible analogy alone.* It was not more than a speculation. We possessed no certain knowledge *from observation* of the true nature of those remote points of light. This long and earnestly-coveted information is at last furnished by spectrum analysis. We are now able to read in the light of each star some indications of its nature. Since I have not a magician's power to convert this theatre into an observatory, and so exhibit to you the spectra of the stars themselves, I have provided photographs of careful drawings. These photographs Mr. Ladd will exhibit upon the screen by means of the electric lamp. I will take first the spectra of two bright stars which we have examined with great care.

The upper one represents the spectrum of Aldebaran, and the other that of Betelgeux, the star marked α in the constellation of Orion.

The positions of all these dark lines, about 80 in each star, were determined by careful and repeated measures. These measured lines form but a small part of the numerous fine lines which may be seen in the spectra of these stars.

Beneath the spectrum of each star are represented the bright lines of the metals which have been compared with it. These terrestrial spectra appeared in the instrument as you now see them upon the screen, in *juxtaposition* with the spectrum of the star. By such an arrangement, it is possible to determine with great accuracy whether or not any of these bright lines actually coincide with any of the dark ones. For example:

This closely double line is characteristic of sodium. You see that it coincides, line for line, with a dark line similarly double in the star. The vapor of sodium is therefore present in the atmosphere of the star, and sodium forms one of the elements of the matter of this brilliant but remote star.

These three lines in the green are produced, so far as we know, by the luminous vapor of magnesium *alone.* These lines agree in position exactly line for line with three dark stellar lines. The conclusion, therefore, appears well founded that another of the constituents of this star is magnesium.

Again, there are two strong lines peculiar to the element *hydrogen;* one line has its place in the red part of the spectrum, the other at the blue limit of the green. Both of these correspond to dark lines of absorption in the spectrum of the star. Hydrogen, therefore, is present in the star.

In a similar way, other elements, among them bismuth, antimony, telurium, and mercury, have been shown to exist in the star.

Now, in reference to all those elements, the evidence does not rest upon the coincidence of *one* line, which would be worth but little, but upon the coincidence of a group of two, three, or four lines, occurring in different parts of the spectrum. Other corresponding lines are probably also present, but the faintness of the star's light limited our comparisons to the stronger lines of each element.

What elements do the numerous other lines in the star represent? Some of them are probably due to the vapors of other *terrestrial* elements, which we have not yet compared with these stars. But may not some of these lines be the signs of primary forms of matter unknown upon the earth? Elements new to us may here show themselves which form large and important series of compounds, and therefore give a special character to the physical conditions of these remote systems. In a similar manner the spectra of terrestrial substances have been compared with several other stars. The results are given in the diagrams. Five or six elements have been detected in Betelgeux. Ten other elements do not appear to have a place in the constitution of this star.

β Pegasi contains sodium, magnesium, and perhaps barium.

Sirius contains sodium, magnesium, iron, and hydrogen.

a Lyræ (Vega) contains sodium, magnesium, iron.

Pollux contains sodium, magnesium, iron.

About sixty other stars have been examined, all of which appear to have some elements in common with the sun and earth, but the selective grouping of the elements in each star is probably peculiar and unique.

A few stars, however, stand out from the rest, and appear to be characterized by a peculiarity of great significance. These stars are represented by Betelgeux and β Pegasi. The general grouping of the lines of absorption in these stars is peculiar, but the remarkable and exceptional feature of their spectra is the absence of the two lines which indicate hydrogen, one line in the red and the other in the green. These lines correspond to Fraunhofer's C and F. The absence of these lines in some stars shows that the lines C and F are not due to the aqueous vapor of the atmosphere.

We hardly venture to suggest that the planets which may surround these suns probably resemble them in not possessing the important element hydrogen. To what forms of life could such planets be adapted? Worlds without water! A power of imagination like that possessed by Dante would be needed to people such planets with living creatures.

It is worthy of consideration that, with these few exceptions, the terrestrial elements which appear most widely diffused through the host of stars are precisely some of those which are essential to life such as it exists upon the earth, namely: hydrogen, sodium, magnesium, and iron. Besides, hydrogen, sodium, and magnesium represent the ocean, which is an essential part of a world constituted like the earth.

We learn from these observations that *in plan of structure* the stars, or at least the brightest of them, resemble the sun. Their light, like that of the sun, emanates from intensely white-hot matter, and passes through an atmosphere of absorbent vapors. With this unity of general plan of structure, there exists a great diversity among the individual stars. Star differs from star in chemical constitution. May we not believe that the individual peculiarities of each star are essentially connected with the special purpose which it subserves, and with the living beings which may inhabit the planetary worlds by which it may possibly be surrounded?

When we had obtained this new information respecting the *true nature* of the stars, our attention was directed to the phenomena which specially distinguish some of the stars.

COLORS OF THE STARS.

When the air is clear, especially in southern climes, the twinkling stars do not all resemble diamonds; here and there may be seen in beauteous contrast *richly-colored gems.*

The color of the light of the stars which are bright to the naked eye is always some tint of *red, orange,* or *yellow* When, however, a telescope is employed, in close companionship with many of these ruddy and orange stars, other fainter stars become visible, the color of which may be *blue,* or *green,* or *purple.*

Now it appeared to us to be probable that the origin of these differences of color among the stars may be indicated by their spectra.

Since we had found that the source of the light of the stars is incandescent solid or liquid matter, it appeared to be very probable that at the time of its emission the light of all the stars is white alike. The colors observed among them must then be caused by some modification suffered by the light after its emission.

Again, it was obvious that if the dark lines of absorption were more numerous or stronger in some part of the spectrum, then those colors would be subdued in power, relatively to the color in which few lines only occur. These latter colors remaining strong, would predominate, and give to the light, originally white, their own tints. These suppositions have been confirmed by observations.

Mr. Ladd will throw upon the screen the spectrum of Sirius, which may be taken as an illustration of the *stars the light of which is white.*

As might be expected, the spectra of these stars are remarkable for their freedom from strong groups of absorption lines. The dark lines, though present in great number, are all, with one exception, very thin and faint, and too feeble to modify the original whiteness of the light. The *one* exception consists of three very strong single lines: one line corresponding to Fraunhofer's C, one to F, and the other near G. Two of these certainly indicate the presence of hydrogen. This peculiarity, which seems invariably connected with colorless stars, is very suggestive, and invites speculation. May it be a sign of a temperature of extreme fierceness?

Let us now examine the spectrum of an *orange star.*

This diagram represents the spectrum of the brighter of the two stars which form the double star *a Herculis.* In the spectrum of this star the green and blue parts of the light, and also the deep red, are subdued with strong groups of lines, while the orange and yellow rays preserve nearly their original intensity, and therefore predominate in the star's light.

The question yet remained to be answered, would the faint telescopic stars, which are *blue, green,* and *purple,* and which are never found alone in the heavens, but always under the protection of a strong *ruddy* or *orange* star, furnish spectra in accordance with this theory?

With some little difficulty, and by means of a special arrangement of the spectrum apparatus, we succeeded in observing the spectra of the components of some double stars. There will now be thrown upon the screen the well-known double star β Cygni. In a large telescope the colors of the two stars are beautifully contrasted, as they now appear upon the screen. The spectra of these stars are now shown. The upper spectrum represents the orange star, the lower one that of its beautiful blue but feeble companion. In the orange star you observe that the dark lines are strongest and most closely grouped in the blue and violet parts of the spectrum, and the orange rays therefore, which are comparatively free from lines, predominate.

In the delicate blue companion, the strongest groups of lines are found in the yellow, orange, and in part of the red. In the arrangement of these groups of lines we have a sufficient cause for the predominance of the other portions of the spectrum which unite in the eye to give the blue purple color of the light of this star.

We have, therefore, shown that the colors of the stars are produced by the vapors existing in their atmosphere. The chemical constitution of a star's atmosphere will depend upon the elements existing in the star and upon its temperature.

VARIABLE STARS.

The brightness of many of the stars is found to be variable. From night to night, from month to month, or from season to season, their light may be observed to be continually changing, at one time increasing, at another time diminishing. The careful study of these variable stars by numerous observers has shown that their continual changes do not take place in an uncertain or irregular manner. The greater part of these remarkable objects wax and wane in accordance with a fixed law of periodic variations which is peculiar to each.

We have been seeking for some time to throw light upon this strange phenomenon by means of observation of their spectra. If in any case the periodic variation of brightness is associated with *physical changes* occurring in the star, we might obtain some information by means of the prism. Again, if the diminution in brightness of a star should be caused by the interposition of a dark body, then, in that case, if the dark body be surrounded with an atmosphere, its presence might possibly be revealed to us by the appearance of additional lines of absorption in the spectrum of the star when at its minimum. One such change in the spectrum of a variable star we believe we have already observed.

Betelgeux is a star of a moderate degree of variability. When this star was at its maximum brilliancy in February last, we missed a group of lines, the exact position of which we had determined with great accuracy by micrometric measurement some two years before.

We have observed the spectra of several variable stars at different phases of their periodic variation, but our results are not yet complete.

It is worthy of notice that the variable stars which have a ruddy or an orange tint possess spectra analogous to that of Betelgeux and β Pegasi.

As an example of this group of variable stars, Mr. Ladd will throw upon the screen the spectrum of μ Cephei when at its maximum.

TEMPORARY STARS.

With the variable stars modern opinion would associate the remarkable phenomena of the so-called *new stars* which occasionally, but at long intervals, have suddenly appeared in the sky. But in no case has a permanently bright star been added to the heavens. The splendor of all these objects was temporary only, though whether they died out or still exist as extremely faint stars is uncertain. In case of the two modern temporary stars, that seen by Mr. Hind in 1845, and the bright star recently observed in Corona, though they have lost their ephemeral glory, they still continue as stars of the tenth and eleventh magnitudes.

The old theories respecting these strange objects must be rejected. We cannot believe with Tycho Brahe that objects so ephemeral are *new creations*, nor with Riccioli that they are stars brilliant on one side only, which have been suddenly turned round by the Deity. The theory that they have suddenly darted towards us with a velocity greater than that of light, from a region of remote invisibility, will not now find supporters.

On the 12th of May last a star of the second magnitude suddenly burst forth in the constellation of the Northern Crown. Thanks to the kindness of the discoverer of this phenomenon, Mr. Birmingham, of Tuam, I was enabled, conjointly with Dr. Miller, to examine the spectrum of this star on the 16th May, when it had not fallen much below the third magnitude.

I ought to state that Mr. Barker, of London, Canada West, who announced an observation of this star on 14th May in the Canadian Free Press, now claims to have seen the star on May 4th, and states that it increased in brilliancy up to May 10th, when it was at its maximum.

The spectrum of this star consists of two distinct spectra. One of these is formed by these four bright lines. The other spectrum is analogous to the spectra of the sun and stars.

These two spectra represent two distinct sources of light. Each spectrum is formed by the decomposition of light which is independent of the light which gives birth to the other spectrum.

The continuous spectrum, crowded with groups of dark lines, shows that there exists a photosphere of incandescent solid or liquid matter. Further, that there is an atmosphere of cooler vapors, which give rise by absorption to the groups of dark lines.

So far the constitution of this object is analogous to that of the sun and stars; but in addition there is the second spectrum, which consists of bright lines. There is, therefore, a second and distinct source of light, and this must be, as the character of the spectrum shows, *luminous gas.* Now the position of the two principal of the bright lines of this spectrum informs us that one of the luminous gases is *hydrogen.* The great brightness of these lines shows that the luminous gas is hotter than the photosphere. These facts, taken in connection with the suddenness of the outburst of light in the star, and its immediate very rapid decline in brightness *from the second magnitude down to the eighth magnitude in twelve days,* suggested to us the startling speculation that *the star* had *become suddenly enrapt in the flames of burning hydrogen.* In consequence, it may be, of some great convulsion, enormous quantities of gas were set free. A large part of this gas consisted of hydrogen, which was burning about the star in combination with some other element. This flaming gas emitted the light represented by the spectrum of bright lines. The increased brightness of the spectrum of the other part of the star's light may show that this fierce gaseous conflagration had heated to a more vivid incandescence the solid matter of the photosphere. As the free hydrogen became exhausted the flames gradually abated, the photosphere became less vivid, and the star waned down to its former brightness.

We must not forget that light, though a swift messenger, requires time to pass from the star to us. The great physical convulsion which is new to us is already an event of the past with respect to the star itself. For years the star has existed under the new conditions which followed this fiery catastrophe.

NEBULÆ.

I pass now to objects of another order.

When the eye is aided by a telescope of even moderate power, a large number of faintly luminous patches and spots come forth from the darkness of the sky, which are in strong contrast with the brilliant but point-like images of the *stars.* A few of these objects may be easily discerned to consist of very faint stars closely aggregated together. Many of these strange objects remain, even in the largest telescopes, unresolved into stars, and resemble feebly shining clouds, or masses of phosphorescent haze. During the last 150 years, the intensely important question has been continually before the mind of astronomers, "What is the true nature of these faint, comet-like masses?"

The interest connected with an answer to this question has much increased since Sir William Herschel suggested that these objects are portions of the primordial material out of which the existing stars have been fashioned; and further, that in these objects we may study some of the stages through which the suns and planets pass in their development from luminous cloud.

The telescope has failed to give any certain information of the nature of the nebulæ. It is true that each successive increase of aperture has resolved more of these objects into bright points, but at the same time other fainter nebulæ have been brought into view, and fantastic wisps and diffused patches of light have been seen, which the mind almost refuses to believe can be due to the united glare of innumerable suns still more remote.

Spectrum analysis, if it could be successfully applied to objects so excessively faint, was obviously a method of investigation specially suitable for determining whether any essential physical distinction separates the nebulæ from the stars.

I selected for the first attempt, in August, 1864, one of the class of small but comparatively bright nebulæ. My surprise was very great on looking into the small telescope of the spectrum apparatus to perceive that there was no appearance of a band of colored light, such as a star would give, but in place of this there were three isolated *bright lines* only.

This observation was sufficient to solve the long agitated inquiry in reference to this object at least, and to show that it was not a *group of stars*, but a *true nebula*.

A spectrum of this character, so far as our knowledge at present extends, can be produced only by light which has emanated from matter in the *state of gas*. The light of this nebula, therefore, was not emitted from incandescent solid or liquid matter, as is the light of the sun and stars, but from *glowing or luminous gas*.

It was of importance to learn, if possible, from the *position* of these bright lines, the chemical nature of the gas or gases of which this nebula consists.

Measures taken by the micrometer of the most brilliant of the bright lines showed that this line occurs in the spectrum very nearly in the position of the brightest of the lines in the spectrum of nitrogen. The experiment was then made of comparing the spectrum of nitrogen directly with the bright lines of the nebula I found that the brightest of the lines of the nebula *coincided* with the strongest of the group of lines which are peculiar to nitrogen. It may be, therefore, that the occurrence of this one line only indicates a form of matter more elementary than nitrogen, and which our analysis has not yet enabled us to detect.

In a similar manner the faintest of the lines was found to coincide with the green line of hydrogen.

The middle line of the three lines which form the spectrum of the nebula does not coincide with any strong line in the spectra of about thirty of the terrestrial elements. It is not far from the line of barium, but it does not coincide with it. Besides these bright lines, there was also an exceedingly faint continuous spectrum. The spectrum had no apparent breadth, and must, therefore, have been formed by a minute point of light. The position of this faint spectrum, which crossed the bright lines about the middle of their length, showed that the bright point producing it was situated about the centre of the nebula. Now this nebula possesses a minute but bright nucleus. We learn from this observation that the matter of the nucleus is almost certainly not in a state of gas, as is the material of the surrounding nebula. It consists of opaque matter, which may exist in the form of an incandescent fog of solid or liquid particles.

The new and unexpected results arrived at by the prismatic examination of this nebula showed the importance of examining as many as possible of these remarkable bodies. Would all the nebulæ give similar spectra? Especially it was of importance to ascertain whether those nebulæ which the telescope had certainly resolved into a close aggregation of bright points would give a spectrum indicating gaseity.

The observation with the prism of these objects is extremely difficult, on account of their great faintness. Besides this, it is only when the sky is very clear and the moon is absent that the prismatic arrangement of their light is even possible. During the last two years I have examined the spectra of more than sixty nebulæ and clusters. These may be divided into two great groups. One group consists of the nebulæ which give a spectrum similar to the one I have already described, or else of one or two only of the three bright lines. Of the six objects examined, about one-third belong to the class of gaseous bodies. The light from the remaining forty nebulæ and clusters becomes spread out by the prism into a spectrum which is *apparently* continuous.

I will exhibit upon the screen a few of the more remarkable of the nebulæ *which are gaseous in their constitution.*

This photograph is from a drawing by Lord Rosse of a small nebula in Aquarius. (I. H. IV.)

We have here a gaseous system which reminds the observer of Saturn and his rings. The ring is seen edgeways.

The three bright lines represent the spectrum into which the light of this object is resolved by the prism.

In this other nebula we find probably an analogous general form of structure. In consequence of the nebula lying in a different position to us, its ring is seen, not edgeways, but open on the flat. The spectrum consists of three bright lines.

The arrangement of the streams of light in the object now on the screen suggests a spiral structure. This nebula is remarkable as the only one in which, in addition to the three bright lines, a fourth line was also seen.

The most remarkable, and possibly the nearest to our system, of the nebulæ presenting a *ring formation*, is the well-known annular nebula in Lyra. The spectrum consists of one bright line only. When the slit of the instrument crosses the nebula, the line consists of two brighter portions, corresponding to the sections of the ring. A much fainter line joins them, which shows that the faint central portion of the nebula has a similar constitution.

A nebula remarkable for its large extent and peculiar form is that known as the *dumb-bell nebula.* The spectrum of this nebula consists of one line only. A prismatic examination of the light from different parts of this object shows that it is throughout of a similar constitution.

The most widely known, perhaps, of all the nebulæ is the remarkable cloud-like object in the sword-handle of Orion.

This object is also gaseous. Its spectrum consists of three bright lines. Lord Rosse informs me that the bluish-green matter of the nebula has not been resolved by his telescope. In some parts, however, he sees a large number of very minute *red* stars, which, though apparently connected with the irresolvable matter of the nebula, are yet doubtless distinct from it. These stars would be too faint to furnish a visible spectrum.

I now pass to some examples of the other great group of nebulæ and clusters.

All the true *clusters,* which are resolved by the telescope into distinct bright points, give a spectrum, which does not consist of separate bright lines, but is *apparently* continuous in its light. There are many *nebulæ* which furnish a similar spectrum.

I take, as an example of these nebulæ, the great nebula in Andromeda, which is visible to the naked eye, and is not seldom mistaken for a comet. The spectrum of this nebula, though apparently continuous, has some suggestive peculiarities. The whole of the red and part of the orange are wanting. Besides this character, the brighter parts of the spectrum have a very unequal and mottled appearance.

It is remarkable that the easily resolved cluster in Hercules has a spectrum precisely similar. The prismatic connection of this cluster with the nebula in Andromeda is confirmed by telescopic observation. Lord Rosse has discovered in this cluster dark streaks or lines similar to those which are seen in the nebula in Andromeda.

In connection with these observations, it was of great interest to ascertain whether the broad classification afforded by the prism of the nebulæ and clusters would correspond with the indications of resolvability furnished by the telescope. Would it be found that all the *unresolved* nebulæ are *gaseous*, and that those which give a *continuous spectrum* are *clusters of stars?*

Lord Oxmantown has examined all the observations of the sixty nebulæ and clusters in my list, which have been made with the great reflecting telescope erected by his father the Earl of Rosse.

The results are given in this diagram:

	Continuous Spectrum.	Gaseous Spectrum.
Clusters	10	0
Resolved, or Resolved?	5	0
Resolvable, or resolvable?	10	6
Blue or green, no resolvability, no resolvability seen	{ 0 6	4 5
	31	15
Not observed by Lord Rosse	10	4
	41	19

Considering the great difficulty of successful telescopic observation of these objects the correspondence between the results of prismatic and telescopic observation may be regarded as close and suggestive.

Half of the nebulæ which give a continuous spectrum have been resolved, and about one-third more are probably resolvable; while of the gaseous nebulæ *none have been certainly resolved*, according to Lord Rosse.

The inquiry now presents itself upon us, what superstructure of interpretation have we a right to raise upon the new facts with which the prism has furnished us?

Is the existence of the gaseous nebulæ an evidence of the reality of that primordial nebulous matter required by the theories of Sir William Herschel and Laplace.

Again, if we do not accept the view that these nebulæ are composed of portions of the original elementary matter out of which suns and planets have been elaborated, what is the cosmical rank and relation which we ought to assign to them?

As aids to a *future* determination of these great questions I will refer in a few words to some other observations.

COMETS.

There are objects in the heavens which occasionally, and under some conditions, resemble closely some of the nebulæ. In some positions in their orbits some of the comets appear as round vaporous masses, and, except by their motion, cannot be distinguished from nebulæ. Does this occasional general resemblance indicate a similarity of nature? If such be the case, if the material of the comets is similar to that of the nebulæ, then the study of the wonderful changes which comets undergo in the neighborhood of the sun may furnish useful information for a more correct interpretation of the structure and condition of the nebulæ. In 1864 Donati found that the spectrum of a comet visible in that year consisted of *bright lines*.

Last January a small telescopic comet was visible. Its appearance in a large telescope is represented on the screen. It was a nearly circular, very faint vaporous mass. Nearly in the centre, a small and rather dim nucleus was seen. When this object was viewed in the spectroscope, two spectra were distinguished. A very faint continuous spectrum of the coma showing that it was visible by reflecting solar light. About the middle of this faint spectrum a bright point was seen. This bright point is the spectrum of the nucleus, and shows that its light is different from that of the coma. This short bright line indicates that the nucleus of this comet was self-luminous, and further, the position of this line of the spectrum suggests that *the material of the comet was similar to the matter of which the gaseous nebulæ consist.*

MEASURES OF THE INTRINSIC BRIGHTNESS OF THE NEBULÆ.

It appeared to me that some information of the nature of the nebulæ might be obtained from observations of another order. If physical changes of the magnitude necessary for the conversion of the gaseous bodies into suns are now in progress in the nebulæ, surely this process of development would be accompanied by marked changes in the intrinsic brightness of their light, and in their size.

Now since the spectroscope shows these bodies to be continuous masses of gas, it is possible to obtain an approximate measure of their *real brightness*. It is known that as long as a distant object remains of sensible size, its brightness remains unaltered. By a new photometric method, I found the *intrinsic* intensity of the light of three of the gaseous nebulæ in terms of a sperm candle burning at the rate of 158 grains per hour:

Nebula No. 4,628, $\frac{1}{1508}$ part of the intensity of the candle.
Annular nebula, Lyra, $\frac{1}{6032}$ part of the intensity of the candle.
Dumb-bell nebula, $\frac{1}{19604}$ part of the intensity of the candle.

These numbers represent not the *apparent* brightness only, but the *true brightness* of these luminous masses, except so far as it may have been diminished by a possible power of extinction existing in cosmical space, and by the absorption of our atmosphere. It is obvious that similar observations, made at considerable intervals of time, may show whether the light of these objects is undergoing increase or diminution, or is subject to a periodic variation.

If the dumb-bell nebula, the feeble light of which is not more than one twenty-thousandth part of that of a candle, be in accordance with popular theory a *sun-germ*, then it is scarcely possible to put in an intelligible form the enormous number of times by which its light must increase before this faint nebula, feebler now in its glimmering than a rushlight, can rival the dazzling splendor of our sun.

MEASURES OF THE NEBULÆ.

Some of the nebulæ are sufficiently defined in outline to admit of accurate measurement. By means of a series of micrometric observations, it may be possible to ascertain whether any considerable alteration in size takes place in nebulæ.

METEORS.

Mr. Alexander Herschel has recently succeeded in subjecting another order of the heavenly bodies to prismatic analysis. He has obtained the spectrum of a bright meteor, and also the spectra of some of the trains which meteors leave behind them. A remarkable result of his observations appears to be that sodium, in the state of luminous vapor, is present in the trains of most meteors.

CONCLUSION.

In conclusion, the new knowledge that has been gained from these observations with the prism may be summed up as follows:

1. All the brighter stars, at least, have a structure analogous to that of the sun.

2. The stars contain material elements common to the sun and earth.

3. The colors of the stars have their origin in the chemical constitution of the atmospheres which surround them.

4. The changes in brightness of some of the variable stars are attended with changes in the lines of absorption of their spectra.

5. The phenomena of the star in Corona appear to show that in this object, at least, great physical changes are in operation.

6. There exist in the heavens *true nebulæ.* These objects consist of luminous gas.

7. The material of comets is very similar to the matter of the gaseous nebulæ, and may be identical with it.

8. The bright points of the star-clusters may not be in all cases stars of the same order as the separate bright stars.

It may be asked what cosmical theory of the origin and relations of the heavenly bodies do these new facts suggest? It would be easy to speculate, but it appears to me that it would not be philosophical to dogmatize at present on a subject of which we know so very little. Our views of the universe are undergoing important changes; let us wait for more facts, with minds unfettered by any dogmatic theory, and therefore free to receive the obvious teaching, whatever it may be, of new observations.

Star differs from star in glory, each nebula and each cluster has its own special features; doubtless in wisdom, and for high and important purposes, the Creator has made them all.

ON THE EXTERNAL APPEARANCE OF THE SUN'S DISK.

TRANSLATED FROM THE GERMAN PERIODICAL "AUS DER NATUR," ETC., LEIPZIG, NO. 11—1864.

In one of the last meetings of the Astronomical Society of England, some interesting remarks on the aspect presented by the exterior envelope of the sun were offered by M. Dawes. He first adverted to the great increase in the number of the observers who apply themselves to the study of that orb, since the number of large telescopes had itself so greatly increased and the dangers incident to the sight been correspondingly diminished. But the appearances in the sun, when carefully studied with these powerful instruments and under favorable atmospherical circumstances, differ so widely from those which had been observed with the imperfect instruments of former times, that it can occasion no surprise if some observers, unapprised of what has been already noticed, should take the objects which fall within the field of their telescope to be new discoveries. That this is sometimes the case, there is every reason to believe. Not this circumstance alone, but still more the new names invented for these supposed discoveries, tend certainly more to impede than to promote the advancement of science. It would seem desirable, therefore, that attention should be directed to those appearances which have been long since observed and described, and that they should be collected and compared with the facts which have been more recently verified by the help of the improved telescope.

To recognize the spotted appearance of the sun, no very great enlargement is necessary. M. Dawes has often observed it with a refractor of only $2\frac{1}{2}$ inches opening, and a magnifying power of 60. If the surface of the sun be observed with an instrument of from six to eight inches opening, it presents the appearance of being chiefly composed of luminous masses which are separated from one another by rows of small black points. The interval between these points is but feebly brought out, and is filled with a matter which shines less brightly than the general surface. Whatever the magnifying power employed, the division between the luminous masses seems never to be complete.

These masses present almost all possible varieties of irregular forms. The rarest of all are those which Nasmyth has compared to willow leaves. They are long, slender, and pointed. This form is only observed in the immediate neighborhood of the larger spots, in their half-shade, and their shadows extending but a small distance from them—a peculiarity mentioned by Dawes as early as 1852, in his description of a new telescope, wherein he also states that the under side of the half-shades appears thickly indented, that lustrous points seem to be directed to the centre of the spots, and that they present, on the whole, the appearance of a band woven of straw whose inner ends have not been brought into close conformity.

Sir John Herschel has stated, in his Introduction to Astronomy, that the part of the sun's disk which is free from spots, shines with no uniform lustre; that the surface of the sun appears dotted with small black points or pores, which, when attentively observed, appear to be undergoing constant change, and that nothing can be more fitly compared with this appearance than the slow subsidence of the flakes of a chemical precipitation in a transparent liquid when looked at from above. M. Dawes has observed and confirms this phenomenon, though he throws some doubt upon the circumstance of a continual change in the state of these pores. He says that he has explored and considered the surface of the sun with the greatest care, reducing the diaphragm of his telescope to small openings of from 20 to 60″ diameter, and availing himself of a greater degree of enlargement than is generally to be commanded; that he has frequently retained the same shining masses, with the included pores, under observation for not less than two hours, but has seldom at any time observed a change, even with a magnifying power of 4,600 fold. He adds that the disturbances which so often take place in the atmosphere are sufficient to produce the belief that a constant change is going on in the objects we observe, and that the eye, when confined to so narrow a field, soon grows weary, and the vision becomes embarrassed.

To this comparative repose, however, there is one fact which forms an exception, for when our observation is directed to the immediate neighborhood of the spots, these are found to enlarge or diminish with great rapidity. It is chiefly under these circumstances that the lustrous masses assume the lengthened shape before spoken of; but these changes are the most vivid when these bright masses extend themselves by a rapid movement across the abyss, and thus form those dazzling bridges which often proceed from the principal spots. The point from which such a movement proceeds is frequently indicated by an accumulation and bending of the greater axis of one of the lengthened masses in the direction of the movement. Here M. Dawes shares the opinion of Sir John Herschel respecting the appearance of chemical precipitation, and refers to it the cause of these phenomena.

Wherever this appearance occurred, M. Dawes narrowed his observation to the edge of the spots, and embracing but a small field, examined with attention the formation of the first part of the bridge. The bright masses presented the appearance of haums of straw, lying nearly all in the same direction and but few oblique to the line of the bridge, the sides of which seemed notched, on account of the unequal length of the parts of which it was formed. It may be remarked that these bridges are always constituted by bright stripes or lines which proceed from the outer envelope and are projected to the half-shadows, without mingling with the under and less brilliant strata. M. Dawes at least has never found it otherwise. The light of these stripes has always been of such intensity that the lines formed by the bridges, however narrow, rendered it impossible to distinguish with the eye the spot from the shadow.

As regards investigations respecting the origin or cause of the sun-spots, M. Dawes advises observers to direct particular attention to the dark nucleus which presents itself in the shadow of the most symmetrical spots. For twelve years has he been made sensible of the inconvenience arising from the application of the same name to different objects and the failure to distinguish the shadow from the nucleus. This admonition has unfortunately been little heeded; in the description of the spots the shadow is constantly confounded with the true nucleus. M. Dawes the more insists upon this point, because his own observations have satisfied him that the presence or absence of the nucleus is competent to determine the origin of the spots, or at least to throw a strong light upon the question, and that the origin of the spots in which the nucleus occurs is altogether different from that where no such phenomenon appears.

To avoid all confusion it is of course indispensable to denominate each several thing by its true name. From the diligent observation of Dawes it results that the shadow under the half-shadow is not the sun's nucleus, and that this nucleus sometimes appears in the centre of the shadow. These two essentially different facts should never be lost sight of.

ON ACCIDENTAL OR SUBJECTIVE COLORS.

PERSISTENCE OF IMAGES.—CONTRAST.—IRRADIATION.—DALTONISM, ETC.

BY THE ABBÉ MOIGNO.

Translated for the Smithsonian Institution from the author's "*Repertoire d'optique moderne ou Analyse complét des travaux modernes relatif aux phénomenes de la lumière.*"

In order to distinguish clearly between purely objective and subjective colors, of the latter of which it is here proposed to treat, the following *definitions* should be premised. *Objective* colors are those in regard to which the eye is simply an organ of vision, an instrument rather passive than active, perceiving merely an external phenomenon without contributing in any manner to its production. The colors of the solar spectrum, those produced by refraction, those by which bodies manifest themselves to us, &c., are objective colors. *Subjective* colors are those which are produced, at least in part, by the eye itself, by a certain reaction of the organ of vision under the influence of a first sensation. If, for example, having closely observed in a strong light the yellow covering of a pamphlet, we open it rapidly, the white pages seem overspread with blue light: now, this blue light is evidently nothing real, is no objective color, but a phenomenon undoubtedly produced in the eye itself—a subjective phenomenon. These latter colors have hence been called *accidental* and *physiological.* The first term appears to us to be preferable, and we shall usually adopt it in the analysis which we are about to present of the researches relating to this class of luminous phenomena.

Reaction of the eye.—In order, first, to give as clear an idea of this remarkable property which the eye possesses of reacting under the impression of a light more or less vivid, and of becoming in some degree active instead of passive, as it previously had been, we shall cite a portion of the letter addressed to Locke by the great Newton, on the images occasioned in the eye by the action of the solar light: "I have made on myself, and at the peril of my sight, the experiment which you mention, and which occurs in Mr. Boyle's book on colors. I practiced it in the following manner: Having looked with the right eye, for a very short time, at the image of the sun reflected from a mirror, I then turned my eye towards an obscure corner of the apartment, and closing the eyelid observed the impression which resulted—that is to say, the colored circle which surrounded the image of the sun, and which, growing weaker by degrees, finally disappeared. I repeated this act a second and third time. At the third repetition, when the luminous image and the colors which surrounded it were rapidly disappearing, and while my attention was concentrated in the expectation that I should completely lose sight of them, I saw them, with no little surprise, again make their appearance and gradually become as vivid and strong as they had been at the moment when I ceased contemplating the sun. * * * I was obliged, in order to recover the use of my eyes, to confine myself in a darkened apartment for three whole days, nor did I perfectly recover my sight but by wholly abstaining from the view of all brilliant objects."

This fact and a thousand others abundantly prove that when the portion of the retina, excited by the presence of a luminous object, is suddenly withdrawn from that action, the impression produced by the object does not immediately cease, but persists during a very sensible interval of time. Let us pause for a few instants to consider with M. Plateau this capital phenomenon, on which depend, in part at least, the subjective colors.

Persistence of images.—Every one knows that when a burning coal is rapidly whirled in the dark, a luminous curve will be seen, as if the coal left on the eye the trace of its passage. This appearance evidently results from the fact that the impression produced by the object at any point of its course still subsists for some time after it has left that point, so that the successive positions of the luminous object must appear simultaneous. To the same cause are to be referred numerous appearances which exhibit themselves whenever we observe an animated object in rapid motion. To this fireworks owe a part of their brilliant effect; rain and hail, for this reason, instead of presenting in their fall the appearance of rounded bodies, offer that of parallel lines; the teeth of a wheel which turns rapidly disappear and seem to be replaced by a semitransparent gauze, through which objects may be distinguished; a vibrating cord presents an analogous effect, &c. We may also directly observe the phenomenon of the persistence of impressions if, after having looked at a luminous object of sufficient lustre, a window for instance, we suddenly close and cover the eyes, when the image of the object will be seen to continue for some time.

Measure of the duration of impressions.—Although this persistence of impressions is constantly reproduced and under a multitude of circumstances, and has received many useful and curious applications, it has been very little studied. D'Arcy first conceived the idea of measuring the duration of the phenomenon by causing a burning coal to revolve in darkness so as to produce the appearance of a luminous ring. And if, in effect, we succeed in giving to the object such a velocity that it shall repass at each point of its course, precisely at the instant when the impression produced by its passage is at the point of vanishing, the duration of the impression will be measured by that of a revolution. D'Arcy concluded from his experiments that the duration of the impression produced on his eye by a burning coal was equal to $8'''$ or $0''.13$. But this method is defective; it gives not the complete duration of the impression, but only the time during which this impression is maintained without sensible loss. To obtain with exactness the total duration of the impression it would be necessary that the velocity of the object should be such that it shall find at each point in its course the preceding impression so much weakened as to be just at the point of extinction; which is almost impossible. M. Plateau has, however, succeeded in surmounting, in part, these difficulties, and has determined the total duration of the impressions produced on his eye by white, yellow, red, or blue objects, observed by daylight. The values for the different colors employed are perceptibly equal: their mean is $0''.34$, instead of $0''$ 13 found by D'Arcy; still this result is but a first approximation.

Length of time necessary for the production of the impression.—Experiments of this kind plainly establish the existence of another fact too little remarked: namely, that for the complete formation of an impression on the retina some lapse of time is requisite. This might have been inferred from the well-known fact that an object which passes very rapidly before the eye is scarcely distinguishable, or even is not perceived at all. The following experiment places this beyond doubt: Let a small white object, a piece of paper for instance, be made to move circularly before a dark back-ground with such a velocity that the apparent ring shall present a tint perfectly uniform and constant; this ring will not appear white, but gray. Now, it is apparent from the uniformity of the tint that, during the duration of a revolution, the impression produced is maintained without sensible loss; if this impression were white, it is evident that the entire ring ought to appear decidedly white, not gray; it follows that by reason of the small space of time during which the object acts on each point of the retina, it produces only a grayish impression—that is to say, an impression imperfectly white. No attempt has been made to measure the time necessary for the complete production of the impression. As regards the time during which a given impression preserves an intensity perceptibly constant, M. Plateau has arrived at the following conclusions:

Laws of the persistence of images.—First. The interval of time during which an impression is maintained without sensible loss is proportionably greater as the impression is less intense. Second. This interval, for the production of a complete impression on the eye of M. Plateau by a white paper, in broad daylight, was found to be less than 0″. 008; for yellow paper, a little longer than white; still longer for red, and longer still for blue. These limits may be much exceeded when the impressions have little intensity. It will be seen that, according to these results, it cannot be said in general, as in speaking of the particular case of a burning coal, that, in order to obtain a continuous sensation, it suffices that the luminous impression be repeated eight or ten times per second. In order to produce on the eye of M. Plateau the continuous sensation of whiteness, such as that of a piece of paper in the full light of day, it was necessary, from the number above given, that the impression of light should be repeated more than 125 times per second. Third. The total duration of the impression appears to be greater in proportion as the impression is more intense. When the object which has produced the impression is very luminous, such as the setting sun, and we cover the eyes, the impression sometimes subsists for several minutes. In some persons, the imagination may exert great influence over phenomena of this sort. Fourth. The total duration of the impression appears to be greater in proportion as the object has been observed for a shorter time; provided, however, that the time be sufficient to develop a complete impression. When, for example, M. Plateau cast his eyes on a small isolated window and speedily directed them to an obscure part of the chamber, he saw the image with its luminous panes and dark sashes distinctly persist, though growing fainter, for about three seconds. If he looked at the window a little longer, the duration of the image was less; and, finally, after a prolonged observation of the window, the image was so fugitive that he could with difficulty perceive a trace of it. Fifth. When the impression proceeds from a very luminous object, such as the setting sun, or even a window, it usually passes through a series of different colors. Thus, when, after having looked at a window for a very short time, M. Plateau covered his eyes with a handkerchief, he saw the image of the luminous panes become successively red, violet, blue, again violet, whitish, then greenish, and green. These phenomena, which seem to vary much with the circumstances of the experiment, have received as yet no satisfactory explanation. Sixth. When, lastly, the impression persisting on the retina proceeds from a brilliant object, it is observed occasionally to disappear and reappear several times in succession before it completely vanishes. This, for example, takes place, according to Darwin, when the setting sun has been observed for some instants, so as not too much to fatigue the sight, and the observer afterwards, with the eyes closed and covered, considers the persistence of the impression produced.

Kaleidophone.—Mr. Wheatstone has made use of the persistence of these impressions to exhibit to the eye the mode in which the transverse vibrations of an elastic rod attached by one of its extremities take place. For this purpose, the rod is terminated by a small and polished metallic ball, or by a small hollow sphere of glass, plated on the interior, and the apparatus is exposed to the sun or the light of a candle. When the rod is put in vibration by means of a bow, or by removing it from its position of equilibrium and then leaving it to itself, the brilliant point which the sun or candle occasions on the little ball produces, by its rapid movement, the appearance of beautiful curves, more or less complicated, according to the form of the rod and the sound which it has been made to produce; thus each system of vibration and each sound has its image. To this ingenious apparatus Mr. Wheatstone has given the name of *kaleidophone.*

Photometer of Mr. Wheatstone.—Another instrument of this distinguished English savant, founded on the same principle, and called a *photometer*, produces still more singular appearances. It consists in causing to revolve eccentrically a disk of cork, having attached to it a greater or less number of little balls similar to those just spoken of, and arranged under the form of regular or irregular polygons. Each of these balls, illuminated by a ray, gives rise in its rotation to a curve, and the different curves, by their involutions, describe the most curious figures.

Different chronoscopes.—Means of appreciating the instantaneousness or duration of various phenomena —Mr. Wheatstone has also devised some very curious processes, founded in like manner on the persistence of impressions, which have enabled him to prove the instantaneousness of certain luminous phenomena, such as the electric spark, or to estimate their duration, however short. One of them consists in observing the phenomenon by reflection in a mirror, to which a very rapid movement is given of such a nature that supposing the luminous object to be persistent, its image seems to describe a great circle. Now, if the phenomenon be instantaneous, the image can be seen only in a single point of this circle, and will not appear changed in form ; if, on the contrary, the phenomenon has an appreciable duration, the image will be elongated in such a manner as to form an arc of a length proportioned to the duration, so that from the magnitude of the perceived arc the duration of the luminous phenomenon may be calculated. An electric spark observed in this manner does not appear at all elongated, whence it must be regarded as instantaneous. Mr. Wheatstone has, in this way, rendered perceptible the intermission of certain flames or luminous jets which, to the naked eye, seem perfectly continuous, such as a current of electric sparks received at a very small distance from a conductor. The light being thus, in an extremely small interval of time, stretched along a greatly extended line, the slightest discontinuity becomes visible, and it is for this reason that the current of sparks presents under such circumstances the aspect of a series of luminous points separated by dark spaces.

Again, Mr. Wheatstone has demonstrated, in the following manner, the instantaneousness of the electric spark : A disk of white pasteboard, on which is drawn in black any figure whatever, is made to turn with great rapidity in its own plane around a fixed axis. From the movement of rotation it becomes impossible any longer to distinguish the figure traced on the circle, which now presents only a series of concentric bands of different tints. This is an evident result of the persistence of impressions. Now let the circle be put in motion in a chamber perfectly darkened, and be then suddenly illuminated by an electric spark, immediately the figure will be very distinctly perceived, as if it were in a state of complete immobility, no matter what velocity may be given to the circle. But this effect could not take place if the spark had a sensible duration, for then the figure would be seen in several successive positions, and thence a confusion would result, so much the greater as the duration of the spark was more prolonged. Mr. Wheatstone has further availed himself of this process to destroy, in certain cases, the false appearances which the persistence of impressions gives to objects in rapid motion. These objects being illuminated by this means, but for an exceedingly short time, they exhibit themselves only in a single position, and the impression which they produce having a sensible duration, we are enabled to judge of their true form. It is thus that Mr. Wheatstone has succeeded in seeing very distinctly with the microscope the movable wheels of the infusory animal called the *rotifer*. The foregoing processes will be found described at length in our treatise on electric telegraphy, as well as the uses to which MM. Arago, Wheatstone, and Dové have applied the same principles, in order to make manifest the duration of flashes of light, the discontinuity of their illumination, the velocity of electricity, the unequal velocity of light in different refracting mediums, &c.

Other processes for manifesting the continuity or discontinuity of various phenomena.—Savart, in a particular case, made use of a process which in like manner enabled him to rectify false appearances resulting from rapid movement, and to determine the real figure of a liquid vein flowing from a circular orifice pierced in a thin plate. His apparatus consists essentially of a large riband, whose surface is divided by transverse bands alternately white and black, and to which a rapid movement may be given in the direction of its length. The liquid being supposed to flow vertically, the movable riband is placed behind the vein, and in a position parallel to it. If a movement sufficiently rapid be then communicated to the apparatus, the eye, placed before the system, can distinguish the several peculiarities presented by the vein; such as the annular dilatations which are propagated along the portions of the vein contiguous to the orifice; a portion which, without the aid of this instrument, appears as smooth as a stem of crystal. The effect produced by this apparatus is analogous to those produced by two wheels turning with rapidity, one behind the other, in planes parallel and approximated, and with unequal velocities of speed or direction.

M. Matteucci has remarked quite recently that, in order to confirm the ingenious observations of Savart, on the constitution of the liquid vein, it is only requisite to illuminate the vein with a large electric spark, or rather a series of sparks. On examining in this manner the fluid vein, obtained as Savart prescribes, it will be easily seen that the part which, to the eye, appears continuous, is in reality composed of drops having exactly the form assigned to them by the illustrious physicist, some being elongated, others flattened, others almost spherical.

M. Plateau has proposed a general process different from that of Mr. Wheatstone, to show under their real form the objects to which their rapid movement gives a deceptive appearance. His apparatus consists of a disk of blackened pasteboard, of about twenty-five centimetres in diameter, movable around an axis like a wheel, and perforated towards its circumference with some twenty narrow slits in the direction of the radii. These openings should be about two millimetres in breadth, and two centimetres in length, and situated at equal distances from one another. To make use of this instrument, it is necessary to give to the disk a movement of rotation sufficiently rapid, then closing one eye, to observe with the other, through the transparent circular band which results from the revolution of the slits, the movable object of which we wish to ascertain the true form; the eye, at the same time, must be held as near as possible to the openings, and the observer be at a certain distance from the object.

Let us suppose, in the first place, that the object has a periodical movement; that is to say, that it passes successively through the same positions; that, for example, it is a chord in vibration, a burning coal moved circularly, &c.; or that such objects as the radii or teeth of a wheel, &c., occupy in succession the same place; then, if the velocity of our disk be such that whenever an opening passes before the eye the same object or similar objects recur in the same position, there will obviously be formed on the retina a succession of identical impressions, which their persistence will connect with one another; hence will result the continuous appearance of an object or series of immovable objects, having the real form, or very nearly such, of the objects we are observing. Thus, for instance, a wheel which turns so rapidly that its radii or teeth appear confounded, would seem perfectly motionless through our disk, supposing the latter to have a suitable velocity. Even if the movement of the object do not present a regular succession, as in the case of one of those luminous meteors whose brilliant trace is probably due to the persistence of the impression, the instrument will still be useful. In that case the object cannot be isolated, as regards the eye, in a succession of identical positions; but the images which it produces on the retina, in the different positions which correspond to the passage of the successive openings, will persist sufficiently long to enable us to judge in general of its real form.

In illustration, we will refer to the effect which is presented, under certain circumstances, by the flame of a candle subjected to this mode of observation. We know that such a flame seems, from time to time, to undergo a rapid oscillatory movement in the direction of its length; its summit appears to rise and sink alternately. If, at the moment of such agitation, this flame be observed by means of the disk in question, the upper part will be seen to be divided into several distinct portions, situated some above the others, and separated by dark intervals. From this it must be inferred that the apparent oscillatory movement of the flame is occasioned by the detachment from its summit of a succession of partial and separate flames which rise rapidly, and are extinguished one after the other.

Thaumatrope.—The phenomenon which we have been considering is the principle of many curious illusions. Every one is acquainted with the little instrument called a *thaumatrope*, which consists of a circular piece of pasteboard made to move rapidly around one of its diameters, and which bears on each of its faces figures drawn in such a way that the combination of the two impressions they produce on the retina forms a third and regular picture.

Anorthoscope.—M. Plateau has made use of the persistence of impressions to produce a new sort of anamorphosis by causing two disks to turn rapidly, one behind the other, with velocities relatively determinate, the disk in the rear bearing a distorted figure brightly illuminated, and that in front being perforated with a narrow opening. He has shown that from the most deformed figures may thus be produced regular images which seem perfectly motionless. The regular figures thus generated result from the successive apparent intersections of the opening with different parts of the deformed figure; intersections which the persistence of impressions causes to appear simultaneous. This application has given rise to a curious apparatus, known by the name of *anorthoscope*.

Experiments of Mr. Faraday.—Mr. Faraday has published a note on certain illusions resulting principally from the regular appearances presented by two wheels of the same size and with the same number of radii, placed one behind the other, and turning rapidly on a common axis in planes very close to one another, with equal velocities and in contrary directions. The eye, placed in front of such a system in the direction of the axis, will see the appearance of a wheel perfectly motionless, and whose radii will be double in number to those of each of the two wheels in motion.

Another experiment by Mr. Faraday consists in causing to turn, in front of a mirror and four to five metres distant from it, a dentated wheel of pasteboard, and observing in the glass the image of this wheel through the seemingly gauzy medium produced by the movement of the teeth and their intervals, the eye being placed very near the wheel. The image then appears completely motionless and in its real form, as if the movement of the wheel had ceased. Mr. Faraday has further shown that, if the part of the wheel comprised between the teeth and the centre be divided into sectors colored and suitably arranged, the colors, which become confused when the revolving wheel is looked at directly, are immediately separated if its image in the mirror be observed in the manner above indicated. It would be easy to deduce from the principles which we have already established the reason of the effects just described. In the same manner may be explained the following experiment, due also to Mr. Faraday: If a new range of openings be cut in the wheel between the teeth and the centre, the number of which differs a little from that of the teeth, and this wheel be then subjected to the experiment of the mirror, on looking through one or another range of openings, the image of that through which we look appears to undergo a slow movement of rotation. Several rows of openings may be thus pierced so as to produce the appearance of movements more or less rapid and of different directions.

Phantascope, phenakistoscope.—The experiments which we have been recounting have led M. Plateau to realize a new species of illusions, which consists in causing figures delineated on a disk to appear animated and alive. The instrument with which this is effected is called a *phantascope*, and produces appearances truly singular. The same idea, since carried into execution in a much more imperfect manner, has given rise to the *phenakistoscope*. The latter apparatus consists essentially of a disk of pasteboard perforated towards the circumference with a series of narrow openings; on the disk are painted small figures which, when it is made to revolve opposite a mirror, seem, when the image is observed through the openings. to become animated and to execute different movements. The effect proceeds from such an arrangement of the instrument that figures which differ progressively from one another in form, position, or place, present themselves in quick succession to the eye, so that, the persistence of the impressions connecting the images with each other, the same figures appear to be passing in a continuous manner from one state to another.

[A very ingenious and interesting modification of this apparatus has lately been invented by William Lincoln, a student of Brown University, Providence, Rhode Island. In this the series of figures are drawn on long slips of paper, one of which at a time is placed for exhibition around the interior circumference of a hollow cylinder which is made to revolve on its axis in a vertical position. The figures are viewed through a series of narrow vertical slits in the cylinder, and as these pass the eye in rapid succession, they give in each instance a glimpse of the opposite figures, and thus produce the effect of apparent motion. With this apparatus, which is called a zootrope, several persons can see the effect at the same time. As the slits near the eye are moving in one direction, while the figures on the opposite concave side of the cylinder are moving with equal velocity in the opposite direction, the number of figures will appear to be double. J. H.]

We return now to subjective colors, still taking for our guide the excellent dissertation of M. Plateau.

SUBJECTIVE COLORS.

First class—The succedaneous colors.—If we regard attentively a colored object placed on a black ground, keeping the eye constantly fixed on the same point, we shall see, at the end of some moments, the color of that object gradually lose something of its lustre; and if the eyes be then suddenly directed to a white surface, we shall presently see an image of the same form with the object appear, but of a color complementary to the latter; the term *complementary* being understood to designate two colors which, added to or blended with one another, produce white. Thus, the prolonged contemplation of a *red* object gives rise afterwards to a *green* image, and, conversely, the contemplation of a *green* object is followed by the appearance of a *red* image; if the object is yellow or blue, the consequent object will be *violet* or *orange*, and *vice versa.* Further, a *white* object produces in this way a blackish image, while a *black* object produces a white image, lighter than the ground on which it is delineated. All these images remain visible for some time; their intensity, as well as duration, being so much greater as the time during which the object has been observed is more considerable. These appearances form a portion of those to which has been given the name of *accidental colors*, a name conferred by Buffon, to whom, as Jurin asserts, we owe the first observations on this sort of phenomena. Time, it will be seen, enters essentially into this first order of subjective colors, since it is their nature to *succeed* to the objective colors which cause them to appear.

Properties of this first order of subjective colors.—The following are the principal properties presented by these singular appearances:

1. The disappearance of the images does not generally take place by a gradual and continuous diminution of intensity; they present, on the contrary, an alternation of disappearances and reappearances; occasionally the primitive impres-

sion is seen to recur once or even several times. The following experiment of M. Plateau shows this effect in a very remarkable manner: One of his eyes being closed and covered with a handkerchief, he adapted to the other a blackened tube about fifty centimetres in length and three in width, and regarded with fixed attention, for a minute at least, a red paper placed in a strong light, and so large that its borders were hidden by the tube; then, without uncovering the closed eye, he suddenly directed the tube to the ceiling of the apartment. A green circular image was now seen to form, but this was very soon replaced by a red image of feeble intensity; after which the green image reappeared, to be succeeded anew by a reddish image, and so on; the red image reappearing as often as four times, though with a progressive diminution of intensity of both the red and green.

2. The same phenomena are still manifested, when, in place of turning the eyes on a white surface, after having regarded the object a sufficient time, they are suddenly closed, and at the same time completely covered with a handkerchief on which the hands are applied; in this absolute darkness the image of the object will be seen perfectly delineated and colored with the complementary tint.

3. The accidental image appears of more or less size, according to the distance of the surface on which it is projected. When that surface is at the same distance from the eye as the object, the image presents the same magnitude; if the surface be more or less remote, the image appears proportionably augmented or diminished.

4. The subjective colors combine with one another in the same manner as the real colors; that is to say, from the accidental yellow and blue is formed green; from the accidental red and blue, violet. Of this we may satisfy ourselves by the following experiment, which we owe to P. Scherffer: Place beside one another, on a black ground, two small squares of paper, colored, the one violet, the other orange, (colors whose accidentals are yellow and blue,) and mark with a black point the middle of each of these squares. Then let the eyes be alternately directed to one and the other of the points, keeping them fixed upon each for about a second, and, after having repeated this operation a considerable number of times, close the eyes, or direct them towards a white surface. We shall now distinguish three squares in juxtaposition, of which the formation is easily conceived, the middle square being evidently formed from the superposition of the accidental colors produced by the two colors employed. In the supposed case the middle square will be *green;* it would be violet if the two colors employed were green and orange, the accidentals of which are the red or blue, &c. One case, however, forms an exception; that, namely, in which the two accidental colors which we combine are complementary the one to the other; then, instead of producing white, like two complementary real colors, they seem, on the contrary, only to give place to an effect of obscurity. Thus, when the two small squares of paper are, the one *green*, the other *red*, colors whose accidentals are *red* and *green*, the square which occupies the middle in the accidental image appears blackish if projected on a white ground, and completely black if the eyes be covered.

5. The accidental subjective colors combine with the real colors in the same manner as these last with one another; that is to say, from accidental red and real blue results violet; from accidental blue and real yellow, green. It is easy to be satisfied of the fact by projecting the accidental image on a surface painted of the color with which we wish to combine it; thus, by projecting on a leaf of *blue* paper the accidental image *red* proceeding from a green object, the resulting appearance is a fine violet. But it is necessary to bear in mind the following remarks: The object being always placed on a black ground, if we project its accidental image on a surface of the same color with itself, or, in other terms, on a surface whose color is complementary with that of the image, the latter

will appear only of a dark gray, as if the sensation were partially destroyed at that point of the surface. This will be the case, for example, if, after having looked for some time at a small piece of red paper placed on a black ground, we cast the eyes on a sheet of this same red paper. On the contrary, if we project the accidental image on a surface whose color is complementary to that of the object—that is to say, which is identical with that of the image—the latter will appear of a finer and purer color than the rest of the surface on which it is detached. This, for example, will take place with a *red* object and a *green* surface, with a yellow object and violet surface.

6. With a view to isolate from all extraneous influence the colored object looked at, we have assumed thus far that it rested on a black ground; if this ground be white, we obtain analogous effects as regards the color of the subjective image, but different as regards the relative brightness of that image, and the surface on which it is projected. Thus when the object rests on a black ground its accidental image, projected on a white surface, appears darker than the latter, and the contrary takes place if the object rests on a white ground.

In order to form a complete idea of the principal properties of these subjective colors, it remains to add the following facts:

7. We have said that when the eyelids are covered, after looking at a bright object, the primitive impression is usually found to persist for a considerable time. In such a case, if the covering be removed from the eyelids, or the eyes be opened and directed to a white surface, it generally occurs that this primitive impression immediately changes into an accidental image, which passes anew to the primitive state if the eyes be again covered; and the impression may thus be made to pass several times in succession from one to the other condition. The effect is very well manifested by the image of a window, and it was in this way that Franklin observed it. This image appears, to the eyes closed and covered, to be formed of luminous panes and dark sashes; but if the light be allowed to reach the eye, the panes become dark and the sashes luminous. If we close the eyes anew the first effect recurs, and so on alternately. Darwin has observed an analogous effect after looking at the setting sun. Mr. Brewster observes that if, after having looked at that orb, a reddish brown spectrum be seen, a certain degree of pressure upon the eye will cause this image to change into a green spectrum, and that if the pressure ceases the image will resume its former color.

Let us remark, in conclusion, that since the primitive impression always survives for some time the cause which has produced it, the accidental or subjective image must be regarded as in reality succeeding the objective image after a measurable space of time. If, in the greatest number of experiments relative to subjective colors, we cannot seize the trace of that first impression which is effaced, it is because it is generally too fugitive, and the time of its existence is further diminished by the circumstance that, in order to give much intensity and duration to the accidental image, the object should be regarded for quite a long time. In effect, while the prolonged contemplation of the object increases the intensity and duration of the second phenomenon, it shortens, in general, as we have seen, the duration of the first. There are cases, however, in which the succession of the two phenomena is perfectly manifest; thus, when M. Plateau contemplated the window during a space of time neither too long nor too short, he distinguished perfectly, first, the image of the window with its luminous panes and dark sashes, then the subjective image with the panes dark and the sashes luminous, &c.

As Scherffer has pointed out in his interesting memoir, we may convert these phenomena of vision into a source of amusement in the following manner: A bust of a man or woman is painted on a black ground in such colors as shall be accidental or subjective as regards the natural colors of such a figure. The skin would, in the supposed case, be of the color of bronze, the eyebrows and hair white, the eyes would have the pupils white on a black ground, &c., which would of course produce a hideous representation; but on looking fixedly and sufficiently long at one point in this figure, and then casting the eyes on a white wall, there will be seen the appearance of a head with its natural colors.

Second class—Simultaneous subjective colors.—All the appearances with which we have been thus far occupied *succeed* the contemplation of the colored objects; but experiment proves that even *during* this contemplation there is manifested another order of phenomena consisting also in the apparition of complementary colors, and which form the second class of subjective or accidental colors. Thus Buffon remarked that whilst we are looking fixedly at a colored object placed on a black ground, there is perceptible outside of this object and along its contour a colored border of a tint similar to that of the subjective image, which is obtained by afterwards casting the eyes on another part of the white ground. Here, then, it is not at a succeeding time, but in the contiguous space that the accidental color arises. Some physicists, however, have assigned this phenomenon to the preceding class of subjective colors, by attributing it to the circumstance that, during the contemplation of the object, the eye cannot remain completely immovable, whence the image vacillates on the retina, and around this image there are parts of the retina which, after having received the impression of the colored light proceeding from the border of the object, receive the impression of the white ground. But without speaking of this circumstance, there are a multitude of other experiments which evince the production of accidental colors during the contemplation of colored objects.

Let us refer in the first place to *colored shadows*. Thus Rumford has shown that when a shadow is produced in a colored light, this shadow is tinged with the complementary color; if, for example, a paper be illuminated with green light, a body illuminated with white light and interposed between the green light and the paper, will cast on the latter a red shadow.

Thus again, according to the observations of Meusnier, when the interior of an apartment is illuminated only by the light of the sun transmitted through a curtain of colored stuff, and this curtain is pierced with a hole some millimetres in diameter, through which the direct light may penetrate, if this pencil of light be received on white paper, the part of the paper illumined by the white light of the sun appears vividly colored with a tint complementary to that of the curtain

Prieur, of the Côte d'Or, has shown that if we place between the window and the eye a piece of colored paper possessing a certain transparence, and apply on this paper a small strip of white pasteboard, the strip will appear tinged with the complementary color; we shall see a *rose* color on *green* paper, *lilac* on *yellow* paper, &c. The effect is more decided for certain positions of the colored paper, positions which are readily found on making the experiment; moreover, if the small strip of pasteboard is itself colored, its own color combines with the complementary of the color of the paper. Thus a blue strip will appear violet on green paper, a yellow strip will appear green on orange colored paper, &c.

Let us cite further an experiment made by Dr. Smith: it consists in applying to one of the eyes a small tube of transparent colored paper, on which a strong light is thrown sideways. Then if a white surface be looked at, both eyes being open, the portion of that surface seen through the colored tube appears tinged with the complementary color.

[I have found the following arrangement to afford a very satisfactory means of exhibiting colored shadows: Place before the object-glass of a magic lantern, or before a convex lens through which a beam of light from the sun is passing, a plate of colored glass so as to cast upon a white screen or an opposite wall, in a dark room, a colored circle of, say, six feet in diameter. If on this circle the more feeble light of a candle is thrown, and an object, such as the hand, is interposed between it and the screen, an intensely colored shadow will be exhibited. By varying the color of the glass-plate or that of the light of the candle, shadows of different colors and of different intensity will be produced.

J. H.]

Effect of contrast.—In the preceding experiments, the space whose color appears modified is supposed to be of small extent relatively to the colored surface which surrounds it, and the color of this last does not appear sensibly altered; yet M. Chevreul has proved in a general manner that this modification of colors is reciprocal; that is to say, that when we see simultaneously two colored objects placed in the neighborhood of one another, their two colors seem to react simultaneously in such a way that *to each of them is added the complementary color of the other.* Thus, when I place beside one another a *red* and a *yellow* object, the first will seem to tend more or less to *violet*, and the second to *green.* This reciprocal modification of the two colors is commonly too feeble to be perceived without having recourse to a particular expedient. The following is the ingenious process of M. Chevreul: two strips of paper or cloth of the two colors which it is proposed to submit to observation, one, for example, *red,* the other *yellow,* are pasted close to one another on a card; the strips should be 12 millimetres in breadth and 5 centimetres in length. Parallel to one of the strips is now pasted, at the distance of a millimetre, a second strip which is identical with the former in dimensions and color, and designed to serve as a term of comparison; the same operation is practiced in relation to the strip tinted with the other color, so that there are finally four colored strips, two of one color and two of the other. The two inner ones are in contact, and it remains to observe the modifications which they mutually produce on one another. With this view the card is to be looked at in a particular direction and for some seconds. The reciprocal effect of the two contiguous colors, which would escape us under ordinary circumstances, becomes in this way almost always sensible, by help of the exterior strips which serve for comparison. Thus, in the example we have chosen of *red* and *yellow* strips, it will be seen that the interior red strip will tend to a *violet* color, and the contiguous yellow strip to a *green.* M. Chevreul reports a great number of other examples which follow the same law; it is necessary to mention here only the following facts:

1. If the two colors employed are complementary one of the other, as red and green, they enliven one another by their juxtaposition, acquiring a remarkable brightness and purity.

2. If we place in juxtaposition with white any color whatever, the former is slightly tinted with the complementary color, and the color employed becomes brighter and deeper. Thus, by the contact of white and red, the first becomes greenish, the second deeper and more brilliant.

3. If any color be placed in contact with black, the latter takes, in a greater or less degree, the complementary tint of the color employed; and this last appears, in general, brighter and clearer.

4. Black and white undergo, likewise, by their juxtaposition, a reciprocal modification; the first becomes brighter, and the second deeper.

5. The mutual modifications of colors are not limited to cases in which the colored objects that modify one another are in contact. M. Chevreul proved, by experiment, that they may be rendered sensible, even when the objects are five centimetres apart, though the intensity of the effect is less as the distance is greater.

6. To these observations, derived from the memoir of M. Chevreul, M Plateau adds another, which is verified every moment, and which may be deduced, in some sort, from 2, 3, and 4 preceding. When two neighboring objects differ in brightness, this difference appears, in general, augmented by their neighborhood; one appears brighter, the other duller, than if they were seen separately or surrounded with objects of a brightness equal to their own. Further, objects

moderately illuminated may disappear completely, as is shown by the results of M Brewster's observations, when their image is portrayed on the retina, in the neighborhood of a part of the organ vividly excited by the presence of a brilliant object. Of this we may easily be convinced by regarding objects placed nearly behind the flame of a candle. These objects appear of a duller hue in proportion as their image more closely approaches that of the flame, and when the distance of the two images is very small they disappear altogether.

In order to complete our statement of the principal facts relating to accidental colors, we add to the experiments already cited the following, which we owe to Dr. Smith: Place a lighted candle very near one of the eyes, (the right eye, for instance,) but out of the line of the optical axis; then hold in front of the eyes a small strip of white paper, and direct your sight toward a remote point, so as to see the small slip double; the image seen by the right eye will appear green, and that seen by the left eye will appear of a reddish tint.

General law of simultaneous subjective colors.—Reflection on the assemblage of experiments which we have been reporting will show that they lead to the following conclusions:

When we look directly or indirectly at a colored space, there is manifested, beyond the outline of that space and to quite a considerable distance, the appearance, more or less decided, of a color complementary to its own, a color which continues to decrease in intensity in proportion as the distance augments. White and black, brightness and obscurity, are assimilated, in this case, to two colors complementary one to the other. If two colored spaces or objects are near one another, the effect is then reciprocal, regard being had to the extent and brightness of each of the two objects.

This development of the complementary color is especially distinct when the space on which we observe it is small and surrounded by a much more considerable extent of the color which is to produce this complementary color, as in the experiments of Rumford, Meusnier, and Prieur. The intensity of the effect is also much augmented if we contrive that the small space shall be feebly illuminated relatively to the colored ground on which it is projected. This serves to account for the colored appearances displayed in a great number of circumstances. Thus, as M. Brewster remarks, when an apartment whose walls are of a bright color is illuminated by the sun, those parts of the furniture on which the light does not directly fall seem to be tinged with a color complementary to that of the walls. So, again, according to the observation of M. Chevreul, when designs are imprinted on colored stuffs or papers, the color of these designs is usually modified by the complementary of that of the ground. Fault has been sometimes found with the manufacturer from this circumstance, because the designs do not appear of the color which had been prescribed. In this case, it can easily be determined whether the difference is owing to an effect of subjective color. It suffices to cut and apply some white paper in such manner that it shall cover the ground and allow only the design to be seen.

It is known that the shadows produced on a white wall at the rising or setting of the sun appear blue or green. It is because the light of that body is then of an orange or red tint, and the shadows are tinged with the complementary color. When an apartment in which candles are lit is still feebly illuminated by the light of departing day, the shadows produced by bodies interposed between the candles and white objects appear blue for the same reason. It is in this way, also, that M. Necker de Saussure accounts for the remarkable phenomena presented by the summit of Mont Blanc at the setting of the sun. M. Arago thought that to the same cause might be attributed the blue or green color under which the sun appeared through the extraordinary mist of 1831, assuming that the mists or clouds near the sun may have been colored red by reflection.

There is no one who has not observed that in a place partially illuminated by the sun, the parts which are in shadow appear much darker than if the sun were so hidden by clouds that the shadow extended over all objects. On the contrary, the effect produced by the colored windows of Gothic churches proceeds, in great part, from the circumstance of their being environed by dark walls.

The facts which relate to this class of colors are susceptible of numerous applications in all the arts where the assortment of tints is involved. M. Chevreul mentions a great number relating to the art of the upholsterer, figured impressions on paper or cloth, &c. For example, he remarks that it is never proper, in assorting furniture, to combine upholstery of a bright red color with mahogany, for the greenish complementary tint which is developed by such accessories causes the reddish color which is prized in mahogany to disappear, and the latter then resembles oak or walnut, &c. M. Chevreul also applies these considerations to the arrangement of flowers in a parterre. The principal rule to be observed in this case is to place blue flowers beside orange-colored; violet flowers beside yellow. As regards red and rose-colored flowers, they never appear with so much advantage as when surrounded by verdure and white flowers. These should also be interposed between groups formed of blue and orange-colored flowers, violet and yellow flowers. We see that this arrangement is founded on the consideration that two complementary colors vivify one another by being associated, and that white heightens the tone of the colors which it is near. Considerations of the same kind are applicable to the female toilet. For example, the head-dress which would seem most advantageous for a fair complexion is a green hat, lined with rose; because the latter color is reflected on the visage, and the green which surrounds it further tends to add to this the effect of its complementary color.

Advantage has also been derived from the effects which result from a difference of brightness in adjacent objects. Painters, in order to judge well of the effect of a painting, observe it through a black tube. The colors in this way acquire considerable vivacity and brightness. The magic effect of the diorama depends in part on the circumstance that the picture alone is well illuminated, while all that surrounds it, as well as the hall containing the spectators, remains in a certain degree of obscurity. When it is desirable to produce a strong effect at the theatre, it is necessary to diminish as much as possible the light in the body of the hall.

This second class of colors may be made to afford curious and amusing effects in the following manner: Let colored papers having a certain transparency be spread on a rectangular frame of wood or pasteboard, in such a way that half the rectangle shall present a red paper and the other half a paper of some other color; then let a flower, of which all the parts have little width, two or three millimetres at most, be cut out of white pasteboard; for this purpose the petals and leaves may be cut in open-work. This flower is now to be placed on the colored papers in such manner that the stem and leaves shall be relieved on the red paper, and the flower on the other colored paper. If the frame be then held between the window and eye in a certain position, the stem and leaves will appear green, and the flower will seem colored with the tint complementary to that of the ground. If we have several such frames, in which one-half is always red and the other half of different colors, the same white flower, applied in succession on different grounds, will each time take a new color. It will be blue on an orange-colored ground, rose on a green ground, while the parts which would naturally be green continue of that color.

Intrinsic nature of the phenomenon of subjective colors of the second order.—It has been seen that the subjective colors of the first class are sometimes followed by one or more reappearances of the primitive impression. The colors which we are now examining sometimes present a phenomenon which is, in relation to space, what the former is in relation to time. We observe, under some circumstances, that at a certain distance from the outline of the colored space, the complementary tint is replaced by a slight shade of the same color with that space. Thus, in the experiment of Prieur de la Côte d'Or, when the small strip of white pasteboard has a certain breadth, ten or twelve millimetres, for example, it occurs, for certain positions of the paper, that the edges only of the latter appear to take the complementary tint, and the interior is lightly tinged with the same color as the paper: for example, on a *red* paper, the small strip will have the edges green and the interior *rose-colored;* that is to say, at a certain distance from the outline of the red space, the green complementary tint is replaced by a shade of red.

It has also been seen that the appearance of the images of the first class succeeds the phenomenon of the persistence of the primitive impressions. Now, we find. in regard to the accidental colors of the second class, a fact which is the analogue of the former. In effect, physicists recognize that the impression produced on the retina extends a little beyond the space directly excited by the light; so that if we observe a red object, for instance, on a black ground, the red image perceived is a little more extended than it would be if the sensation were limited to the space on which the red light directly strikes. From this it results, that if we consider the appearances which are manifested beyond the colored space, beginning at its real outline and proceeding outwardly, the impression of its color extends to a short distance, beyond which the complementary color appears; while this latter, as we have seen, is replaced at a still greater distance by a new development of the first color.

Conclusions.—Recapitulation.—Thus, on comparing the two preceding classes of subjective colors, we see, that in the first, as soon as the retina ceases to be directly excited by the presence of the colored object, there occur: 1st. A persistence, generally very short, of the primitive impression; 2d. The apparition of the accidental image; 3d. Usually successive disappearances and reappearances, more or less numerous, of that accidental image; and, in certain cases, alternate apparitions of the primitive impression and of the accidental image.

In the second class, beyond the outline which the colored space or object would directly present, we find: 1st. A prolongation, to a certain distance, of the real impression; 2d. Beyond that prolongation and to a distance usually considerable, a development of the accidental color; 3d. Under certain circumstances, beyond the space colored by the accidental color, a space which in this case is of small extent, we observe a new development of the real color of the object.

It will hence be seen that the phenomena of the second class are really, in relation to *space*, what the phenomena of the first class are in relation to *time*. There are other appearances which depend alike on these two sorts of accidental colors.

New class of phenomena.—We will cite only a few examples: 1st. When instead of placing the object which we are observing on a white or black ground, we place it on a colored ground of sufficient extent, the color of the image afterwards perceived on casting the eyes on a white surface is not simply the complementary of the color of the object; it is found to be combined with the color of the ground on which the object is placed. Thus a small piece of *orange-colored* paper placed on a sheet of *yellow* paper, will produce a *green* accidental image on a *violet* ground; that is to say, the image will be composed of *blue*, which is the complementary of the *orange*, and of *yellow*, which is the color of the paper itself on which the object was placed. Thus, too, a small piece of *violet* paper placed on a sheet of *red* paper will produce an *orange-colored* image on a *green* ground. These effects were remarked by Darwin, and depend on the two classes of subjective colors which we have been considering. In effect, while we are observing an *orange-colored* object placed on a *yellow* ground, a light tint of *violet* produced by the neighborhood of the *yellow* will become mixed with that orange color; and when afterwards the eyes are directed to a *white* surface, the phenomena of succession manifesting themselves, the *orange* changes into *blue*, and the *violet* which was mixed with it also changes into its complementary—that is to say, into *yellow;* whence the image must appear composed of *yellow* and *blue*, or *green*.

2d. When the object we look at is *black* and placed on a colored ground, its accidental image always appears tinged with the color of that ground. Thus, a *black* object placed on a *red* ground will give a *pale red* accidental image on a *green* ground, &c. The effect is more decided with the eyes closed and covered. Here again, while we were looking at the black object, a light complementary tint of the color of the ground manifested itself, a tint which, when we cease to look at the object, changes into its complementary—that is to say, into the color of the ground itself.

3d. If, on the contrary, we look fixedly and sufficiently long at a small colored object on a black ground of adequate extent, and afterwards cover our eyes, the accidental image appears surrounded to a certain distance by an aureola lightly tinged with the color of the object itself. Thus, a *red* object produces a *green* image surrounded by a *reddish* aureola; an effect which evidently proceeds from the circumstance that, during the contemplation of the colored object, the space which surrounds it takes, to a certain distance, a light tint complementary of the color of the object, and this tint afterwards, to the closed and covered eyes, changes into its complementary—that is to say, into the color of the object itself.

Under the name of subjective colors are further comprised certain colored appearances, produced under circumstances entirely different: for example, when the eye is subjected to pressure in the dark, or when a blow is received on the eye, or in certain indispositions of the stomach, &c. Of these we shall say a few words hereafter.

DIFFERENT HYPOTHESES ADVANCED FOR THE EXPLANATION OF THESE SINGULAR PHENOMENA.

Colors of the first class.—Theory of Scherffer.—The theory of which we owe the first idea to Scherffer seems now adopted, with a slight simplification, by the generality of physicists. It supposes that the sustained action of the rays of a certain color on a part of the retina momentarily diminishes its sensibility for rays of that color, so that if the eyes be then directed to a white surface the portion of the retina whose sensibility is thus modified can, for some time, only receive a complete impression from the constituent part of this white which is complementary to the color that has fatigued the organ. Thus, on this hypothesis, when we contemplate fixedly and sufficiently long a *red* object, the part of the retina on which the image of the object is depicted becomes less sensible to the *red* light; and if the eyes be then directed to a white surface, since this white may be considered as composed of *red* and *green*, an image will be perceived in which the green predominates. This simple and ingenious theory completely explains most of the phenomena relative to subjective colors of the first class; the diminution of brightness of the colored object in proportion as the contemplation is prolonged; the increase of intensity of the image, the longer the object has been observed; the combination of the accidental colors with one another, &c. Still there are important facts for the explanation of which this theory is evidently insufficient. Such, for example, are: 1st, the production of subjective colors in complete darkness; how should the simple insensibility of the organ for a color produce, in the absence of all light, the sensation of the complementary color? 2d. The combination of the accidental with the real colors; if a *red* accidental image is occasioned by the retina having become less sensible to *green*, how comes it that *violet* is seen when this image is projected on a *blue* surface? After having regarded fixedly a small circle of paper placed in the *red* ray, and then turned the eyes upon a larger circle illuminated by the *yellow* ray, M. Plateau distinctly saw upon the latter an image of a fine *yellowish green*, while, if the retina had simply become insensible to red, it could evidently have perceived only a *blackish* image. 3d. The apparently irregular process of decrease presented by the accidental images. The following fact would suffice, it might seem, to overthrow it completely: When, after having pressed his eyes in a particular manner, this pressure was made suddenly to cease, M. Plateau, his eyes being closed and covered, saw a large *red* spot bordered with *green;* but as soon as he opened his eyes or cast them on a white surface, these two colors changed into their complementaries; the spot became *green* and the surrounding border *red.* But here no exterior light had been present to fatigue the organ and to render it less sensible to certain colors; we have here spontaneous sensations of *red* and *green,* which change into sensations of *green* and *red.*

Colors of the second class.—Theory of contrast.—The most generally accepted theory attributes these colors to contrast; that is to say, to a moral cause which renders more intense our perception of whatever is *unlike* in the colors collated with one another, while it weakens the perception of what they have *in common;* thus a small *white* object being projected upon a colored ground—a *red* one, for instance—the effect of the contrast is to diminish our perception of the *red* constituent part of that *white,* and to exalt, on the contrary, our perception of the complementary part, or the *green.* If, for example, a *green* object be placed in juxtaposition with a *violet* color, contrast will enfeeble the perception of the *blue,* which is common to the two objects, and will enliven, on the other hand, the sensation of the *yellow* and *red,* by which they differ; the first of the two objects will therefore appear more *yellow* and the second more *red,* which is in accordance with the law indicated by M. Chevreul. If the objects finally differ in brightness, contrast will likewise intensify this difference, as in the case, for example, of black placed in presence of white, &c.

Without entirely denying the influence of contrast, M. Plateau thinks that this cause, though capable of increasing or even producing in certain cases the subjective phenomena of the second class, is not that to which they can be collectively attributed. His reasons are the following: 1st, how is it possible to admit that a moral cause should be competent to create, in complete darkness, a sensation of light purely imaginary, like that of the aureolas before mentioned? 2d, in the experiments of M. Chevreul, since the effect was manifested when two colors in juxtaposition had no ingredient common to them, the operation of contrast is more difficult to explain. Several physicists, Young among others, have attempted to extend to this second class of colors the hypothesis of a modification in the sensibility of the retina, by assuming that when a portion of the retina is subjected to the action of any color the surrounding parts lose something of their sensibility for that color. This theory, indeed, attributes the phenomena to a physical cause, but it is, nevertheless, wholly insufficient. What, in that case, would become of the phenomena of accidental colors produced in darkness? How should we explain the manifestation, in certain cases, of a tint identical with that of the colored object at some distance from the outline of that object? &c.

Theory of M. Plateau.—We shall, in the first place, remark with Scherffer that it results from the apparent change of magnitude of the accidental images, when projected on surfaces more or less distant; that these images are due to a physical modification of the organ. Such would, in effect, be the phenomenon produced, if it be admitted that a definite portion of the retina undergoes any modification; for, as the resulting image corresponds to a constant visual angle, that image must seem to us the greater according as we refer it to a greater distance. We see no reason, on the contrary, why it should be so, on the hypothesis of a moral cause; the image should then always appear to us of the same magnitude with the object. Analogy, moreover, would lead us to regard the colors of the second class as owing, like the first, to a physical modification of the organ.

If, now, we compare the accidental impressions with the corresponding real impressions, we shall soon see that the first should be regarded as of a nature *opposed* to that of the second: 1st, the opposition is evident for the case of a white object on a black ground; 2dly, it may be said, in general, that two complementary colors are opposed, since their tints neutralize one another by producing white; but, as has been seen, the opposition goes further in the case of a real impression and the corresponding accidental impression; it then destroys the perception of brightness by producing *black;* 3dly, while two complementary real colors produce *white* by their combination, two accidental complementary colors, on the contrary, produce *black*.

The foregoing being admitted, let us recur to the collective phenomena which succeed the contemplation of colored objects, or which accompany the contemplation. When a portion of the retina, after having been excited by the presence of a colored object, is suddenly withdrawn from that action, it regains by degrees the normal state, producing for us the phenomena of the persistence of the primitive impression and of the accidental colors of the first class.

On the other hand, if, while the retina is subjected to the action of the colored light, we examine the parts of the organ which surround the space directly excited, we see that the normal state is not found except at a distance more or less considerable from the contour of that space, and that this normal state is reached in passing through the effects of irradiation and of the accidental colors of the second class.

Thus, on the one hand, the *persistence of the primitive impression and the accidental colors of the first class* constitute the passage from the state of excitation of a portion of the retina to the normal state, when we consider that passage *in regard to time;* on the other hand, the *irradiation and the accidental colors of the second class* constitute the passage from the state of excitation of this same portion of the retina to the normal state, when we consider this passage *in regard to space.* All the phenomena, therefore, with which we are occupied must be referable to the same principle; and M. Plateau has pointed out that they are connected with one another in the most natural manner; that they are simple consequences of the law of continuity.

Let us examine, then, more closely the transition to the normal state in regard to *time.* When the retina is suddenly withdrawn from the action of the rays emitted from the colored object, the state of excitation of the organ at first continues for some time, growing feebler, yet *without changing its nature*, and thence results the persistence of the primitive impression; but presently this state of the organ gives place to an *opposite* state, whence results the *subjective image* of the object. This new sensation soon attains a maximum of intensity, and grows feebler in its turn, usually manifesting, however, an oscillatory progress, more or less regular, sometimes confined to a succession of disappearances and reappearances, sometimes alternating with recurrences of the primitive impression. Now, it is impossible not to see a striking analogy between these phenomena and the movement of a body removed from a position of stable equilibrium and which returns to it by an oscillatory action. Are we not naturally led, by the whole train of phenomena, to admit that the retina, diverted from its normal state by the presence of a colored object and then suddenly left to itself, at first regains rapidly the point of repose, but that, impelled by its own movement, it passes that point and attains an opposite state, whence it tends anew toward the point of repose, at which it finally arrives in a permanent manner only after a series of decreasing oscillations?

If, now, we examine under the same point of view the transition to the normal state in regard to *space*, it will be seen that the state of excitation caused by the light emanating from the object is not limited to the portion of the retina directly struck by that light, but that this state is prolonged without changing its nature to a small distance from the contour of that position, producing the phenomenon of irradiation, which we will presently explain; that beyond this limit is manifested an opposite state of the organ, from which results the accidental color, and that, in certain cases, at a still greater distance, the color of the object is repeated. Here, therefore, we have oscillations in regard to *space* instead of oscillations in regard to *time*, and we discover an equal analogy with natural phenomena; it is thus, in effect, that in a vibrating surface the parts separated by nodal lines exist in opposite states; that, in liquid waves, we find as we remove from the centre of excitation, alternate states of elevation and depression becoming less and less distinguishable.

It is under this general point of view that M. Plateau regards the assemblage of subjective phenomena. This theory, so simple that we had ourselves adopted it before knowing that it had been stated and maintained by the distinguished professor of Liege, does not merely explain all the facts, but almost enables us to discover them *a priori*. Let it be once supposed, in effect, that an organ which has been excited is then suddenly left to itself, and we cannot conceive otherwise than that it will return promptly, but in an interrupted manner, to its normal state. just as, during the excitation, it cannot be supposed that along the outline of the excited space there should be an abrupt passage from the state of excitation to the state of repose.

M. Plateau, in order the better to substantiate his ingenious theory, proceeded to submit to direct experiment the undulatory movement of the impression. He found, first, that the number of oscillations is greater in proportion to the greater length of time during which the object has been observed; second, that this time, on the contrary, does not appear to have any influence on the duration of each of the oscillations taken separately. The manner of operating was as follows: One of two physicists regarded fixedly, during a stated number of seconds, a bit of orange-colored paper placed on a black ground in a well-lighted situation, and then turned his eyes quickly on a white wall. He then indicated with the greatest possible precision the moments at which the accidental impression attained its successive *maxima* of intensity, while the other observer, provided with a watch marking the half-seconds, noted the time. As many as eleven reappearances were thus verified by MM. Plateau and Quételet.

One word more on the before-cited experiment of Dr. Smith. M. Plateau is of the opinion that the green appearance of the image perceived by the excited eye should be attributed to the circumstance that a part of the light of the candle traverses the envelopes of the eye and diffuses on the retina a reddish hue; the white image, being environed by this red light, must then seem tinged with the complementary color, green. What confirms him in this opinion is that the effect is perfectly well produced when the light of the candle is prevented, by means of a small opaque obstacle held near the eye, from penetrating by the pupil, while it is still permitted to illuminate the outer parts of the organ. In this case, the image of the flame is in no manner depicted on the retina, and yet the red and green appearances are equally produced. If it be objected, says M. Plateau, that the red light which traverses the envelopes of the eye is disseminated everywhere, and that it must diffuse itself also on the image of the small strip, which should consequently appear reddish and not greenish, I would answer by citing an analogous fact reported by M. Gergonne.

Into one of the schools of mathematics in the college of Montpellier fronting the east, the rays of the morning sun in summer only penetrate after having traversed the foliage of the acacias with which the court is planted. On entering this apartment, all the objects within it present a greenish tint, which seems to be gradually effaced; but then another phenomenon succeeds—all the traces of chalk upon the black-board become invested with hues more or less rose-colored. These traces, though receiving the green light like the rest of the tableau, are tinged nevertheless with the complementary color. M. Trechsel, in his memoir on colored shadows, has adduced, from experiment, an analogous phenomenon: let the sun's light, introduced cautiously by slightly raising the curtain for instance, be made to fall on the back of the *camera obscura* when it is colored *green* by an object-glass of that hue, the place illuminated by the sun's light will assume a tint of pale *red*, without the presence of any shadow. If the object-glass be *red*, the light of day will cause the place on which it falls to appear of a *greenish blue.* In the observation of M. Gergonne, the white chalk replaced the white ray.

M. Plateau's theory of objective colors is for us the precise expression of the facts; it explains everything, nay, it foresees everything. There is but one class of phenomena which still leaves anything like serious uncertainty. Colored shadows bear no resemblance to other accidental colors; the latter are, in general, very difficult to perceive and very fugitive; the colored shadows, on the contrary, are vivid, well defined, persistent, and the mind, however convinced, repels with difficulty the idea that they are no illusions, no purely subjective appearance, but in truth real colors. The following was said of them by Count Rumford, who produced them by receiving the sun's light through two small apertures perforated at sufficient distances to obtain two distinct shadows of the same opaque body, which apertures he covered with colored glasses: "The shadows," he says, "were tinged with an infinite variety of colors, the most unexpected and often the most beautiful; they varied continually, sometimes with inconceivable rapidity; the eyes were fascinated, and the attention involuntarily fixed on this magic tableau, equally enchanting and new. The clouds borne by the wind seemed, each in its turn, to bring an endless succession of different colors, with the most harmonious tints." Happily, to the enraptured amateur succeeded the cool philosopher; he would fain resist the testimony of his senses; he suspects that these brilliant appearances are deceptive, and that they can only be the effect of contrast between neighboring colors; he therefore submits these charming illusions to the control of new experiments, and another light, that of truth, dawns upon him. He places a friend in the position he had occupied in order to be certain that these brilliant phenomena have not ceased, and limiting his own vision by means of a tube blackened within, directs it so as to see the shadows alone; and now, while the friend is in ecstacies over its brilliant color, the rigid investigator sees nothing but an obscure and colorless shadow. The demonstration is exact and complete, and it will be no further necessary to explore these colored shadows with sensitive plates and papers in order to assure ourselves that in no manner do they produce the action indicated by their color.

The memoir of M. Fechner on subjective colors, published in 1840 in the Annals of Poggendorff, is the most recent treatise upon these subtle inquiries. We have studied it with great care, and if we do not here analyze it, it is because, while founded on observations well made and judiciously reasoned, it has seemed to us to contain no fact essentially new. On reading it, we have remained satisfied that, in the phenomenon of colored shadows, contrast alone, and not irradiation, is in action, and that the final reason of the problem is to be sought in a fundamental principle analogous to that of relative movement. This last principle, we know, consists in the essential fact that the relative movement of two or more bodies in motion by no means depends on the common forces or velocities, but solely on the forces in excess, that is to say, the forces which act on some of the bodies without acting on the others. In like manner, as regards light, the relative sensation that is really perceived by the eye, does not depend on the common tints, but on the excedent or differential tints. Thus, the portion of a green ground illuminated by a white ray on the white chalk, as we have seen, appears red in certain circumstances. So too, if a white ground has been illuminated with a colored light, with orange for instance, and white light be made to fall on a shadow projected in that colored light, the orange ray of the white light will not be sensible to the eye, because it is common to the ray and to the ground; we shall perceive, therefore, only the united sensation of the other rays contained in the pencil; these will produce a greenish white tint complementary to the orange, and with this the shadow will appear to be invested.

We are thus led to distinguish three orders of subjective colors: the first would have its cause in the successive vibrations of the retina; the second would be explained by irradiation, or by the participation of several portions of the retina vibrating simultaneously under the impression of a first excitation; the third would find its reason in contrast, or the elimination of the colors common to the two rays which act at the same time on the organ of vision.

We make no pretensions, however, to giving a definitive solution of this difficult problem, and pronouncing in the last resort between two physicists so distinguished as MM. Plateau and Fechner. Divided in a theoretical point of view, they have been, alas, too closely associated in a common misfortune. Victims of the experiments which they have pursued with excessive ardor, in the highly laudable but hazardous attempt to elucidate the nature of subjective or accidental colors, both have been smitten with blindness; Belgium and Germany have both been called upon to mourn the incapacity inflicted by that cruel infirmity on these noble martyrs of science. M. Fechner, after several years of suffering, has happily recovered his sight as if by miracle. It was our fortune to witness the restoration of the indefatigable physicist of Leipsig; with how much joy should we learn the recovery of the gifted and laborious author of the admirable memoir on irradiation,—a memoir which we propose presently to analyze, in order more clearly to illustrate what we have said respecting subjective colors.

Researches of M. Chevreul on the laws of simultaneous contrast.—The preceding dissertation comprises all that is essential in the learned researches of M. Chevreul; but we might reasonably be accused of injustice did we not give a summary account of the labors of our eminent compatriot, remarkable as they are in a practical point of view. M. Chevreul was the first who clearly stated this general law: in a case in which the eye sees at the same time two colors which are in contact with each other, it sees them as unlike as is possible; these mutual modifications of colors are not limited to the case in which the colored zones thus influenced are contiguous to one another; for we still perceive them when these zones are separated. He has clearly established that whenever the eye sees simultaneously two colored objects, all that is analogous in the sensation of the two colors undergoes such an abatement that all that is different becomes more sensible in the simultaneous impression of those two colors on the retina. The experimental demonstration of these two propositions essentially distinguishes his observations from those which had been made before. The law of these modifications being once known enables us to foresee the changes which two given colors, when we know the complementary colors of each of them and their relative tone, will undergo by juxtaposition; for these changes must be the result of the complementary of the one being added to the other; and if the two colors have not the same height of tone, that which is deeper will appear more so, and the other will appear lighter than it is; provided, however, that this last effect be not destroyed by the former. After so accurate and complete a study of the influence of colors, M. Chevreul might proceed with confidence to the details of application. He teaches by turns the difficult art of assorting colored threads in such a way as to faithfully reproduce the tints of a painting; of giving to paintings, to tapestry, to carpets the highest perfection of coloring; of stamping designs on cloth and paper; of adapting the accessories of furniture; of giving greater brilliancy to the colored windows of cathedrals; of distributing flowers in gardens; of choosing and distributing the colors of raiment; of judging more correctly of the tints of dyed stuffs. He gives the reason of the fact, long and generally known, that an assortment of complementary colors is never disagreeable, showing from his experiments that these colors re-enforce and embellish one another by proximity, whatever difference may otherwise exist between the bodies placed in juxtaposition. He proves, on the other hand, that if the non-complementary colors which approximate most to the seven primitive colors are, in general, embellished by juxtaposition, they yet seem to the eye more or less inappropriate; that the greater the analogy of colors the greater is the probability that their mutual juxtaposition will be injurious to one of them, or even to both; that if theory cannot prescribe, for the satisfaction of the eye, the arrangement of non-complementary colors in so decisive a manner as it is competent to do with regard to complementary colors, it is because of the impossibility of designating at present in a precise manner these innumerable colors of bodies in default of invariable types to which they might be referred, such as are, for instance, the colored rings of Newton.

Chromatic scale of M. Nobili.—These last expressions of M. Chevreul recall to us an excellent memoir of Nobili on colors in general, and particularly on a new chromatic scale. Author of metallochromy, that wonderful art by means of which he obtained on plates of steel the most brilliant colors by depositing on their surface, through electro-chemical processes, thin coats of different saline substances, of acetate of lead for instance, and persuaded that science never so well appreciates its own interest as when it associates itself with the arts, the distinguished physicist last mentioned devoted long hours to the formation of a chromatic scale composed of forty-four tints, disposed according to the order of the thin layers or films which form them. This assemblage produced an indescribable effect; it was of the same nature with that which is produced on the ear by a scale of semitones executed by an exquisite vocal organ; so great, indeed, was the pleasure of contemplating these colors which passed gradually from one tone to another, that the eye could scarcely be satiated with the view of so charming a spectacle. Nobili attempted to reproduce his scale in oil and water-colors; but the best executed copies gave but an imperfect idea of the original colors, and since Nobili is dead, carrying with him not the secret of his art but the untiring application, the incommunicable dexterity which alone can with certainty produce uniformity of tints, science and art will, perhaps, have long to deplore the want of a gamut of colors universally adopted.

A singular anomaly of the chromatic scale of Nobili particularly attracted attention; the green was found far from the blue, between the red and yellow. Men of art were surprised at this inversion; when the finest green tints of the gamut were submitted to them, impelled by habit, they placed them among the yellows and blues of the second interval; but, in that position they fatigued the eye; harmony was destroyed, and was not re-established except by returning to the primitive order. But in what consists this harmony? It may be felt, but not defined. Its principal character in nature is the conjunction of two complementary colors, or colors, as we have seen, of such a nature that one gives rise to the other. Venturi was right in saying: *Agreeable and harmonious above all is the union or succession of those colors which are in correspondence with one another, so that the sensation of one draws after it the imaginative sensation of the other.* In this we evidently have but one of the aspects of the harmony of colors; it extends much further; but, in the present state of the science, it would be impossible to convey complete ideas on the subject.

Nobili concluded his long memoir with some curious and useful remarks on the clearness, the strength, the beauty, the warmth and coolness, the liveliness and the gravity of colors. The clearest is the yellow; a clear tint results from the mixture of little color with much white light; a strong tint is the product of much color and little white light; orange is the brightest, yellow is monotonous; yellow and red are warm, light blue is cool; red and orange are gay, violet and indigo grave. Nobili undertakes to explain this last difference by an original comparison between colors and sounds. An exclamation, a cry of joy, are composed of notes which proceed from grave to shrill; a wail, an accent of pain, descend from shrill to grave; the same series of notes chanted by ascending from grave to shrill, or descending from shrill to grave, produces contrary effects; gaiety in the first case, sadness in the second. Now, if we take the complementary tints of red and violet, which are the bluish green and the greenish yellow, it will be seen that we pass, in the first case, from longer vibrations to shorter ones; in the second case it is the reverse. This digression will be pardoned us, if only as a grateful recollection of the eminent and excellent savant who honored us with a share of his friendship.

Singular effect in optics.—Before proceeding to treat of irradiation, we must notice a singular effect produced by the juxtaposition of certain colors which was observed by M. Wheatstone. This savant had remarked on some occasion that a carpet having a small green and red figure presented, when it was illuminated by gas-light and attentively observed, the appearance of a movement in all the figures of the design. To this optical illusion the name of *palpitating hearts* was given, because one of the figures of the pattern was a heart. M. Wheatstone made a trial of several other designs formed of other pairs of complementary colors, and ascertained that the movement was perceptible, but never so decidedly as with the colors green and red. He was not able to distinguish the phenomenon except by the light of gas; but M. Brewster afterwards perceived that it was produced also by the solar light admitted through a small hole in the darkened chamber. In order to explain this fact, M. Brewster recalls the experiment which shows that any fixed object appears to move when the light that illuminates it is constantly changing in position and intensity. The experiment is made by moving a candle before a statue rapidly and in all directions; the flashes of light and the shadows, incessantly shifting, produce different appearances which resemble the movements of the figures on the red and green carpet. In the case of the *palpitating hearts*, the mixture of two complementary colors, whether it be regarded directly or as producing accidental impressions, forms a succession of shades and lights which give rise to the apparent movement of the figures. When one of the *hearts* is looked at fixedly, it ceases to move almost entirely, while the others, which are seen obliquely, appear to vibrate in a very distinct manner; this effect is attributable to the augmentation of the sensibility of the retina in oblique vision, in proportion as the point which receives the impression is more remote from the opening of the pupil.

IRRADIATION.

Definition.—Irradiation is properly the phenomenon by virtue of which a luminous object environed by an obscure space appears more or less amplified. This apparent encroachment of the border of a luminous object on the obscure space which surrounds it, involves an opposite illusion in the case of a dark object projected on a luminous field: the dimensions of this object appear diminished, because the irradiation produced along its outline by the luminous field falls within the outline. A singular difference of opinion divided astronomers in relation even to the existence of aberration; some admitting it, others calling it in doubt; these doubts solely resulted from the fact that astronomers had not clearly distinguished the part played by ocular irradiation in observations made by means of the telescope.

Existence of irradiation.—To verify the phenomenon it suffices to cast the eyes on the luminous crescent of the moon. Every one knows that when the moon appears under the form of a crescent, while at the same time the rest of its disk is seen faintly illuminated by an ash-colored light, the exterior outline of the luminous portion seems to protrude considerably beyond that of the obscure portion; in other words, the crescent appears to form part of a disk sensibly greater than that to which the rest of the planet belongs. If a decisive experiment, capable of being repeated at all times, be desired, it may be made in the following manner: On a rectangular blackened pasteboard, 20 centimetres in height and 15 in breadth, let two white rectangles or windows, separated by a black band of a demi-centimetre in breadth, be left blank on the upper half; let this band, prolonged in blank, divide into two black portions the lower half of the pasteboard. Thus there are two bands of equal width, but the one white on a black ground, the other black on a white ground. To make use of this apparatus, it is to be placed vertically near a window, so as to be well lighted, and observed from a distance of four or five metres. The white band will now appear considerably wider than the upper and black band, and this apparent difference increases with the distance. It will be perceived that the apparatus is constructed in such a manner as to augment the effect resulting from irradiation; for if, on the one hand, the white band acquires apparently additional width, the black band, on the other, is diminished by the irradiations of the two lateral white spaces. Irradiation, therefore, with the naked eye, must be regarded as one of the best established facts of vision, and one of the most easily verified. Its intensity is not the same in different eyes, and even varies in the same individual, but there is no one who cannot observe it in a manner more or less distinct. In telescopes, it becomes complicated; its effect is diminished; the total error depends at once on the enlargement in itself, on the lustre of the image, the nature as well as state of the eye, and the perfection of the instrument—qualities essentially variable. If, when micrometers of double image were employed to measure the irradiation, it has not been practicable to find its sensible value; if it has even occurred that the double image micrometer has caused an irradiation to disappear which had been realized by the thread micrometer, this depends on a cause revealed by experiments which have rigorously demonstrated the following general principle: two irradiations in presence of and sufficiently near one another, both undergo a diminution. This diminution is so much the more considerable as the borders of the luminous spaces from which the two irradiations issue approach one another more closely.

On the cause of ocular irradiation.—The theory of irradiation now universally adopted consists in the admission that the impression produced on the eye by a luminous object is propagated on the retina to a small distance around the space directly excited by the light, so that the total sensation corresponds to an image somewhat greater than the true one. The principle of continuity on which this theory is founded is so simple, that, if the existence of irradiation were not known, it would seem that it might be foreseen *a priori*. Let us suppose, in effect, that a luminous or illuminated body projected on a ground completely black, is attentively looked at. The light emanating from this object will strike a definite portion of the retina, and the rest of the organ will not be subjected to any direct excitation. But is it presumable that the parts of the retina which immediately environ the portion directly excited should be in perfect repose? It will scarcely be admitted that a state of energetic excitement and a state of absolute repose should be thus, in the same organ, in immediate contact. We are hence led *a priori* to think that, around the image of the object, must be manifested some appearance which constitutes the gradual transition between the state of excitation of the part of the organ subjected to the direct action of the light, and the state of repose of the parts more remote. Now, in whatever manner this transition is effected, it must be considered as altogether probable that the excitation is propagated to a greater or less distance, without a change of nature, around the space struck by the light, and that hence there will result the sensation of a larger image. Irradiation will be thus, in relation to space, what the known phenomenon of the persistence of impressions of the retina is in relation to time. On one hand, when the retina, after having been excited for a certain time by the light emanating from an object, is suddenly withdrawn from that action, the impression still persists during some moments. On the other hand, while the retina is subjected to the action of the light emanating from an object, the impression will extend to a small distance around the image of that object. Both one and the other phenomenon would be results of a simple law of continuity, in virtue of which, when a portion of the organ is diverted from its normal condition, the dynamic state which results can neither be instantaneously annihilated, nor subsist in immediate contiguity with a state of perfect repose.

To these purely rational considerations, Mr. Plateau adds a great number of facts, a few of which only will be here cited: 1st. We know that the small space which, in the retina, corresponds to the insertion of the optic nerve, and which is called the *punctum cæcum*, appears insensible to the action of the direct light; hence, if a small white or colored object be placed on a black surface, and, closing one eye, we direct the other in such a manner that the image of this object falls on the point in question, the object disappears. Now, if instead of a colored object on a black ground, we place a black object on a colored ground, the object equally disappears, and the color of the surface extends itself over the place which it occupies. The most insensible part of the retina, therefore, is impressible by communication; in other words, the surrounding impression is propagated laterally over this small space; the same then, with stronger reason, must be the case in the sensitive portions. 2d. It is a constant fact that irradiation augments with the duration of the contemplation of the object; it is admitted, moreover, by physicists that by continued contemplation the retina becomes less and less susceptible of impression by the direct light; if these two facts be considered, the conclusion must be that in proportion as the direct light seems to lose its power over the retina, the propagated impression will be more strongly developed. 3d. On a colored ground place a narrow strip of white paper, or else trace a black line on a white surface, and let the eye be then fixed on another point distant from this small object some seven or eight centimetres, so that the latter shall be only seen indirectly. At the end of some instants, if the eye be kept motionless, the object completely disappears, and the color of the surrounding surface seems to extend over the place which it occupies. The lateral propagation of the impressions to the adjacent parts of the retina is therefore as nearly as possible demonstrated.

On the whole, then, the theory which attributes irradiation to a propagation of the impression is supported by considerations of reason, analogy, and experiment. This same theory, moreover, explains the different laws to which the phenomenon is subject. Hence we infer, in the first place, that irradiation, a phenomenon of sensation, cannot, as experience confirms, be the same in different persons; and that in the same eye it will be sometimes more, sometimes less, developed. It results, in the second place, that irradiation must appear so much the greater as the luminous object is more remote; for, first, the absolute width of the small band of propagated impression which surrounds on the retina the image of the object, cannot depend on that object, provided the latter preserves the same brightness and continues to be projected on a ground equally dark; hence it follows that the visual angle, subtended by this same width, must remain equally constant whatever be the distance. Now, as the observer necessarily refers the resulting appearance to the object itself, he must attribute to the small luminous band, which to him seems added to the contour of the object, an absolute width proportional to the distance which exists, or which he supposes to exist, between the object and his eye. In effect, if we imagine for an instant the appearance to be a reality, and that a similar small band is really added to the outline of the object, it can evidently not be seen under a visual angle constant and independent of the distance, except by making its absolute width vary proportionally with that distance. If, now, we could demonstrate that this apparent augmentation really corresponds to a visual angle constant for the same eye and the same object, this demonstration would constitute one of the most powerful arguments that could be brought in favor of the theory in question; for this constancy of the visual angle is the principal character of the phenomena of vision, which depend on a modification undergone by a portion of the retina of a constant extent. This important fact M. Plateau has succeeded in establishing by the help of an apparatus which we will not at present stop to describe. It is sufficiently demonstrated, by a long series of observations made on many persons, that the angular value of the irradiation is independent of the distance from the object to the eye; and it thence results, as a necessary corollary, that the absolute width which we attribute to the irradiation is, all things else being equal, proportional to the distance which exists, or which appears to us to exist, between the object and ourselves. Irradiation had not been observed hitherto by physicists and astronomers, except for remote objects. It will be found, however, to exist for small as well as great distances—only, the apparent effect diminishing proportionably with the distance, it strikes us less in the case of near objects; but, from the experiments of M. Plateau, it may be accepted as an ascertained fact that irradiation manifests itself at all distances, from the shortest susceptible of distinct division to any degree of remoteness. There was still an important question to resolve It was necessary to determine approximately the irradiation in the case of the same person in different circumstances. This M. Plateau has done, and has satisfied himself that for the same individual, and with an object of the same brightness, irradiation varies considerably from one day to another.

In the third place, it is well established that irradiation increases with the brightness of the object, and, again, the hypothesis of a propagation of the impression on the retina renders the most satisfactory reason for this fact; for it will not be disputed that the more energetic the excitation, the greater will be the distance to which it is propagated. But observation further demonstrates that the augmentation does not increase proportionally to the brightness; that its progress is much less rapid. For example, the angular diameters of the sun and moon being little different from one another, the brightness of the solar disk must be to that of the disk of the moon very nearly in the same ratio of intensity as the light which reaches us from these two bodies. But the brightness of the solar disk is nearly a thousand times that of the disk of the moon. If irradiation, then, increased proportionably with the brightness of the object, that which the sun developed would be enormous relatively to that of the moon, and the first of these orbs would present to the naked eye the aspect of an immense globe. The aspect really presented to us by the disk of the sun compels us, therefore, to admit that irradiation augments much less rapidly than the brightness of the object which produces it. Hence it follows that if the law which connects these two quantities be represented by a curve having for abscissæ the brightness, and for ordinates the corresponding irradiation, this curve will turn its concavity towards the axis of the abscissæ; and as the increase of irradiation, at first very considerable when we commence with a feeble brightness, becomes at last insensible when the brightness attains a certain limit, the curve in question will have an asymptote parallel to the axis of the abscissæ.

M. Plateau has sought to verify these conclusions by direct experiment, and to obtain the outline of the curve in question. To do so there was but one condition very difficult to fulfil—that, namely, of giving to the object successive degrees of brightness, at once determinate and having a known ratio with one another. This he accomplished in a very simple manner by taking advantage of a principle of photometry which we shall examine when considering the measurement of the intensities of light.

It remained to appreciate the influence exerted on irradiation by the greater or less illumination of the field surrounding the luminous object. As the disposition of the retina to receive propagated impressions bears, in virtue of the facts and observations above developed, an inverse ratio to the disposition of the same organ to receive direct impressions, we are authorized to believe that when the light acts directly on a portion of the retina, that portion becomes, from this circumstance itself, less apt to receive a propagated impression. Hence it is readily conceived that if the ground itself on which the luminous object is projected conveys to the eye a certain quantity of light, the direct impression which it produces will counteract the irradiation of the border of the object, and so much the more as the brightness of the ground is greater. This being granted, let us imagine an object which is projected on a ground not wholly deprived of light, and suppose that the illumination of that ground is made to increase gradually. The irradiation produced along the outline of the object will go on diminishing until the lustre of the field has become equal to that of the object. This limit being passed, if the former continues to increase, it is evident that the field will then produce, in its turn, an irradiation which will encroach upon the object, and which will consequently be developed in an inverse direction to the former as regards the line which bounds the real contour of that object. The irradiation will have passed, so to say, from positive to negative. This change, which is a direct consequence of the known facts, justifies the assumption that at the instant when the lustre of the field has beome equal to that of the object the irradiation of the latter is reduced to zero; and as the effect must be reciprocal, if, instead of an object and surrounding field, two objects be supposed of equal brightness and in contact, the irradiations of these two objects will be null at the point or line where they touch. This result has been verified.

To evince directly the influence exerted by the greater or less illumination of the field, recourse may be had to the following means: We begin by staining with black, for half its length, a rectangular piece of white paper, which is thus divided into two rectangles, one completely dark, the other semi-transparent; this latter half should be oiled for the purpose of increasing its transparence. In this paper we cut with the point of a penknife a longitudinal opening five millimetres in width, of which one half traverses the black, the other the translucid space. We then stretch this paper on a frame, or, what is better, paste it on a pane of glass. When this apparatus is placed against a window, it is apparent that the longitudinal opening will form a luminous band, one half of which is projected on a black ground, and the other on a ground possessing a certain degree of brightness, although much less than that of the band. If we now place ourselves at the distance of some metres, the two halves will appear to be unequal widths, the first seeming considerably to exceed the second.

From the foregoing discussion it results that the hypothesis of the propagation of the impression to the parts adjacent on the retina explains in a satisfactory manner all the laws of irradiation manifested to the naked eye. Unfortunately, adds M. Plateau, when, in order to observe the effects of irradiation, we equip the eye with a lens, a wholly different order of facts is presented. Thus, while a certain apparatus observed with a naked eye and at the distance of distinct vision manifested a very apparent irradiation, the virtual image of the same apparatus, produced by a strong magnifying glass situated at the same distance, and possessing moreover perceptibly the same brightness, exhibits no appreciable irradiation. We are thus led to this singular conclusion, that magnifying glasses appear to possess the faculty of considerably diminishing ocular irradiation. Is not this because the lens, by restricting the luminous pencil without proportionably modifying its brightness, causes it to occupy a smaller space on the retina? M. Plateau does not say so, and does not attempt to explain this peculiarity. With a convergent lens of sufficiently short focus, irradiation became too small to be perceived It is evident from this that in astronomic observations the eye-glass of the telescope must exert a decided action upon the irradiation which surrounds the image of a star.

M. Plateau has shown that the modification which irradiation undergoes from the interposition of a lens seems to be controlled by the following laws: First, irradiation is diminished by convergent lenses; this effect, considerable when the focal distance is short, grows weaker as that distance increases, becomes null when the latter is infinite, and changes its sign with it; that is to say, irradiation increases, on the contrary, under the influence of divergent lenses. Secondly, experiments seem to indicate that the action of the lenses seems to depend only on their focal distance, and not on the absolute curvature of their surface.

M. Plateau concludes his admirable memoir, written with the utmost clearness both of reasoning and style, by presenting, under the form of a summary, the following conclusions drawn from the doctrine of irradiation:

I. *Ocular irradiation.*—1. Irradiation is a well-established fact, easy to verify, very variable, but capable of being measured with precision under all circumstances. 2. It is manifested at all distances from the object which produces it, from the shortest distance of distinct vision to any degree of remoteness whatever. 3. The visual angle which subtends and which measures it is independent of the distance of the object. 4. It follows from this that the absolute breadth which we attribute to it is, all else being equal, proportional to the distance which exists, or which appears to us to exist, between the object and our eyes. 5. Irradiation increases with the brightness of the object, but according to a law much less rapid. If this law be represented by a curve having for abscissæ the successive values of the brightness commencing at zero, and for ordinates the corresponding values of the irradiation, this curve passes by the origin of the co-ordinates, turns its concavity towards the axis of the abscissæ, and presents an asymptote parallel to that axis. The curve is already very near its asymptote for a brightness of the order of that of the northern sky. 6. When the field which surrounds the object is not completely deprived of light, the irradiation of that object is diminished, and the more strongly in proportion as the brightness of the field approximates to an equality with that of the object. If equality takes place, irradiation vanishes. 7. Hence it follows that when two objects of equal brightness adjoin one another, irradiation is null for each of them at the point or line of contact. 8. Two irradiations in presence of and sufficiently near one another, both suffer diminution. This diminution is the more considerable as the borders of the luminous spaces whence the two irradiations emanate are in closer proximity. 9. Irradiation increases with the prolonged contemplation of the object. 10. In the same individual and for an object of the same brightness, the irradiation varies considerably from one day to another. 11. Irradiation is modified when a lens is placed before the eye; it is diminished by convergent lenses, and augmented by divergent lenses. 12. This action of lenses seems to depend only on their focal distance, and not on the absolute curves of their surfaces. It appears the more decided in proportion as the focal distance is shorter. 13. The most probable cause of irradiation seems to be the one now generally admitted, namely, that the excitation produced by the light is propagated on the retina a little beyond the contour of the image. By means of this principle, which is besides sustained by facts, we are able to give a reason for all the laws of irradiation observed with the naked eye; but difficulties are encountered when the action is exerted by lenses.

II. *Irradiation observed through astronomical instruments.*—14. The error produced in astronomical observations by what has been called in that case irradiation, proceeds from two causes essentially distinct—ocular irradiation and the aberrations of the telescope. 15. The part in that error which is due to ocular irradiation depends on the enlargement itself, on the brilliancy of the image, and on the eye of the observer. It is, moreover, considerably diminished by the action exercised by the eye-glass of the telescope, as a convergent lens placed before the eye, and this diminution is greater as the eye-glass is more powerful. As regards the eye of the observer, the effect will differ from one person to another, and, for the same person, will vary from one period to another. 16. This same part of the total error vanishes in observations in which a micrometer *à double image* is employed. 17. The other part of the total error which proceeds from the aberrations of the telescope varies, of course, with different instruments; but for the same telescope it may be considered as constant. 18. The effect of irradiation in telescopes, or the total error proceeding both from the ocular irradiation and the aberrations of the instrument, is necessarily variable, since it depends on variable elements; it may be insensible in certain cases, and attain a considerable value in others. 19. It is possible, even with an indifferent telescope and an eye very sensitive to irradiation, to obtain, by help of certain processes, results which may be considered as disengaged from this total error.

Objections of M. Arago and reply of M. Plateau.—On presenting to the Academy of Sciences the able memoir which we have been analyzing, M. Arago developed various considerations which led him to dissent from the physical explanation which the accomplished professor of Ghent has given of his experiments. M. Arago also recurred to the observations made twenty-five years before by himself in order to ascertain whether the measurements of the planetary diameters, taken with his telescope *à double image*, were affected by any irradiation; he spoke of the influence which it would seem that diaphragms placed before the object-glass might exert over measurements of this kind, since they augment so sensibly the diameter of the stars. He announced, in fine, the early publication of a memoir in which he would take occasion minutely to record the result of researches which, on the one hand, touch upon physiology, and, on the other, connect themselves with several of the most important questions of astronomical science. This promise was made in 1839; but the memoir never appeared. The following is the decisive reply of M. Plateau, as entered in the proceedings, to the objections of the distinguished secretary of the Academy:

"In the session of the 6th of May last M. Arago occupied the attention of the Academy with my memoir on irradiation, and presented at the same time some observations on the theoretical part of the work. He thought that the physiological explanation which I had sought to establish could not be maintained, and advanced a new theory, according to which irradiation is the result of the chromatic aberration of the eye. M. Arago's reasonings not having been printed, I am but imperfectly informed of them, and do not know how far they tend to refute the arguments which I have adduced in favor of the physiological theory. I shall therefore not repeat those arguments, but shall confine myself to an examination of the new hypothesis presented by M. Arago.

"It is true that physicists do not now recognize the eye as a perfectly achromatic instrument, and from this non-achromatism it necessarily follows that the images of objects on the retina are surrounded with a small band of aberration, which must slightly augment the apparent dimensions of luminous objects projected on a dark ground, and diminish that of dark objects projected on a luminous ground. But is this effect perceptible under ordinary circumstances, and has the small band of aberration sufficient breadth to allow us to distinguish it, and to attribute to it the known phenomenon of irradiation? This is the question, and one, I think, capable of solution.

"Let it first be remarked that by virtue of the very cause which produces it the small band which the chromatic aberration of the eye generates around images cannot be devoid of colors. Consequently, if the irradiation manifested by a white object on a black ground is due to this cause, it would seem that the object must appear colored on its borders. Now, among all the observers who have occupied themselves with ocular irradiation, not one has made mention of colored appearances, and in the numerous experiments which I have made on irradiation, under a multitude of different circumstances, I have never perceived anything of the kind. This absence of visible colors can scarcely be attributed to the small angular breadth of the irradiation; those in whom the phenomenon has much development will be easily able to satisfy themselves by repeating some of my experiments, or by observing the well-known appearance of the crescent moon, that the band of irradiation is of quite sufficient breadth to enable them to see these colors, if any existed.

"In the second place, I see not how it would be easy to explain by this aberration of refrangibility that singular law to which irradiation is subjected, namely, that when two objects of equal brightness are separated only by a small interval, each of them diminishes the irradiation of the other in the parts opposite, and this the more strongly in proportion as the two objects are nearer to one another, so that when at last they touch, irradiation is null for each of them at the point of contact. How shall we admit an action exerted by a luminous image on the aberration produced around another image?

"But we can easily decide by direct experiments whether ocular irradiation is or is not due to chromatic aberration. For this it will be sufficient to ascertain whether irradiation is still produced when the object is illuminated by a homogeneous light. If, in this case, irradiation is no longer perceived, we shall be bound to admit as true the hypothesis which attributes the phenomenon to the chromatic aberration of the eye; but if, on the contrary, irradiation still manifests itself, and to the same degree as with a compound light equal in brightness to the homogeneous light employed, it becomes impossible to seek in the aberration in question the cause of the phenomenon. Now, I have executed these experiments by the process which I here indicate.

"The homogeneous light of which I have made use is that yielded by the flame of a mixture of alcohol, water, and salt. With this mixture I saturated a parcel of cotton wick, which I placed behind a roughened glass arranged vertically. The mixture, kindled in the dark, yielded me a voluminous flame, and the glass observed from the other side formed a luminous field of sufficient brightness. To render the light still more homogeneous, I interposed between the flame and the roughened glass a yellow glass of an intense color. Everything being thus prepared, I placed successively before the roughened glass the open-worked apparatus described in § 28 of my memoir, and that which has served for my experiments of measurement, after having brought, in the latter, the vertical edge of the movable plate into prolongation with that of the fixed plate. The two pieces of apparatus were thus projected on a field having considerable brightness and receiving a light so closely approaching homogeneity that on observing them by refraction through a prism placed vertically at a distance of five metres, their images not only preserved great distinctness, but presented laterally only a greenish shade so slight that much attention was needed to perceive it. I should not forget to say that, in order to give to the eyes more sensibility, the experiments were not made by day in a darkened chamber, but at night.

"Now, under the circumstances just described, and which would necessarily exclude the effects which might have depended on the aberration of refrangibility, the above apparatus exhibited to me a highly developed irradiation. The same result manifested itself to MM. Burggræve and Le François, who had assisted me in the experiments of measurement reported in my memoir, and who are consequently accustomed to judge of the phenomena of irradiation. In order afterwards to compare the effects produced with those to which a compound light of similar brightness would give rise, I placed at the side of the same roughened glass another like it, behind which I so arranged several lighted candles as to illuminate it in a uniform manner and throw a light on the second mirror equal to that of the first. An opaque screen served to separate the candles and the flame of the alcohol in order that each of the mirrors should receive but one of the two lights. I had thus two luminous fields of the same brightness, one of which, however, was illuminated by a homogeneous yellow light, and the other by a light which, without being white as that of day, was still composite enough for the case in question.

"I then placed before these two luminous fields identically similar apparatus of irradiation, so that, by observing them simultaneously, it was easy to see whether the irradiations developed by the two lights sensibly differed. Now, this comparison, made by the two persons above named and myself, showed us no appreciable difference; each apparatus manifested a decided irradiation, but that which proceeded from the compound light had neither more nor less extent than that produced by the homogeneous light.

"These facts seem to me to lead unavoidably to these conclusions: that if the aberration of refrangibility in the eye must needs be admitted, irradiation should be attributed to some other cause, and that the effect of aberration must be considered as entirely masked, under ordinary circumstances, by the band of irradiation."

This vindication by M. Plateau has remained unanswered.

Colors produced in the eye by pressure or other indirect causes.—It is not light alone which produces in our eye a luminous sensation. It is well known that irritation, of whatever nature, which affects the retina occasions an impression of light. Compress, for instance, the jugular veins, and, however profound the surrounding darkness, flashes and luminous rays will seem to glance before the eyes. Under numerous and common circumstances the most varied colors appear to us without the agency of external light. The chief accidental causes of the production of these colors are:

1. *Mechanical irritation or pressure.*—A slight compression applied to the external angle of one eye produces a yellow spot in the same angle of the other eye if it is closed, and a blue spot if it is open; every one can easily make experiment of this. Newton had remarked that a weak pressure with the finger on the globe of the eye or blow on that organ produced the sensation of color; these colors he thought vanished instantly when the finger remained unmoved, but Brewster has proved that they continue as long as the pressure is maintained. He has shown also that when the eye is pressed so as to compress lightly the pulpy substance of the retina a circular spot of colorless light is produced, although the eye be in complete darkness and has not for hours been exposed to the light. If the eye is then illuminated it will be found that the compressed part of the retina is more sensible to the light than any other part, and seems in consequence more luminous. A slight compression of the retina, therefore, augments the sensibility for the light it receives, and gives rise to a sensation of light. On the contrary, adds M. Brewster, when the retina is dilated under the influence of light, it experiences an absolute blindness or becomes insensible to luminous impressions. These properties of the retina often evince themselves spontaneously, with different modifications, in consequence of the movement of the eye by its own muscles. Pressure, when it is too feeble to produce a luminous impression, may modify other impressions first produced on the retina. If, after having been directed toward the sun, the eye sees a reddish brown spectrum, pressure on another part of the retina will cause a change of the spectrum to green, and the brown will reappear when the pressure has ceased.

When pressure is applied at the same time to both eyes, the luminous appearances become remarkable in another way, and particularly in then affecting a regular form, which seems to be the same in all persons. If, for example, pressure be exerted on both eyes in opposite directions, whether to separate them or bring them closer together, there will first be perceived a bluish red light; then, at the end of some instants, a light of yellowish white tint. Almost at the same time this light will be separatedas into small lozenges, which will distribute themselves regularly upon a fasciculus of right lines converging towards the same centre, and which do not appear to deviate by more than 45° on each side from the perpendicular to the right line which passes by the centres of the two eyes. The fasciculus of right lines shows itself but for an instant and appears to be transformed into hyperbolas, all having for a common axis the perpendicular just spoken of, and common foci wherein are seated two uniform and reddish spots; these foci then swerve aside and the groundwork of the brilliant tableau assumes an undulatory appearance. When the pressure has ceased or is relaxed there is no longer anything perceived except a black spot surrounded with yellowish light and covered with red and yellow filaments, which vibrate with great rapidity. When the eyes are kept covered, this spot and the circle which surrounds it take in the end a uniform yellowish tint, which long persists and vanishes gradually. This phenomenon is rarely seen with all the circumstances we have described, because some practice is necessary to produce and to observe it, and the pressure on the eyes, which is quite painful, must be sufficiently strong.

2. *Electricity.*—The galvanic commotion produces not only a species of flash before the eyes, but the sensation of different colors, according to the different application of the electric poles. When the zinc or positive pole is placed in the mouth, and the copper or negative pole in the middle of the forehead, the eye receives the impression of a spot of violet color; if the poles be changed the spot becomes yellow.

3. *Imagination.*—This, in certain persons, acts in such a manner upon the organ of vision that if they represent to themselves any object, that object is instantly displayed before their eyes with its natural color and form. Goëthe assures us that when he closed his eyes and thought of any flower it immediately presented itself before him. Whenever Newton, after the experiment of which we have spoken, thought of the sun, that orb appeared to him at the instant, even in the midst of the most profound darkness. After the lapse of years he would have been able, had he chosen to encounter the risk, to reproduce at will, by the power of his imagination, the sensations which he had before experienced.

4. *Internal causes.*—Sensations of color are also evoked by congestion of the blood, lymphatic irritation of the eye, inflammation, &c. In these particular cases of indisposition the pressure of the blood vessels on the retina produces floating masses of light visible in the dark; first of pale blue, then green, then yellow, and sometimes even of red. All these colors are occasionally seen on the borders of the luminous mass.

Undulations excited in the retina by the action of luminous points and lines.—The following observations we owe to Sir D. Brewster: If we look through a narrow opening of about a demi-millimetre towards a bright portion of the sky or the flame of a candle, the luminous background will be seen covered with a great number of broken parallel lines, alternately lustrous and obscure. These lines are always parallel to the narrow cleft, and naturally change their place while the cleft is turned circularly before the eye. Through a certain number of parallel clefts, as through the teeth of a comb, the parallel broken lines are seen more distinctly, and, if an oblique motion be given to the comb in the direction of its teeth, the broken lines become still more distinct, although not so straight as at first, and we see new black lines placed in different directions, as if they were detached portions of a great number of dark ramifications. All these phenomena are better seen by employing homogeneous light. If we take two systems of narrow clefts and cross them under different angles, we shall see two systems of broken lines crossing each other under the same angles; and if, when the lines of the two systems are parallel, we give to one of them a rapid and alternating movement, perpendicularly to the direction of its clefts, the broken parallels will be perceived with particular distinctness.

Phenomena analogous to those just described may be seen by observing black and parallel lines traced on white paper, like those which represent the sea on an engraved chart, or by looking at the sky through the luminous intervals left between parallel threads. If the eye observes these objects fixedly and continuously, the black lines soon lose their straight direction and their parallelism, and present luminous spaces circumscribed nearly like rings of a certain number of links. When this change takes place, the eye which observes it feels a sensation of uneasiness—an effect which is produced also in the closed eye. When this dazzling effect supervenes the luminous spaces between the broken lines become colored, some with yellow, others with blue and green.

The phenomena produced in these two experiments pertain naturally to *rectilinear undulations propagated on the retina*, and the interference and crossing of the undulations, in consequence of which the black lines are broken into detached portions and the colors are produced, spring from the want of fixedness in the head and hand, which causes a parallelism in the successive undulations.

The action on the retina of small and dilating luminous points produces very interesting phenomena. If we observe the sun through a small opening situated at a great distance from the eye, or the small image of the sun formed by a convex lens or a concave mirror, or seen on a convex surface, the light which falls on the retina does not form a clear and definite image of the luminous point, but emits in all directions an infinity of rays which cover, in certain cases, almost the whole retina. These rays are extremely brilliant and sometimes accompanied by colors of singular variety and beauty. The glittering point propagates around itself circular undulations which are broken and colored by interference, and which, being in constant movement from the centre of the retina in all directions, occasion the irradiations above indicated. This curious phenomenon appears in all its splendor when we look at the luminous centre which results from the ignition of a jet of oxygen and hydrogen gas on a piece of chalk, or the combustion of points of charcoal at the poles of a strong battery. How often have we contemplated with surprise this circular zone of sparkling rays which appear under the form of discontinuous right lines scattered at equal distances from points by turns excessively brilliant and obscure.

If we observe through a narrow opening the luminous centres just described, a very singular effect is produced. A vortex of circular rays exhibits itself on each side of the brilliant point, having a rapid rotary movement. This remarkable configuration of the rays is evidently produced by the union of a system of parallel undulations with a system of spherical or circular waves; the intersections of the parallel fringes and divergent irradiations form the circular rays as in the case of ordinary caustics. All these phenomena, adds Sir D. Brewster, whatever be their true cause, evidently show that the light which falls on the retina exerts an action even on the parts which do not receive it directly, and that the same action renders other parts of the retina insensible to the light which actually falls upon them.

M. Péclet also has made an observation not less curious. If the sky be observed through a fissure half a millimetre in width dark stripes will be perceived irregularly distributed in the opening, but always in the same manner, whatever may be the size, form, and nature of the illuminating body; they do not change even when this body is sufficiently slender to yield fringes by diffraction; and when the opening is covered by a colored glass, the stripes seen through the glass are on the prolongation of those which are formed in the free part of the fissure. These stripes change place and become weaker when the width of the opening is increased; beyond a millimetre only very indistinct ones are perceptible near the borders. If the fissure is more remote, the stripes become less numerous, and entirely disappear at the distance of distinct vision. When a narrow fissure is before the eyes, and either the fissure or the head is inclined, the stripes change their place and arrangement. From these facts it seems evidently to result that the stripes in question are formed in the eye. Their origin may be satisfactorily explained by admitting that there exists in the organ a certain number of obscure points of very small diameter; for each luminous point of the fissure will project on the retina a shadow of that point, and the series of shadows projected by the different points of the fissure form a dark line which will be parallel to it; this hypothesis meets all the requirements of the phenomenon. The accuracy of this explanation is confirmed, moreover, by decisive experiments. Let a plate of glass, on which has been made with India ink a very small black point, be placed between the eye and a narrow fissure, and a dark stripe will be seen parallel to the fissure If the sky be observed through a narrow fissure whose length we can at will diminish by means of a movable plate whose edge is perpendicular to its direction, when the orifice has a length which differs little from the width, we shall perceive a circular field strewn with dark points, always disposed in the same manner in regard to the eye; and by the elongation of the fissure each dark point produces a stripe. As to the nature of the dark points of the eye, M. Péclet thinks that they proceed from the mammillated structure of the transparent cornea, or from the envelope of the aqueous humor; for each little mammilla will act as a lens of short focus; the light which traverses them will be dispersed in a very open cone; and each of them will cast upon the retina a shadow like an opaque body.

We must seek in the stripes of M. Péclet the explanation of an illusion by which M. Zantedeschi has allowed himself to be deceived. This Italian physicist has announced to the learned world, as a discovery worthy of attention and one which had escaped the penetrating eye of Fraunhofer, the presence in the solar spectrum of bands not transversal, but longitudinal or parallel to the lengthwise dimensions of the spectrum. Had they existed as an integral part of the spectrum, equally with the bands of Fraunhofer, these new dark bands would have been wholly inexplicable, or rather unintelligible. They are fortunately but an extrinsic peculiarity; they doubtless proceed either from the particles of dust inseparable from the edges of the fissure, or perhaps from the dark globules of the eye.

There remains to be noticed a singular fact observed by M. Libri. If a black and vertical line of little length be traced on a white wall, and we station ourselves at a little distance from this wall, a distance which will vary, for the same observer, with the width of the line, the quantity of reflected light, &c.; if, further, after having closed one eye, a very slender thread be placed near the other, so that in looking at the wall and the line there traced, the image of the thread shall seem to cut this line obliquely, the thread will appear, at the point of intersection, to be divided, so to say, into two parts, one of which will be elevated parallel to its first direction, and the other will be lowered. These parts will leave between their corresponding extremities a small void space, which will be precisely the place that the true image ought to occupy. It should be observed that the superior part will always appear lowered, and the inferior part, on the contrary, will appear above its real height. If the obliquity of the thread be diminished, and little by little its projection be brought to cut perpendicularly the line traced on the wall, the two extremities of the images will be seen gradually to approach each other, and then to form a single thread; and finally the same appearances will present themselves in an inverse direction, when by continuing to make the thread turn, we render it again oblique in a position opposite to the first.

On processes to render visible the ramifications of the retina.—The following experiment has facilitated, in a physiological point of view, the explanation of a certain number of facts; we owe it to Professor Purkinje, of Breslau: If a lighted candle be held before the eye, at the distance of about a foot, in a direction a little deviating from the line of distinct vision, the eye perceives, in general, a mass of reddish light around the candle, and, in this light, as on a background, may be seen the ramifications of the sanguineous vessels of the retina, the base of the optic nerve and the *foramen centrale.*

Sir D. Brewster holds it to be the better opinion that the light which surrounds the candle is reflected on the retina by the inner concave surface, whether of the crystalline or the cornea, and that the objects are, in one way or another, amplified by the concave surfaces. His own opinion on the subject is that the light is propagated from the luminous image of the candle, and that the retina, in contact with the blood-vessels, is insensible to the propagated light, although sensible to the direct light; and consequently the blood-vessels would be delineated in dark lines. As the retina does not extend over the *foramen centrale*, it would at this point naturally present a black spot, and, as to the confused vision of the optic nerve, the latter appears less luminous than the surrounding retina.

M. Wheatstone, who has varied this experiment in different ways, says that it succeeds best in a dark chamber. When, one of the eyes being withdrawn from the action of the light, the flame of a candle is placed beside the unshielded eye, so as not to occupy the least of the central part of the field of vision, then, so long as the candle is at rest, nothing is observed but a diminution of the sensibility of the retina for the light; but when the flame is moved, by raising or lowering it within a small limit, for a space of time which varies with the sensibility of the individual on whom the experiment is tried, the phenomenon presents itself spontaneously. The blood-vessels of the retina, with all their ramifications, exactly as they are represented in the plates of Sömmering, distinctly show themselves, and are projected apparently on a plane before the eye, with amplified dimensions. The image continues manifest only while the light is in motion, and disappears immediately or soon after it has passed to a state of rest.

M. Wheatstone does not accept the ingenious explanation of this appearance offered by Sir D. Brewster; he thinks there is no difficulty in accounting for the image; that it is evidently the shadow resulting from the obstruction of the light by the blood-vessels spread over the retina. The real difficulty was in explaining why this shadow is not always visible. To account for this, M. Wheatstone advances different facts which tend to prove that *an object more or less brilliant than the ground on which it is placed, becomes invisible when it is continually presented to the same point of the retina, and the rapidity of its disappearance is so much the greater as the difference of the luminous intensities between the object and the background is less; but, by changing continually the place of the image of the object on the retina, or by causing it to act intermittingly on the same point, the object may be rendered visible in a permanent manner.*

Applying this explanation to the phenomenon in question, M. Wheatstone observes that every time the flame of the candle changes its place, the shadows of the vessels fall on different points of the retina, which is evident from the movement of the image while the eye remains at rest, a movement which is always in a contrary direction to that of the flame: hence the shadow, by thus changing its place on the retina, remains, according to the law previously established, visible in a permanent manner; but when the flame is at rest, the shadow also is at rest, and consequently disappears.

M. Wheatstone has contrived an instrument for showing a singular modification of this experiment. It consists of a circular concave plate of metal, about two inches in diameter, blackened on the exterior face, and perforated at its centre with a hole nearly equal to the bore of a light gun; on the interior face is fixed a similar concave plate of roughened glass. By placing the opening between the eye and the flame of a candle and setting the plate in motion, so as continually to displace the image of the opening upon the retina, the blood-vessels will be seen as before, but in a brighter manner, and the spaces between the ramifications exhibit an innumerable quantity of small vessels, which anastomose in different directions, and which were invisible in the preceding experiment. At the very centre of the field of vision there is a small circular space in which no trace of vessels is seen.

M. Melloni on the coloring of the retina and the crystalline.—According to the theory which we have long developed and defended, vision is the result of extremely rapid vibrations excited in the retina by a certain series of ethereal undulations. The vibrations will depend less on the quantity of movement than on the particular disposition of the retina which renders it apt to vibrate with more or less facility, in unison with such or such an ethereal vibration; this, in terms of acoustics, would be a sort of resonance, proceeding from the accord or harmonic relation established between the tension of the molecular groups of the retina and the period of undulation of the incident ray. Those undulations, which are between the yellow and the orange, which correspond, as Fraunhofer has proved, to the maximum of luminous intensity, will better harmonize with the constitution of the retina, and will communicate to the molecules a more intense vibratory movement. The quantity of light perceived would depend on the intensity of the molecular vibrations, and the color on the number of the vibrations. By assuming that the red and violet undulations find in the retina a less perfect consonance than the yellow undulations, it will be readily comprehended that the former give a less quantity of light. This hypothesis is the more plausible, inasmuch as, followed to its final consequences, it leads to a very happy explanation of the obscure radiations, *chemical* or *calorific*, situated outside of the solar spectrum, radiations endowed, as will be shown further on, with all the properties of luminous rays, visibility excepted.

It must needs, then, be admitted that the ethereal undulations of different colored bands of the spectrum have a different aptitude for exciting the vibrations of the retina, and that the maximum of effect pertains to the yellow.

It has been shown that the substances which vibrate with the same facility under the action of luminous undulations of any length whatever are white; colored substances, on the contrary, are those which vibrate with more intensity under the influence of one or of several species of luminous undulations, while showing themselves less sensible to others. Now, it is a constant fact that the yellow undulations produce by *consonance* the *maximum* of effect on the retina. The retina, then, must be yellow, and not colorless, as has been thought till now. This conclusion rests on a perfect analogy of luminous properties between the retina and the different bodies of nature. It may be conceived, however, that the vital force might communicate to the retina, even if white, a degree of excitability more in unison with the color of yellow. To decide, therefore, this question of the color of the retina, appeal must of necessity be had to observation, and this has been done by M. Melloni.

We must presume, he says, that no observer sufficiently experienced has examined with attention this precious membrane; for, otherwise, he would certainly have recognized that it possesses a yellow tint very distinguishable. If we attentively observe a section of the retina, it will be seen that its thickness increases from the borders to the centre, occupied by the yellow spot of Sömmering, or rather of Buzzi, an Italian physician, who discovered it in 1782. Nor is it the central and thickest part of the retina alone which is and which exhibits a yellow tint to the eye, whether naked or equipped with a microscope; it is the entire retina, and if near the border the color is no longer distinct, it is solely on account of the excessive thinness of the membrane. The yellow color, indeed, cannot be mistaken when the retina is observed under an oblique incidence, or when the thin portions are folded upon themselves. In the eye of a man dead with the symptoms of very intense jaundice, the whole retina had a most decided tint, and the central spot had acquired an extraordinary vivacity of coloration.

The yellow of the central spot, which in our view constitutes the natural color of the retina, fades and disappears as age advances—an observation which M. Melloni has not found reported in any treatise on physics. It presents itself, however, in a very marked manner on a comparison of retinas taken at different epochs of life. From the change of color in the retina there would necessarily result an alteration in regard to the perception of the elementary rays; but, by one of those innumerable provisions which surprise us at every step in the science of the development of organized beings, nature has taken measures to repair the effects of this disorder. The crystalline is perfectly limpid and colorless up to the age of twenty-five or thirty years; but this period having passed, we see it assume a very slight tint of straw-colored yellow, which is developed at first on the central part, afterwards attains the borders, augments progressively in value, and finally becomes, in aged persons of seventy-five or eighty years, quite as decided as the color of yellow amber. If we consider the effect produced upon the sight by this new development of color, it will be at once comprehended that the yellow acquired by the crystalline is destined to repair the loss of that tint in the retina. To show that the sum of the two variations really compensate for one another, M. Melloni procured a number of eyes of very different ages, and, having extracted the crystallines, he placed the latter on the central parts of the corresponding retinas; all the systems were thus found to present the same shade of yellow. The experiment carried to the two opposite limits is one of great interest; for in early youth the coloration, which is not yet developed in the crystalline, is amply manifested on the retina, while in decrepitude it has entirely retreated to the crystalline, leaving no trace of itself on the retina. The aged crystalline placed beside the young retina presents the same tint, notwithstanding the vast difference of constitution.

The appearance and progress of the yellow tint in the crystalline constitutes, therefore, a true process of what might be called *attuning*, put in operation by nature for the purpose of maintaining at the same luminous pitch the instrument of vision. It can thus be understood why white continues to be white for our eyes at all the epochs of life, notwithstanding the increasing coloration of the crystalline and the interposition of a yellow medium between external objects and the retina.

DALTONISM, OR INNATE IMPERFECTIONS IN THE PERCEPTION OF COLORS.

Definition.—There are persons whose eyes are incapable of distinguishing colors. They readily perceive the form of objects, distinguish light from darkness, appreciating the slightest differences; but they mistake certain colors. Their retina, insensible for some tints, cannot transmit their impression to the brain. The number of such persons is much more considerable than is generally thought. Of forty youths who pursued the higher studies of the gymnasium at Dresden, M. Seebeck found five to be daltonians, some of whom, however, were without consciousness of their infirmity. One of the singular features of the defect is that it is often hereditary. Three remarkable memoirs—published, one, in 1837, by M. Seebeck, jr.; the second, at Geneva, by M. Elie Wartman; the third, at Paris, by M. Victor Zokalski, in 1841—seem to embrace all that relates to this difficult question, and to the analysis of these we propose to devote a few pages.

Observations.—As all researches on the subject set out with the observations made by Dalton on himself, an account of which he inserted in the memoirs of the Literary Society of Manchester, we shall give substantially a transcription of it: "I had always thought," says Dalton, "that their true name was often not applied to colors. *Violet*, to designate the flower of that color, seemed, of course, proper; but if the term *red* were substituted, this license appeared to me neither just nor natural. *Blue*, to be sure, might be permitted to replace the word *violet*, for these two colors appear to me closely analogous, while red and violet bear no relation to one another. When pursuing my scientific studies I gave particular attention to optics. The theory of light and of colors was familiar to me before I had discovered the defect of my vision, a defect which I had always attributed to the confusion prevailing in the terminology of colors. In the course of the year 1790 I occupied myself with botany, and this study involved a particular notice of colors. White, yellow, or green I called without hesitation by its proper name; while for me there was little difference between purple, violet, and crimson. I would often ask my friends which color was blue and which violet; but the inquiry was taken for a pleasantry and answered slightingly. Meanwhile the peculiarity of my vision was not clearly known to myself till the autumn of 1792. I was one day examining the flower of the *geranium zonale* by the light of a candle. This flower, which by daylight appears to me blue, and which is, in fact, violet, now seemed entirely changed. The supposed blue color had wholly disappeared and given place to red, which is for me the exact opposite of blue. I doubted not that this change occurred to every one, and asked some of my friends if it were so. Judge of my surprise when I learned that no one, except my brother, saw as I did. This observation taught me that my vision, as well as my brother's, differed from that of the rest of the world, and that artificial light produced to us changes of color which were not perceived by other persons.

"Two years passed before I applied myself to serious inquiry on the subject; at the end of which time I availed myself of the services of a friend profoundly skilled in the theory of colors, their distinctions, and constitution. I am near-sighted, and the eye-glasses which suit me best are of five-inch focus. I see well at a convenient distance; but when the weather is obscure or too bright I distinguish with difficulty. It was on the solar spectrum that I commenced my observations; six colors are there universally recognized: red, orange, yellow, green, blue, and purple, which last Newton divided into two others, indigo and violet. As for myself, I recognize in the spectrum but two, or at most perhaps three colors—the yellow, the blue, and the purple. My yellow comprises the red, orange, yellow, and green of all the world; my blue is so confounded with the purple that I hardly discern them to be other than one and the same color. The part of the spectrum which is called red seems to me scarcely anything else but a shadow or an absence of light. The yellow, orange, and green are, for me, the same color with different degrees of intensity. The point of the spectrum at which the green touches the blue presents to me a highly striking contrast and a most decided difference. The distinction of blue and purple is far from being so marked; for the purple, in my estimate, is but a mixture of blue and shadow. When I look at the flame of a candle through a prism, nearly the same phenomena present themselves; yet the red seems more lively than that of the solar spectrum. The following is a statement of the manner in which each color is presented to me, whether by solar or by artificial light:

"*Red:* By daylight I comprise under this name crimson, scarlet, red, and violet. Crimson resembles blue with which has been mingled a shade of deep brown, its varieties not being so different as to be distinguishable. This color appears to me grave in the presence of bright colors. Blue cloth and crimson offer me no decided difference. Violet is composed of red and blue, but it produces no other impression on me than that of a pale and faint blue; if placed beside bright blue it seems the same color, only somewhat tarnished. I regard as blue the rose, the violet, the common clover, and several kinds of geranium. A spot of common ink on white paper is, for me, of the same color wi h the cheek of a person glowing with health. The red and scarlet form an entirely different class from the preceding colors; I perceive in them not the least trace ot blue; the scarlet is rather brighter than the red. Blood, in my view, has a deep bottle-green color. By candle-light red and scarlet become more brilliant and vivid. These two colors, which by day present a deep cineritious gray, assume a strange brilliancy under an artificial light. Crimson loses its blue and changes into a yellowish green. But it is the violet which changes most, and presents the greatest difference when seen by day or by candle-light; here there is no longer a trace of blue, but it seems to be composed of yellow and red, as by day it is composed of red and blue. As to yellow and orange, my vision is absolutely the same with that of all the world, as seen by day or by artificial light.

"*Green* by daylight: I am certain that of this color I have a peculiar idea; it seems to me but little different from red. In order to recognize in it a peculiar tint I in vain place a bay leaf beside a stick of sealing-wax: hence it may be safely inferred that I see green or red, or both colors, differently from the rest of the world. Orange and bright green appear to me to have much resemblance. The green most agreeable to me is that which is very deep, and it becomes the more distinct as it verges upon yellow. I recognize plants as well as another, and am able, like others, to appreciate their differences or resemblances. Nevertheless, an infusion of tea, or a solution of liver of sulphur, whatever they may be to others, are green for me; the green cloth which covers some tables appears to me brown and soiled, and I am persuaded that a mixture of brown and red would, to my eyes, well replace that color; but when such cloth begins to grow old and becomes yellow to others, then only does it assume for me a green tint. Like all the world, I find a difficulty in distinguishing green by artificial light, which for me, as for others, then closely approaches to blue. In all these cases I have been satisfied by numerous trials that my brother sees just like myself, and therefore differently from all other persons.

"Persuaded that cases of this kind could not fail to be of interest to inquisitive minds, I was preparing to publish a statement of the peculiarities of my vision, when I remembered having read in the *Philosophical Transactions* some account of a mariner of Maryport, named Harris, who, it was represented, *could not distinguish between colors.* The case of Harris seeming somewhat different from my own, I thought that a comparison might cast some light on the nature of our imperfection, and wrote to obtain exact details on the subject; the information received convinced me that the defects of vision in the two cases were not absolutely parallel, and this suspicion was confirmed on my sending at least twenty specimens of colored riband to Maryport, with a request that they might be submitted to the inspection of Harris both by solar and artificial light. Subsequent observation has satisfied me that cases of this nature are by no means so rare as we might suppose them to be; in carefully studying the vision of persons of my acquaintance I have found more than twenty who might be ranged in the same category with myself; one or two differed only a little from the others. Among twenty-five pupils to whom I was one day endeavoring to explain this subject, there were found to be two affected in a similar manner; and, another day, one in the same number of auditors. Among all these persons my brother and I alone could not distinguish blue from violet by daylight, while, by candle-light we both recognized a great difference between these colors. With regard to green, all the persons spoken of had perceptions precisely like our own. As has been said, however, I have observed some twenty persons whose vision was similar to mine, but I have never heard of any woman who was affected with the visual imperfection of which we are speaking."

A second account, accompanied by details, is to be found in the writings of Sommer, a man of eminence, who describes as follows the infirmity under which he labored:

"The sub-orbital part of my eye is very prominent, and covered with thick blond hairs, which form an arch a little flattened. The folds of the upper eyelids are parallel to the border of the orbit, the lower eyelids thick, and the lashes somewhat short. The bulb and the cornea are very little arched, and the iris is throughout of a greenish blue, more deeply colored towards the pupillary border, and sprinkled over with yellowish points; the pupil is normal as to size and mobility. On the whole, the eye bears all the characters of a sanguine temperament. My sight, excellent in youth, was much enfeebled through being exerted by artificial light; still I see very well both near and distant objects. Different degrees of intensity of light act upon my vision as upon that of other persons.

"The *muscæ volitantes* which, for some ten years, have floated before my view, alarmed me at first, because I took the phenomenon for a precursor of amaurosis, but its stationary condition soon reassured me. There is no connection, however, between this accident and the imperfection of my sight in regard to the distinction of colors; for, as that imperfection existed from birth, my associates remarked it as well as myself, and I still recall many a laughable adventure to which it gave rise in my childhood.

"I can always distinguish, by daylight, yellow from blue, bright blue from green, deep red from black; the last I very well discriminate from bright colors, but I often confound it with the dark green and blue. Yellow, black, deep blue are for me the fundamental colors. Although I well perceive a difference between the colors often presented to me, yet I could not undertake to designate them by name without risk of committing an error. If I hold beside one another a leaf and a stick of sealing wax, I recognize very clearly the difference of intensity in the two colors, but I could not affirm that one of the objects is green, the other red, if the object itself did not enable me to divine it. Blue, not deep, and red, not intense, have for me a great resemblance. Placed near one another, the bright brown which approaches yellow, green somewhat deep, and red of a decided hue, all seem to me shades of one and the same color. I confound blue with red, green with brown, orange with bright brown, and a multitude of other compound colors. Crimson, purple, lilac, flame color, &c., I know only by name, and it is not possible to convey to me the least idea of them, though of course I have often had these colors before my eyes. In general, I cannot retain colors, and if a series of them were shown me and their names indicated, I should almost certainly be mistaken in recounting them if their positions were altered.

"I remember that on meeting, one day, a lady wearing a blue hat ornamented with roses, I could make no distinction between the hat and the roses, but thought both red. At another time, when walking out, a shower of rain came on, and a multitude of red umbrellas (then in fashion) were raised; to me they all appeared of the same color with the sky. I confound constantly the colors of dress, and of flowers, and there is not a tint which does not recall some ludicrous mistake which it has occasioned me to make. The impositions to which I am thus exposed, the fear of being charged with singularity, cause me to use much reserve in the indication of colors, and to call to my help all the particulars which may enable me to determine their character. Many objects have each a specialty of color, which preserves me from making as many blunders as might be supposed. Thus, usually the garb of a huntsman may safely be called green; nor can one be often deceived in speaking of tiles or flesh as red, or of the sky as blue; but if the least change of color occurs in these objects, my judgment is directly and necessarily at fault. For the few colors which I recognize I possess a good memory and distinct conception. Artificial light, however, confuses for me all tints, and I cannot then venture to indicate colors which I recognize well enough by the light of the sun. The landscape which Göethe caused to be painted without any trace of blue, and which, he maintained, ought to appear entirely proper to an *akyanopist*, had nothing unnatural to me; I should not even have remarked the complete absence of blue had I not been previously apprised of it. The rainbow seems to me composed of blue and yellow; I perceive, indeed, that there are more than two tints, but I cannot distinguish them, nor even conceive the difference."

Let us cite, as a last example, that of the Harris family, reported by M. Hudart in a letter to the celebrated Priestley:

"Harris, a shoemaker, of Maryport, in Cumberland, could judge without difficulty of the size and form of bodies, but he was without the power of distinguishing their colors. He was well aware that objects offered to the sight of other men something which they readily recognized, though undiscernible to him; that they expressed in words certain qualities of those objects which he could only designate at hazard and with hesitation. It was at the age of seven that he first became apprised of this imperfection. Having found, one day, a child's stocking, he entered a neighboring house to learn to whom it belonged; he observed that every one spoke of it as a red stocking, but as the term had no meaning for him, he regarded it as merely superfluous and thought it enough to call it a stocking. The circumstance remained, however, impressed on his mind, and some similar observations which he afterwards made enlightened him as to the state of his vision.

"The sensation of color being the first we experience, it will appear surprising, no doubt, that he had not sooner discovered what was wanting to his perceptions. But this difficulty will be removed when it is stated that his relatives were Quakers, and that a grave uniformity of colors is generally as strictly observed by this class of persons in their dwellings as in their vesture. Harris discerned objects, whatever their distance, as well as others, when their form sufficed to convey an exact idea; but his vision was at fault as regarded bodies which are only appreciable by the color. His companions could distinguish from afar the *cherries* on a tree, where to him there appeared nothing but *leaves*, and it was only by approaching near enough to judge of the relative form and size that he could discriminate between the foliage and fruit. He was quite capable of distinguishing between black and white or a bright light; and for him the other colors ranged between these two extremes according to their brightness or obscurity. Hence he was often led to confound or comprise under one name very different colors, because to him they presented the same lustre. If a riband was striped with different colors, he did not confound it with a plain one, but he could only perceive in it degrees of light and shade more or less marked.

"Harris was an intelligent man and fond of knowledge, especially that which relates to light and colors. Two of his brothers were born with the same defect as himself, but two other brothers and two sisters were exempt. Having met, in Dublin, with one of his brothers, a mariner, of Maryport, I proposed to examine his vision, and obtained the following results: As I had no prism at command, I asked him if he had ever seen a rainbow; he assured me that he had often observed it, had counted all the colors, but could not call them by name. I presented a riband, in which he recognized several colors and attempted to name that of each of the squares of which it was composed. The white ones he distinguished without hesitation, but he confounded the black with the brown, saying that they were only shades of the same color. When pointed to a *green* square, he replied: 'To me it seems that this is the color which you call yellow.' A red square, with him, was *blue*, and he was confident of seeing *green* when I showed him *orange*. I asked him, finally, whether he thought that what we call *colors* were anything else than degrees of brightness and shade, or whether he believed that there was still some other difference. To this he replied doubtingly, that he imagined he could observe something else, of which, however, he could give no account."

We should exceed our limits if we attempted to present to our readers the observations carefully collected by the authors before cited; a summary account of them will suffice and lead us naturally to a consideration of the theoretical views, which are more pertinent to our purpose. M. Seebeck has distributed the daltonians into two classes: the first comprises the individuals who are rather mistaken in the degree of the coloring than the nature of the color. The tints which they more or less confound are: the bright orange and pure yellow; the deep orange, the clear yellowish or brownish green and the yellowish brown; the pure clear green, the grayish brown and flesh-color; the rose-colored red and the green rather bluish than yellowish; crimson and deep green and chestnut-brown; bluish green and dull violet; lilac and bluish gray; azure and bluish gray and lilac. Their perception is very defective as regards the specific impression of colors in general, and especially so for red and consequently green, which they discriminate but little or not at all from gray. Yellow is the color of which their appreciation is most correct, although they often see less difference between this color and the appearance of colorless bodies than is discerned by eyes of normal conformation. The second division comprises persons who confound bright orange with greenish yellow, brownish yellow and pure yellow; deep orange with brown yellow and grass green; red and deep olive green; vermilion and deep brown, carmine and blackish green; carnation, grayish brown, and bluish green; dull bluish gray and brownish gray, yellowish rose-color and pure gray, deep rose with lilac, azure and gray passing into lilac; crimson and violet, deep violet and deep blue. They have but a feeble perception of the least refrangible rays; it is in this that their most strikingly distinctive characteristic consists, and on this rests chiefly the distinction between the two classes. It explains not only why the individuals of the second class, like those of the first, confound red with dark green, but will account moreover for all the differences which separate them, if it be remarked that the absence of the orange rays occasions much affinity between blue, colorless light, and even red.

We know that as the obscurity becomes greater the least refrangible rays disappear first from the light of the atmosphere, and that from this more prompt disappearance arise the changes of color observed under such circumstances. If, therefore, the defect of sensibility for these rays characterizes the daltonism of the second class, the daltonians of the first class should find themselves, during the approach of darkness, nearly in the same condition with those of the second. Towards evening, M. Seebeck presented to one of the first class of daltonians two series of colors, of one of which he judged very correctly, while he was constantly mistaken as regarded the other; in proportion as the obscurity increased, the first series betrayed him more and more into error, while the second was better and better appreciated; when the obscurity became considerable, the second series was that on which he was no longer deceived, while he erred completely in regard to the first. He could soon no longer distinguish, like the daltonians of the second class, by the light of day, the blue sky from the rosy red; finally, he judged not more correctly of the colors of the second series: the red paper which, by day, appeared to him too dazzling, then appeared too dark. Subjected to the same tests, the eyes of the daltonians of the second class acted quite differently: the obscurity had at first produced on them no effect; their judgments respecting the second series of colors were invariable; more and more correct, they agreed with those of the first daltonian, especially in the impression that, under the influence of the growing obscurity, the red paper had become darker. Obscurity then reduces to the second class the daltonians of the first; reciprocally, by a bright orange green placed before the eye the daltonians of the second would be reduced to the first class. In these observations M. Seebeck has presented us with some very original views, and has proved himself here, as in all his labors, a profound and skilful physicist.

Classification of Dr. Szokalski.—M. Szokalski comprises under the name *chromato-pseudopsy*—formed from three Greek words χρωμα, color; Ψευδος, false; ὄψις, sight—the phenomena which, with M. Wartmann, we have designated by the more simple term *Daltonism.* He distinguishes five classes of daltonians: 1st. That of persons in whom the sense of color fails almost completely, and who see in place of the principal colors, yellow, red, and blue, only different degrees of white and black. The existence of this class is doubtful, for all eyes have at least the sensation of yellow. 2d. That of persons who distinguish the yellow. To these external objects appear colored with the tints produced by different combinations of yellow, blue, and black. 3d. That of persons who not only see yellow, but are moreover capable of a peculiar and uniform perception of blue and red; these are the *akyanopists* of Göethe. 4th. That of persons only destitute of the perception of red, which appears to them ashy-gray. 5th. That of individuals who distinguish all the colors, but not in a distinct manner: instead of being able to discriminate the mixture of two colors, they see but one of them. These divisions are rather arbitrary than real; they seem to us suggested by an incomplete and hypothetical study of the facts. We should have much confidence in M. Szokalski as a practical oculist, but his pamphlet proves that he is not a competent physicist; it might be said, indeed, that he has undertaken to wrest daltonism completely from the domain of physics. "The immediate cause," he says, "of chromato-pseudopsy consists in the confusion of the determinative functions of the brain which furnish us with the perception of colors." This is evidently an error; before the confusion of the functions of the brain—an imaginary confusion probably—there is certainly a physical cause, the insensibility of the retina. M. Szokalski is convinced that the physicists have but false ideas of colors: while they are entirely and essentially subjective, the physicists have made them objective by forgetting the important part which our eye plays in their production. We shall not stop to repel a reproach which has no foundation; there is no physicist who does not admit that *colors are produced in our eyes;* that the sensation of color depends on a certain change in the eye, a change which is caused by the operation of some stimulus. Yet more than this: the different sensations of red, orange, &c., arise necessarily from different stimulants; in each luminous ray, therefore, there is evidently a distinctive character which fits it for determining such or such a sensation, and it would seem then quite natural to attribute to it the color. But we leave these useless discussions and proceed to analyze the much more methodical memoir of M. Elie Wartmann.

Number of Daltonians.—The number is much more considerable than is commonly supposed. M. Seebeck detected five daltonians in rather more than forty youths who composed the two higher classes of a gymnasium in Berlin, and Professor Pierre Prevost has asserted that among twenty individuals there is at least one who is a daltonian.

Characteristic signs.—Are there any means of deciding, from simple inspection of the eye, whether daltonism exists? M. Wartmann hesitates to affirm that in all cases the reply should be negative. It would seem that the daltonians whose eyes are of a hazel brown sometimes offer, under an incidence more or less oblique, a yellow reflection of a peculiar tint, the presence of which may lead us to suspect daltonism. It has been erroneously assumed that there were more daltonians with blue than with black eyes.

Apportionment as to sex.—It can scarcely be doubted that daltonism is much more common in men than women. M. Szokalski having assembled a number of women and showed them separately specimens of variously colored riband, requested each of them to indicate the name and nature of the colors. He was struck with the perfect accord which existed among all their answers. The same trial, made with a certain number of men, was far from yielding the same result. Scarcely were the same specimens shown to them, when a lively discussion arose among them on the names of the colors. The faculty of discerning colors would seem, therefore, to predominate in women. Gall asserts that the organ of color is more developed in woman than in man.

Influence of age and parentage.—Daltonism dates ordinarily from birth. It is often hereditary. It sometimes affects one, two, or more members of a family, while the others remain exempt. It is remarkable that the transmission of daltonism takes place rather through females than males, although females, as we have seen, are themselves very rarely subject to the infirmity. M. Cunier, however, has recently ascertained a case of hereditary daltonism in five consecutive generations, and always in females, who were alone affected.

Explanation of Daltonism.—Some persons will see nothing in this affection but a defect of intelligence or perturbation of the functions of the brain; others assume an abnormal coloration of certain parts of the eye which modifies the true colors; others, again, think that the cause should be sought in the sensibility, more or less great, not only of the retina in general, but specially of the different nervous fibres which vibrate in unison with such or such a color, or which are impressible by such or such a tint. The first opinion we shall not stop to refute. The second was held by Dalton, who concluded from his personal observation that the humors of his own eyes and of those of his students were tinged with blue. He ascribed this peculiar coloring to the vitreous humor; but the researches of anatomists have never brought such coloration to light. Moreover there are observed facts which would be very imperfectly explained on this hypothesis. Persons who habitually use blue glasses are not thereby exposed to a confusion of colors. Göethe, on the other hand, conceived that daltonians were insensible to blue, but saw in its place a pale purple or rose color. This view, however, is evidently inconsistent with facts.

From the sum of the observations heretofore made, two conclusions, we think, may be drawn: 1st, that daltonism never extends to yellow, a color which all eyes are capable of seeing; 2d, that the sensations of two complementary colors are inseparable, in the meaning that the eye is either sensible or insensible to both at the same time. The eye which perceives blue perceives also an orange color. The eye which does not discern red is incapable also of distinguishing green; these two colors it confounds. The above conclusions comprise in themselves all the phenomena of daltonism. They are, moreover, a necessary consequence of the theories of MM. Melloni and Plateau, and might be enunciated *a priori*, even previous to their verification as a consequence of facts. But these conclusions being once admitted, it becomes evident that the cause and explanation of daltonism is to be sought in an anomalous insensibility of the eye, which deprives it of the faculty of vibrating in unison with such or such a luminous ray.

We shall not insist further on this point. Let us conclude by indicating the very simple means by which the elder Seebeck succeeded in rectifying to a certain extent the error committed by daltonians in the appreciation of colors. This means consists in the use of glasses, or more generally of colored mediums. Suppose, for example, that an eye confounds green with red. It will evidently suffice to furnish this eye with a red glass in order that the difference between the two colors shall be distinctly perceived. The surprise of a daltonian is inexpressible when thus enabled to discover the errors which he is constantly making. By this means, however, only mistakes relative to the nature of colors are remedied. The inability to distinguish between shades of the same tint will, in general, still subsist.

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THE FIGURES OF EQUILIBRIUM OF A LIQUID MASS WITHDRAWN FROM THE ACTION OF GRAVITY.

BY J. PLATEAU.

[Continued from page 435, Report for 1865.]

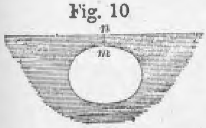
SIXTH SERIES.

Theory of liquid films. Laws of films. Constitution of froth. Generation of films. Production of films by frame-work.

§ 1. Let us prosecute the study of liquid films commenced in the second and continued in the fifth series. In § 28 of the second series I regarded the generation of films as being due to a tendency towards a new figure of equilibrium. I now abandon that opinion, at least in part; though still believing that in the case where gravity does not intervene, the system, when the films are once produced, tends towards the new state of equilibrium which I have indicated. I consider the formation of the films itself as a result of the cohesion and viscosity of the liquid.

Let us examine the matter somewhat more closely. We first take a very simple example, that of the film in shape of a spherical cap developed on the surface of a liquid by a bubble of air which has ascended from the interior of the liquid. Let us consider this bubble at the moment when it is within only a few millimetres of the surface, (Fig. 10. *) In order that its summit shall traverse the distance *m n* which intervenes, the liquid molecules situated around this little right line must necessarily be driven toward every azimuth at once, in such manner that these molecules shall undergo relative displacements. Let us suppose, for simplification, that the ascensional movement of the bubble of air is uniform, so that, in equal intervals of time, the bubble shall propel, between itself and the upper surface of the liquid, equal quantities of this liquid. Let us further suppose that the liquid has no viscosity. Then, in proportion as the distance *m n* diminishes, the portions of liquid propelled during the above intervals of time will respectively acquire velocities constantly increasing, since they must accomplish their movements in spaces more and more straitened; thus the relative displacements of the liquid molecules are so much the more rapid as the summit of the bubble is nearer attaining the surface. I have supposed the ascensional movement of this bubble to be uniform; but as it is in reality accelerated, its acceleration will contribute to increase the rapidity of the relative displacements in question.

Fig. 10



Now, we know that viscosity opposes to the relative displacements of the molecules of liquids a resistance which increases considerably with the velocity of those displacements. If, then, in order to pass to the actual case, we restore to our liquid its viscosity, the resistance to the lateral translation of the liquid molecules around *m n* will continue to augment in proportion as that right line diminishes in length, and will become very great when this line shall have become very short. Hence it necessarily results that when the summit of the bubble has arrived near the surface, the portion of liquid which still separates it therefrom cannot disperse itself with a rapidity equal to that of the ascensional movement of this summit; and, in order that the air which constitutes the bubble shall continue to ascend and pass beyond the level of the liquid, it is evidently necessary either that the liquid shall be rent asunder, or be lifted up. But it can be no longer doubted, after the ingenious researches of MM. Donny† and

* In this figure the bubble is represented a little flattened vertically, which does in fact occur by reason of the resistance of the liquid.

† *Memoires de l'Academie*, tome XVII des *Memoires des Savants Étrangers.* The memoir of M. Donny was presented to the Academy in December, 1843.

Henry,* that the cohesion of liquids is not of the same order with that of solids; it will be understood, therefore, that when the distance *m n* is so far reduced that its further diminution cannot be effected with a rapidity nearly equal to that of the ascension of the summit of the bubble, the liquid will still present in *m n* much too great a resistance to the disunion of its molecules to be forced asunder, and that hence it will be lifted up by the bubble under the shape of a film; and as this film, during its generation, is pressed from below upwards by the bubble of air, and adheres by its circumference to the liquid of the vessel, it must be convex towards the exterior. After the film has commenced forming, it must become still more developed: for, incessantly pressed by the bubble of air, it must continue to rise, while the liquid to which its circumference adheres cannot follow it in mass on account of its weight; this liquid must, therefore, remain behind; but, by virtue of the cohesion and viscosity, there can be no rupture between the incipient film and the environing liquid, and the film will simply increase until the action from below upward exerted on the lower part of the bubble of air shall have had its whole effect. Mr. Hagen,† who has sought to prove, contrary to the principle established by Poisson in his *new theory of capillary action*, that the density of the superficial stratum of liquids is greater than that of their interior, cites, in support of his opinion, the fact of the formation of the films in question; but we see that is not at all necessary to resort to such an hypothesis in order to account for this formation.

In § 25 of the first series it was said that when a mass of oil a little less dense than the alcoholic liquid in which it is immersed rises to the surface of the latter, it is at first more or less flattened against that surface, as if encountering resistance in traversing it; that after some time it makes its way through, and then presents a portion of plane surface more or less extended on a level with that of the alcoholic liquid. This phenomenon is now explicable in a natural manner from the considerations which precede: it fares with the sphere of oil as with the bubble of air; it can only make its way to the exterior by disuniting the molecules of the upper stratum of the ambient liquid, but this not growing thin with sufficient rapidity on account of its viscosity, resists a rupture by virtue of its cohesion. Only it is plain that, in this case, the pellicle cannot be elevated above the level.

§ 2. Let us recur to our convex film developed by the ascension of a bubble of air. When it has attained its full development, and hence remains stationary, it should assume (5 series, § 12) one of the figures of equilibrium which would correspond to the surface of a liquid mass without gravity; now this figure, which is formed by an equal action in all azimuths around the vertical axis of the air bubble, must evidently be one of revolution, and, as it is closed on the axis, it can only constitute (IV, § 2) a portion of a sphere. What, now, does theory teach us on the extent of this portion relatively to the complete sphere? As regards molecular action, the superficial stratum of a full liquid mass may, as we know, be assimilated to a stretched membrane; our liquid film, which is obviously reduced to the superficial strata of its two faces, may therefore be likened to a stretched membrane, and consequently has a tendency to occupy the least possible extent. The question, then, if we neglect certain particulars of which I shall presently speak, and which have no sensible influence when the volume of air is somewhat large, is reduced to this: what, for a given volume, is the segment of a sphere whose surface is smallest? This problem is readily solved by calculation, and we thus find that the segment in question is a hemisphere; but we reach the same result still more simply by the following reasoning, for the idea of which I am indebted to M. Lamarle.

Let us conceive any two spherical segments, equal as regards one another, and

* Philosophical Magazine, 1845, vol. xx, p. 541.

† *Ueber die Oberflächen der Flussigkeiten* (Ann. de M. Poggendorf, 1846, vol. LXVII, p. 1.)

applied one against the other by their bases. In order that the convex surface of each of them should be the smallest possible with reference to the volume enclosed between itself and the common base, it suffices evidently that the whole convex surface of both segments together should be the smallest possible in relation to the total volume; now, according to a known principle, this last condition will be fulfilled if the two segments constitute together a single sphere, in which case each of the two segments will be a hemisphere. Our liquid film, if it contains a sufficient body of air, ought, therefore, to take the hemispherical form, and this is what common observation verifies.

§ 3. We will advert now to the particulars to which allusion was made a little while ago. In the first place, the liquid of the vessel must be slightly raised by the capillary action on both the exterior and interior face of the laminar figure, as it would be on the two faces of a solid lamina previously moistened with the same liquid and partially immersed; it must, therefore, form a small annular mass with concave meridian surfaces, and this also is confirmed by observation. Hence the border of the liquid film does not rest immediately on the plane surface of the liquid of the vessel, but on the crest of the small annular mass in question. In the second place, it will be perceived from this, that if the enclosed volume of air is so small that the space circumscribed by the border of the film shall have little diameter, the surface of the liquid in this space will be in no part plane, but will present, even in the middle, a concave curvature more or less decided, as in the interior of a tube of small extent. This result is also in accordance with experience, and I have ascertained, by means presently to be indicated, that the central portion of the surface in question ceases to appear plane when the diameter of the film, at the crest of the little annular mass, is less than about two centimetres. In the third place, even with a volume of air great enough for the surface of the liquid, in the space circumscribed by the film, to show itself absolutely plane through nearly its whole extent, this surface must be sunk below the exterior level by the pressure which the film, by reason of its curvature, exerts on the enclosed air, (V, § 21,) and that this is the case may be evinced by the following process:

In a large porcelain dish placed on a table in front of a window pour a stratum of glyceric liquid (V, § 13) about 2 centimetres in depth, and, after having inflated, by means of an earthen pipe, a bubble of the same liquid, deposit it in the middle of the surface of the stratum, when it will at once form a spherical segment. We now place ourselves in such a position as to see the sky by reflection on the surface in question, holding, at the same time, a black thread stretched horizontally at a small distance from the film in such manner that a portion of its reflected image shall be perceived in the space circumscribed by the film. The complete image of the thread now appears to be formed of three parts—two without and one within the laminar figure; the former are both incurvated near the film, in consequence of the capillary elevation before spoken of; as regards the third, if the circumscribed surface has, in its middle, a plane portion, we shall find, by suitably adjusting the thread, a position of the latter for which the middle of the image will be rectilinear. This will be the case with films whose diameter exceeds two centimetres, but within that limit the entire part of the image in the interior of the film will appear curved.

When the film has a large diameter, that part of the image of the thread is rectilinear for nearly its whole length; it curves only toward its extremities, by virtue of the capillary attraction; but its straight portion is not in the prolongation of the straight portions exterior to the film; it will be seen a little lower. This depression, which shows that the circumscribed plane surface is, as has been said, below the exterior level, becomes less decided in proportion as the diameter of the film is more considerable—a circumstance referable to the diminution of the curvature, and consequently of the pressure of the film, but which is still very perceptible for a film of a decimetre in diameter.

§ 4. The reasoning of § 2 necessarily supposes that the film rests by its actual border upon the plane surface of the liquid in the vessel, and that the portion of that surface circumscribed by the film preserves its plane shape and its level; now, these conditions, being, as has been just seen, never all entirely fulfilled, it follows that the reasoning in question can only be considered sufficiently rigorous when the variance from the imaginary conditions on which it rests is but inconsiderable. To be more precise: If we fill with glyceric liquid, and somewhat above the edge, a large porcelain salver, previously levelled and placed on a table opposite a window, and, after having deposited thereon a bubble, station ourselves so as to see the film projected on a dark ground, and, closing one eye, keep the other at the level of the little annular mass, we shall distinguish perfectly well the two meridian lines of this little mass, as well that which looks towards the exterior of the figure, as the commencement, proceeding from the summit of the crest, of that which fronts the interior. We therefore clearly perceive this summit, and can estimate, approximately, its vertical height above the exterior plane surface. We shall thus recognize that, for large bubbles, this height scarcely exceeds 2^{mm}, and is less still for small ones. On the other hand, when the film has large dimensions, when, for example, its diameter is a decimetre, the portion of the surface of the liquid circumscribed within its interior may be regarded as exactly plane through almost its whole extent. It results, in fine, from the experiments of the preceding paragraph, that with such a film, the depression of this surface, though still quite sensible, is yet very minute. From the results of § 28 of the 5th series, it follows that if the film, assumed to be hemispherical and of the diameter of a decimetre, were, although formed of glyceric liquid, deposited on pure water, the depression in question would be but $\frac{22.6}{100} = 0^{mm}.226$; and consequently in order to obtain the value of the depression in the present case, that is, when the film is deposited on the glyceric liquid, it suffices to divide the preceding quantity by the density 1.1065 of that liquid, which reduces the depression definitively to $0^{mm}.204$. With such a volume of air and a hemispherical film, a state of things would exist therefore in close conformity with the conditions of the reasoning in question, and we may conclude that the film would, in effect, take that form, or that, at least, the deviation would be inappreciable.

But it is easy to show that, with a volume of air sufficiently small, the film will be far from constituting a hemisphere. Let us imagine, for instance, that the bubble of air is but one millimetre in diameter, and suppose, for an instant, that the film is hemispherical. Upon this hypothesis, the portion of the surface of the liquid circumscribed by the film and reckoned from the border of the latter, or, if you will, from the crest of the small annular mass, would necessarily constitute, by reason of its minute dimensions, a concave hemisphere, so that the bubble of air would continue to form an entire sphere of one millimetre in diameter. That being the case, let us remember that the pressure exerted by a spherical film in virtue of its curvature is (5th series, § 23) the sum of the actions separately due to the curvatures of each of its two faces; or, since these two actions are equal, the double of one of them. Now, the action of the interior face of our little hemispherical film would, as regards its effort to cause the bubble of air to descend, be counterbalanced by the opposite action of the concave hemisphere, which would, as I have said, limit the bubble beneath, and there would remain, on the one hand, the action due to the exterior face of the film, an action which would impel the bubble from above downward, and, on the other hand, a slight hydrostatic pressure which would impel this bubble from below upward if the lower point of it were below the level of the liquid. But, in the case of the glyceric liquid, it further follows, from the results of § 28 of the 5th series, by taking, agreeably to the remark above made, half the value which they yield, and dividing by the density of the liquid, that the first of the above two actions would be equivalent to a difference of level of $10^{mm}.21$; while, even supposing the absence of the little annular mass, the second would evidently proceed only from a difference of level equal to the radius of the air bubble—that is, to $0^{mm}.5$. With our small volume of air and a hemispherical film, equilibrium, then, is impossible; that it should exist, it would be necessary that the bubble of air should remain almost entirely beneath the level of the liquid, and hence should give rise to a film scarcely at all elevated and of very feeble curvature; then, in effect, the slight hydrostatic pressure which tends to cause the bubble of air to rise will be equivalent to the minute weight of a volume of liquid a little less than that of this bubble, and the light pressure exerted by the exterior face of the film, in virtue of its feeble curvature, will suffice to counterbalance it.

Experiment again fully verifies this deduction of theory. Having poured, to a certain height, glyceric liquid into the vessel with plane glass walls which served for experiments on the masses of oil, and slightly agitated the liquid in order to produce small bubbles of air, I chose one of these about 1^{mm} in diameter, and sufficiently near to one of the walls, and observed it by successively placing the eye a little below and then above the level of the liquid. In this way I perceived that the little bubble appeared spherical, and was so far immersed that its projection above the level was very inconsiderable.

§ 5. From this it is clear that if we form successive films on the surface of soap-water or glyceric liquid, beginning with a diameter of one decimetre, followed by others progressively smaller, a limit will be reached below which the films will exhibit a sensible depression, or appear, in other words, to constitute less than a hemisphere. In order to determine this limit approximately in regard to the glyceric liquid, I deposited the bubbles, as was indicated in the preceding paragraph, on the surface of the liquid contained in a salver a little more than full, and ascertained that they appear hemispherical only for diameters greater than about 3 centimetres; below that value the bubbles form segments sensibly less relatively to the entire sphere, and this diminution is the more decided as the diameter of their base is smaller.

§ 6. Although a film of spherical curvature thus formed at the surface of a liquid be in equilibrium of figure, still absolute repose does not exist: it slowly becomes thinner until it bursts. The principal causes of this have been long since indicated: they are, firstly, evaporation, in the case of liquids which are susceptible of it; and secondly, the action of gravity which causes the liquid constantly to descend from the summit of the film towards its base. And here again viscosity has a great influence: if this be very weak, it is plain that the gliding of the molecules towards the base of the film will be effected with great rapidity, and consequently the film will have scarcely any persistence; hence, when we succeed in forming films with pure water, they scarcely subsist at all. This remark concerning the agency of viscosity in the duration of films had already been presented, though in a somewhat different manner, by Professor Henry,* in regard to bubbles of soap compared with those of pure water.

§ 7. Let us suppose now that a second bubble of air rises from the bottom of the vessel, and that at the moment when it has nearly reached the surface, it happens to be partly under the first film; it will thus occasion the formation of a film which will necessarily lift up the former on one side, so that the two quantities of air respectively imprisoned by these two films will be separated by a portion of the second, as by a liquid partition. But this partition will not observe the curvature of the rest of the second film, as I shall proceed to show.

In virtue of their liquid nature, films can evidently not meet under angles with linear edges: for continuity it is necessary that, along the whole line of junc-

* See the article cited in 3d note of section 1.

tion, a small mass with surfaces strongly concave in a perpendicular direction to this line should take form; it is what is realized, as has been seen (2d series, §§ 31 and 32) when, in the interior of the alcoholic liquid, a laminar figure of oil is produced by the gradual exhaustion of a polyhedron. Let us recall, in this respect, the experiment of § 2 of the 5th series—an experiment in which a similar mass, though a thick one, establishes the transition between a plane film and two curved films, as is seen in meridian section in Fig. 1 of this 5th series. It will be understood, therefore, that in the case of our films of soap-water or of glyceric liquid, a mass of this kind exists, though too minute to be distinguished, along the whole length of the arc of junction of the partition and the two other films; now, the surfaces of the latter and that of the partition being thus united by small surfaces having their own curvatures, it is plain that these small surfaces establish an entire independence between the respective curvatures of the other surfaces. It is thus, for example, that in the experiment above recalled, a plane film is connected with two films which are portions of catenoids. In this experiment, it is true, the junction takes place by a thick mass; but the result must evidently be the same as regards the independence of the curvatures, however minute be the transverse dimensions of the mass serving as an intermedium.

This being the case, let us remark that the partition must also constitute a portion of a sphere, for it falls within the same conditions as the two other films; that is to say, it has, like the latter, for limits the small mass of junction and the water of the vessel. As regards its curvature, this evidently depends on the difference of the action exerted on its two faces by the two portions of imprisoned air. If these two portions of air are equal, the two films will pertain to equal spheres, which will press the two volumes of air with the same intensity, and consequently the partition, exposed on its two faces to equal actions, will have no curvature, or, in other words, will be plane; but if the two quantities of air are unequal, in which case the two films will pertain to spheres of different diameters, and will therefore press these two quantities of air unequally, the partition subjected on its two faces to unequal actions will acquire convexity on the side where the elasticity of the air is least, until the effort which it exerts, in virtue of its curvature, on the side of its concave face, counterbalances the excess of elasticity of the air which is in contact with that face.

Let ρ, ρ', and r be the radii of the spheres to which respectively appertain the larger film, the smaller and the partition, and let p, p', and q be the respective pressures which they exert, in virtue of their curvatures, on the air which bathes their concave faces. These pressures being (5th series, §§ 22 and 28) in the inverse ratio of the diameters, and consequently of the radii, we shall have $\frac{p}{q} = \frac{r}{\rho}$, and $\frac{p'}{q} = \frac{r}{\rho'}$; but, according to what has been seen above, it is necessary, for equilibrium, that we should have $q = p' - p$; whence, $1 = \frac{p'}{q} - \frac{p}{q}$. Transferring to this last equation the above values of $\frac{p'}{q}$ and of $\frac{p}{q}$, and resolving by reference to r, there results $r = \frac{\rho\rho'}{\rho - \rho'}$, a formula which gives the radius of the partition when we know those of the two films. If, for example, these two films pertain to equal spheres, we have $\rho = \rho'$, and the formula gives $r =$ infinity; that is to say, the partition is then plane, as we have already found it to be. If the radius of the smaller of the two films is half that of the larger, in other terms, if we have $\rho' = \frac{1}{2}\rho$, the formula gives $r = \rho$; in this case, consequently, the curvature of the partition will be equal to that of the larger film.

§ 8. In order to complete the study of our laminar system it remains only to inquire under what angles the two films and the partition intersect one another.

With this view, let us remark that the small mass of the junction which prevails along the entire common edge of these angles, and which was spoken of in the preceding paragraph, must of itself have its equilibrium of figure. Now, as it has three surfaces it is necessary that the curvatures of these should have to one another a ratio which permits this equilibrium. The small mass has, longitudinally, curvatures which are not of the same direction for its three surfaces, but in consequence of its extreme tenuity in thickness, its transverse curvatures are enormous, relatively to the longitudinal curvatures in question, so that the influence of the latter may be overlooked in view of that of the former. On the other hand, the two small surfaces which face the interior of the system are pressed upon by the respective elasticities of the two portions of imprisoned air, elasticities which are generally unequal, and which exceed the atmospheric pressure, while the small surface which faces the exterior is only subjected to this latter pressure; but as the differences between these three actions of the air result from the curvatures of the two films, curvatures which are extremely feeble compared with the transverse curvatures of the small mass, the influence of these differences may also be overlooked by the side of that of the transverse curvatures in question. Hence it is evidently requisite, for the equilibrium of the small mass, and consequently for that of the whole system, that if we conceive this small mass cut by a plane perpendicular to its axis, the three concave arcs which will limit the section shall be closely identical. Now, from this near identity it necessarily results that the two films and the partition terminate at the small mass under angles either strictly equal, or very nearly so—angles, consequently, each of 120°, or which will differ from this value by an unappreciable quantity.* We shall presently see this result and those of the preceding paragraph verified by experiment.

§ 9. If the bubble of air which gives rise to the second film (§ 7) arrives at the surface of the liquid sufficiently far from the first film for the spherical caps to be complete and isolated, or, what amounts to the same thing, if we deposit on the liquid two bubbles at a suitable distance from each other, the two liquid caps, drawn by capillary action in the same way with light floating bodies, will by degrees approach until they touch one another. To understand what would then happen, let us recall a fact manifested by full spheres of oil in the interior of my alcoholic mixture. When two such spheres have come in contact, the system which they form is not in a state of equilibrium; the two spheres unite to form but a single one, and the reason of this is easily apprehended. This contact cannot take place at one sole point; it must necessarily occur throughout a small surface, so that the two masses constitute in reality only one; but since this is finite and entirely free, and is a system of revolution, the sole figure of equilibrium that it can assume (4th series, §§ 2 and 38) is that of a single sphere. Now, it is visible that the same thing must have a tendency to occur in regard to our two spherical laminar caps when the two small annular masses which exist along their bases (§ 3) have united at the place of their contact; that is to say, the two caps will tend to form but a single one; but in order that this tendency may have its full effect, it would be necessary that the two liquid films should open at the place of contact; and as cohesion resists this, we perceive that the opening will be replaced by a partition, and that the same coordination will obtain here as in the system of the two preceding paragraphs. These results, also, will be verified by experiment.

§ 10. We have seen (§ 7) that the radius r of the partition is determined, when we know the radii ρ and ρ' of the two films, by considering the relative value of the pressures respectively exerted by these three portions of spherical caps on the two quantities of included air. On the other hand, the considera-

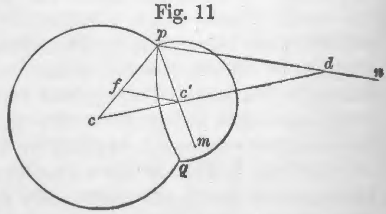
* By considering liquid films as stretched membranes, (§ 2) we should equally arrive at the equality of the angles between three films which join one another by the same liquid edge.

tion of the conditions of equilibrium of the small mass of junction has led to this consequence, that the two films and the partition must intersect one another under angles of exactly or very near 120°; and it is evident that this necessity of intersecting each other under angles of 120° may equally serve to determine the radius of the partition. Now, no relation between the two principles which serve as the bases of these two determinations is to be seen *a priori*, and it may be asked whether the two results coincide; this I propose to examine.

In order to avoid the complications which would arise from the small irregularities mentioned in § 3, I will suppose two films forming originally two complete spheres, spheres which have afterwards partially penetrated each other so as to give rise to a partition, and shall imagine this whole system intersected by a plane passing by the centre of the two films: it is clear that the centre of the sphere to which the partition pertains will be found on the right line which contains the two above centres.

This being premised, it is plain that if the angles under which the two films and the partition meet are of 120°, the radii of the two films brought to a point of the line of intersection of the latter will form between them an angle of 60°, and it will be readily seen that the radius of the partition brought to the same point will also form an angle of 60° with that one of the two others to which it is nearest. Let p (Fig. 11) be one of the two points at which terminate the three arcs, along which the two films and the partition are cut by the plane in question, a plane which we shall take for that of the figure, and let $pc = \rho$ be the radius of the larger film. Draw the indefinite lines pm and pn in such manner that the angles cpm and mpn shall be each of 60°. On pm let us take pc' equal to ρ'—that is to say, to the radius of the smallest film; let us join cc' and prolong the right line till it meets, at d, with pn. The three points, c, c', and d, will evidently be the three centres, and pd will be the radius r of the partition, so that if from these three centres and with these radii we trace three portions of circumferences, terminating on the one hand at the point p, and on the other at its symmetrical q, we shall have, as the figure shows, and still on the hypothesis of angles of 120°, the section of the system of the two films and of the partition. Let us seek now to determine the radius of this partition in a function of the two others. For this, take $pf = pc'$ and join $c'f$, the angle cpc' being 60°, the triangle fpc' will be equilateral, and we shall consequently have $fc' = pc' = \rho'$; for the same reason, the angle $fc'p$ will be 60°, like the angle $c'pd$, whence it follows that the right lines fc' and pd will be parallel; we may therefore assume $\frac{pd}{fc'} = \frac{pc}{fc}$; by then substituting, in this formula, for pd, fc', and pc their respective values r, ρ' and ρ, and observing that fc is equal to $\rho - \rho'$, we shall deduce $r = \frac{\rho\rho'}{\rho - \rho'}$; being identically the value given by the first method, (§ 7;) thus two laws, apparently independent, conduct to the same result.

Fig. 11



It does not follow, however, that the three angles are strictly equal, for what we have just demonstrated has no necessary converse; in other words, the radius of the partition may have the above value in the case of angles differing from one another; but since these angles, if not identical, must approach exceedingly near to identity, as I have shown, and since, on the other hand, their rigorous equality implies the theoretic value of the radius of the partition, it must be regarded as highly probable that this perfect equality exists.

§ 11. The existence of partitions is a fact well known to all who have amused themselves with making soap bubbles; but it was necessary to submit to the control of experiment the results established in what precedes, from § 7 onward, and first those which relate to the curvature of the partition and the angles under which that partition and the two films intersect each other.

With that view I traced on three sheets of paper three figures, representing the bases of three systems, each formed of two portions of laminar spheres and of a partition. I understand by the base of such a system the assemblage of arcs of a circle by which it rests on the surface of the liquid, abstraction being made of the small annular masses. The three outlines are reproduced, at a third of their size, by figures 12, 13, and 14; they were constructed upon the same

Fig. 12

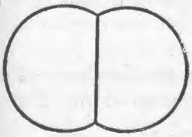


Fig. 13

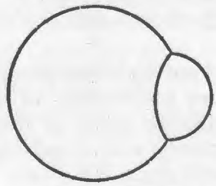
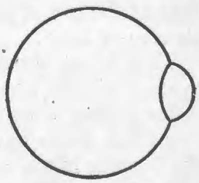


Fig. 14



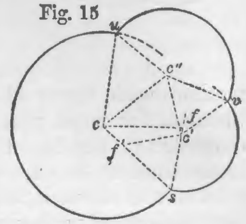
method with figure 11, the arcs being marked in lines about a millimetre in breadth, for what reason will presently be seen. In the first drawing the diameters of the two films were equal, in the second they were as 2 to 1, and in the third as 3 to 1. To make use of one of these outlines, it was placed on a table, and above it was laid a plate of thin glass, the upper face of which was then moistened with glyceric liquid. This being done, a bubble of the same liquid was inflated and was deposited on the glass plate above the portion of circumference which represented the base of the smallest film; this bubble immediately spread so as to form a hemisphere, the base of which had, designedly, a diameter a little less than the portion of circumference in question. A second bubble was then inflated, and in like manner deposited above the portion of circumference representing the base of the other film, the precaution being again taken that after its development on the glass it should have a diameter somewhat smaller than that circumference. As, in depositing this second film, it was placed in contact with the first, the two hemispheres penetrated one another partially, and remained united with a partition. Matters being thus prepared, the orifice of the pipe was dipped in glyceric liquid, as if to form a bubble, and this orifice being then applied toward the inferior part of the smaller film, a little blowing was practiced; the same operation was next performed for the other film, then again for the first, then for the second, and so on, the glass plate being at the same time made to slide, by small quantities, on the drawing, and, with suitable care, it was contrived to give to the two films the diameters of the portions of circumference as traced, and now the base of the laminar systêm obtained, a base formed of those of the two films and that of the partition, exactly covered the drawing. It has been said that the drawings were in broad lines, and this was done because these lines were to be observed through the small annular masses; had the lines been fine, the refractions produced by these masses would have prevented them from being distinguished.

§ 12. In order to verify also the results of § 9, I deposited successively on the surface of the glyceric liquid, contained in the large porcelain plate, (§ 3,) two bubbles of this same liquid, in such manner that the two spherical caps which they formed might be separated by a certain interval. When this was about a centimetre, the films moved indeed toward one another, and united with a partition; but if the two films had large diameters, (10 centimetres or more,) the partition was not in general produced unless the union took place but a few moments after the formation of the films; when these were at first somewhat too distant from each other, so that the time required for their spontaneous congress was rather long, they united without a partition, becoming transformed into one large hemisphere, because doubtless they had grown too thin, and the incipient partition had broken before its existence could be verified.

§ 13. If a third spherical laminar cap joins itself to two others already united, the system will evidently have three partitions, namely, one proceeding from the union of the first two films, and two from the union of each of these films with the third. These three partitions will necessarily terminate at the same arc of junction, and, supposing that they still have spherical curvatures, it will result that at three lines of junction of each of them with two of the films the angles will still be of 120°; it will result, moreover, for reasons already given, (§ 8,) that at the arc of junction of the three partitions with each other the angles will be also of 120°.

This being premised, let us see by what means we can trace the base of a system of this kind, as we have traced (Fig. 11) that of a system of two films. After having described (Fig. 15) the bases of the first two films, bases having for centres c and c', and for radii the lengths given which we will again designate by ρ and ρ', let us take, commencing at the point s, where these two bases meet, and on the radii sc and sc', two lengths sf' and sf, equal to one another and to the radius ρ'' of the third base, then from the points c and c' as centres, and with the lengths cf and $c'f'$ as radii, let us trace two arcs of a circle; their point of intersection on c'' will be the centre of the base of the third film, a base which we will then describe with the radius ρ''. Let us, in effect, suppose the problem solved, and this base traced. If we draw from the point u where it terminates at one of the former the right lines uc and uc'', which will be respectively equal to ρ and ρ'', these lines will make between them an angle of 60°, like the right lines sc and sc'; whence it follows that the triangle cuc'' will be equal to the triangle csf, in which sc and sf are also respectively equal to ρ and ρ'', and thus cc'' will be equal to cf; for the same reasons the triangle $c'vc''$ will be equal to the triangle $c'sf'$, and consequently $c'c''$ will be equal to $c'f'$. Let us propose now to trace the bases of the three partitions. Those of the three films being described (Fig. 17) after the preceding outline we determine, as in Fig. 11, the centre d of the partition pertaining to the first two films, and commencing from s, by drawing sd making with sc' an angle of 60°, until it meets, at d, with the line cc' prolonged; we determine likewise the centre f of the partition pertaining to the first and the third film by drawing uf, making an angle of 60° with $c''u$, until it meets, at f, with cc'' prolonged; finally, we determine by the same process the centre g of the third partition. There remains then only to describe from the points d, f, and g, as centres, and with the radii ds, fu, and gv, three arcs of a circle beginning, respectively, at the points s, u, and v, and directed toward the middle of the figure; these arcs will be the bases of the three partitions, on the hypothesis, however, that these partitions are portions of spheres. If the figure has been constructed with care, we shall recognize, 1st, that the three arcs just spoken of all terminate at the same point o; 2d, that the three centres, f, d, and g, are disposed in a right line; 3d, that if we join the point o to these three centres, the angles fod and god are equal, and each of 60°.

Fig. 15



§ 14. But as it might be thought that these results of a graphic construction are merely very close and not strictly exact, I proceed to establish them absolutely, assuming, however, that the angles of the films with one another and with the partitions are precisely of 120°. I am indebted for this demonstration to M. Vander Mensbrugghe, a young doctor of sciences of our university.

Let us first demonstrate that the three centres f, d, and g, are in a right line. For this purpose, repeating what has been done in Fig. 11, let us take on $s\,c$ a length $s\,w = s\,c' = \rho'$, and join $c'\,w$; we know that this last line will be parallel to $s\,d$, and consequently we may assume $\frac{dc}{dc'} = \frac{sc}{sw} = \frac{\rho}{\rho'}$. For the same reason, in considering the two portions of circumferences which have for centres c and c'', and which intersect each other at u, we shall have, by simply reversing the two ratios, $\frac{fc''}{fc} = \frac{\rho''}{\rho}$; and the two portions of circumferences having for centres c' and c'', and which intersect at v, will also give $\frac{gc'}{gc''} = \frac{\rho'}{\rho''}$. Multiplying these three equalities, member by member, there results $\frac{dc \cdot fc'' . gc'}{dc' fc\ gc''} = 1$.

Let it be remarked now, 1st, that the three centres, d, f, and g, are on the prolongations of the sides of the triangle $c\,c'\,c''$; 2d, that the six quantities, $d\,c$, $f\,c''$, $g\,c'$, $d\,c'$, $f\,c$, $g\,c''$ are the distances, reckoned on these same prolongations, from the three points d, f, and g, to the three summits c, c', and c; 3d, that, in the last of the above formulas, the three factors of the numerator represent right lines, no two of which have a common extremity, and that the same holds with regard to the three factors of the denominator. Now we know, by a theorem of the theory of transversals, that when the condition expressed by this formula is fulfilled in regard to any triangle, the three points in question, taken on the prolongations of the three sides, are necessarily in a right line. Our three centres d, f, and g partake, therefore, of this property.

This first point established, let us demonstrate the others. Let us consider the point o as being simply the intersection of the two arcs $u\ o$ and $v\ o$, having for centres f and g; let us join $o\ d$, $o\,f$, and $o\ g$, and without inquiring at first whether $o\ d$ is really the radius of the arc having d for its centre, and proceeding from the point s, let us show that the angles $f\ o\ d$ and $g\ o\ d$ are each of 60°, or what amounts to the same, that the angle $f\,o\ g$ is of 120°, and that the right line $o\ d$ is its bisector.

Let us seek first to determine the lengths $f\,d$ and $g\ d$, and, to this end, let us consider them as pertaining, respectively, to the triangles $f\,c\ d$ and $g\ c'\ d$, in which we can calculate the sides $f\ c$, $d\ c$, $g\ c'$, and $d\ c'$, as well as the angles which they comprise. To arrive at these last values, let us calculate the sides of the triangle $c\ c'\ c''$. By means of the triangle $c\ s\ c$, in which the sides $c\ s$ and $c'\ s$ are, respectively, ρ and ρ', and comprise between them an angle of 60°, we find without difficulty $c\ c' = \sqrt{\rho^2+\rho'^2-\rho\rho'}$; the triangles $c\ u\ c''$ and $c'\ v\ c''$ likewise give $c\ c'' = \sqrt{\rho^2+\rho''^2-\rho\rho''}$, and $c'\ c'' = \sqrt{\rho'^2+\rho''^2-\rho'\rho''}$. Whence, by the known formula, we deduce

$$\cos c'\,cc'' = \cos dcf = \frac{\rho^2+(\rho-\rho')(\rho-\rho'')}{2\sqrt{\rho^2+\rho'^2-\rho\rho'}\sqrt{\rho^2+\rho''^2-\rho\rho''}};$$

in the same way we find

$$\cos cc'\,c'' = \cos gc'\,d = \frac{\rho'^2+(\rho-\rho')(\rho''-\rho'}{2\sqrt{\rho^2+\rho'^2-\rho\rho'}\sqrt{\rho'^2+\rho''^2-\rho'\rho''}}.$$

On another side we have

$$fd = \sqrt{\overline{fc}^2+\overline{dc}^2-2\overline{dc}.\overline{fc}\cos dcf} \text{ and } gd = \sqrt{\overline{dc'}^2+\overline{gc'}^2-2dc'.gc'.\cos dc'g}$$

formulas in which the lines dc, fc, dc', gc' still remain to be determined; but in the triangle $c\ s\ d$, in which we know that $w\,c'$ is parallel to $s\ d$, we have

$$\frac{dc}{cs} = \frac{dc'}{sw} = \frac{cc'}{cw};$$

whence, by replacing *c s*, *s w* and *c w* by their values ρ, ρ' and $\rho-\rho'$, as well as cc' by its value found above, we obtain

$$dc=\frac{\rho}{\rho-\rho'}\sqrt{\rho^2+\rho'^2-\rho\rho'} \text{ and } dc'=\frac{\rho'}{\rho-\rho'}\sqrt{\rho^2+\rho'^2-\rho\rho'}\ ;$$

the triangles *c u f* and c'' *v g*, on their part, will give

$$fc=\frac{\rho}{\rho-\rho''}\sqrt{\rho^2+\rho''^2-\rho\rho''} \text{ and } gc'=\frac{\rho'}{\rho''-\rho'}\sqrt{\rho''^2+\rho'^2-\rho''\rho'}.$$

There results, then, after substitutions and reductions, and by making, for the sake of abbreviation,

$$\sqrt{\rho^2\rho'^2+\rho^2\rho''^2+\rho'^2\rho''^2-\rho^2\rho'\rho''-\rho\rho'^2\rho''-\rho\rho'\rho''^2}=\mathrm{P},$$

$$fd=\frac{\rho}{(\rho-\rho')(\rho-\rho'')}\ \mathrm{P},$$

$$gd=\frac{\rho'}{(\rho-\rho')(\rho''-\rho')}.\mathrm{P},$$

and consequently

$$fg=\frac{\rho''}{(\rho-\rho'')(\rho''-\rho')}.\mathrm{P},$$

Hence we deduce

$$\frac{fd}{gd}=\frac{\rho(\rho''-\rho')}{\rho'(\rho-\rho'')}$$

On the other hand, according to the result of § 7, by observing that *f o* and *g o* are respectively equal to the radii *f u* and *g v* of the two partitions which we are considering, we have

$$fo=\frac{\rho\rho''}{\rho-\rho''},\ go=\frac{\rho''\rho'}{\rho''-\rho'}, \text{ whence } \frac{fo}{go}=\frac{\rho(\rho''-\rho')}{\rho'(\rho-\rho'')};$$

the two ratios $\frac{fd}{gd}$ and $\frac{fo}{go}$ are therefore equal, and consequently the right line *d o* is the bisector of the angle *f o g*.

Knowing, from what precedes, the three sides of the triangle *f o g*, we thence deduce, after all reductions are made, $\cos fog=-\frac{1}{2}$; whence it results that the angle *f o g* is of 120°, and consequently that the angles *f o d* and *g o d* are each of 60°.

Let us seek lastly the length of the bisector *d o*. With this view it is to be remarked that in every triangle of which one of the angles is 120°, there is a very simple relation between the bisector of that angle and the two sides which comprise it. Let *a b c* (Fig. 16) be a triangle in which the angle at *a* is 120°; let the side *b a* be prolonged by a quantity *a d* equal to *a c*, and join *d c*; this line will be parallel to the bisector *a h*, for the angle *d a c* will be 60°; and since *a d* is equal to *a c*, the triangle *d a c* will be equilateral, and the angle *d c a* will be 60° like the angle *c a h*; we shall have therefore $\frac{dc}{ah}=\frac{ba}{ba+ad}$, where because $dc=ad=ac$, $\frac{ah}{ac}=\frac{ba}{ba+ac}$, and

Fig. 16

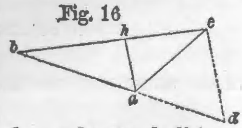
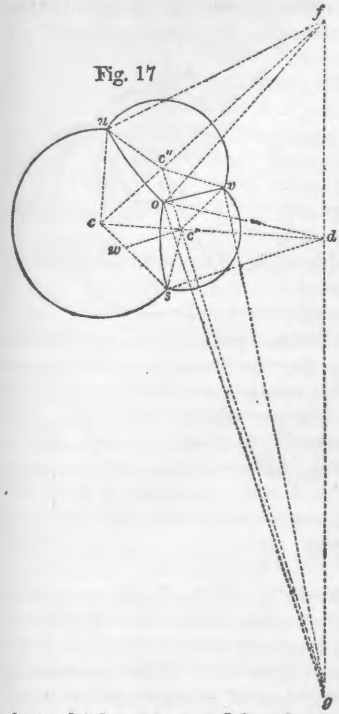


Fig. 17



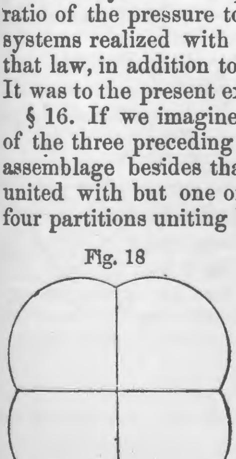
$ah = \frac{ba.ac}{ba+ac}$. This result applied to the triangle fog (Fig. 17) gives therefore $do = \frac{fo\,go}{fo+go}$, and, after substitutions, $do = \frac{\rho\rho'}{\rho-\rho'}$. Now, this is precisely (§7) the value of the radius $d\,s$ of the third partition. It results, then, from the above, as we had proposed to demonstrate, that the centres of the three partitions are in a right line; that the bases of these partitions meet at one and the same point; finally that the radii of these bases, commencing from the point in question, make with one another angles of 60°, and that hence these bases unite under angles of 120°.

Now, since we can consider the three partitions as really constituting portions of spheres, it remains to prove that these three portions intersect one another by a single arc; but this is what evidently follows from the centres f, d, g of these three spheres being in a right line, and from these same spheres having a common point at o. Thus all the theoretic conditions are satisfied by three partitions of spherical curvature disposed, with the three caps, so as to form a system having for its base that which was traced by the construction of the preceding paragraph; it must then be regarded as extremely probable that the system will really take that form.

§ 15. This, in effect, I have experimentally verified by the means heretofore described, (§ 11,) by tracing, namely, in broad lines on paper, in the manner indicated, the base of a system of three spherical caps with their partitions. The base of the laminar system thus realized has been found to be exactly superposed on the drawing. The graphic constructions given in preceding pages for the bases of systems of spherical caps implicitly suppose the law of the inverse ratio of the pressure to the diameter; the exact coincidence of the bases of the systems realized with the bases traced furnishes therefore a new verification of that law, in addition to the direct verification obtained in § 28 of the fifth series. It was to the present experiments that I had allusion in the paragraph just cited.

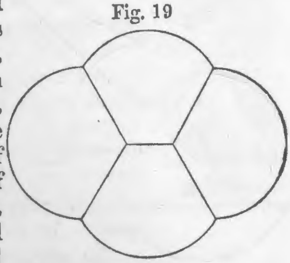
§ 16. If we imagine that a fourth spherical cap unites itself with the system of the three preceding ones, we can conceive two different arrangements of the assemblage besides that in which the fourth cap should so place itself as to be united with but one of the others. One of these arrangements would contain four partitions uniting by a single edge, and the other would contain five uniting by two edges. To simplify the question and the graphic constructions, I will suppose the four caps to be equal in diameter, in which case all the partitions will evidently be plane. Then, it may be conceived, in the first place, that the four caps unite in such way that their centres shall be placed like the four summits of a square, which will give the system whose base is represented by Fig. 18, where there are four partitions terminating at the same edge under right angles; this system is evidently one of equilibrium, since everything in it is symmetrical. It may be conceived, in the second place, that,

Fig. 18



three caps being first united, the fourth unites itself with two of them; in this arrangement, the four centres will be at the summits of a lozenge, and we shall have the system whose base is represented by Fig. 19, where there are five partitions. This system also is evidently, by reason of its symmetry, a system of equilibrium; but here not more than three partitions terminate at the same liquid edge, forming between them angles of 120°. Now, if we attempt to realize on the glass plate the first of these two systems, we shall either not succeed, or, if produced at all, its duration will be inappreciable, and it passes rapidly into the second. The second system is obtained directly without difficulty, and persists. Hence we may conclude that in the former system the equilibrium is unstable, and it thus becomes probable that four partitions terminating at the same edge cannot coexist. We may further remark that, in the laminar assemblage of Fig. 11, the semicircular liquid edge which unites the two spherical caps is only common to three films, namely, to these two caps and to the partition; and these three films, we know, form between them equal angles. In the assemblage of fig. 17, likewise, each of the liquid edges which unite the caps two by two is like that which unites the three partitions, only common to three films forming between them equal angles; again, this is evidently the case in the assemblage of Fig. 19.

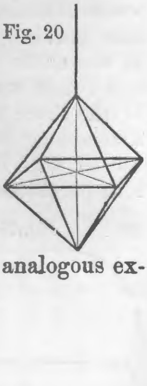
Fig. 19



Let us compare these facts with one of those shown by all the laminar systems which occupy my polyhedral frames of iron wire, when these are withdrawn from the saponaceous or the glyceric liquid. It has been seen (5th series, § 19) that in each of these systems there are never more than three films terminating at the same liquid edge. Here then is a general law of laminar assemblages. Moreover, it follows from considerations before stated (§ 8) that the angles under which the three films intersect one another must always be equal or differ only by inappreciable quantities, and this equality is easily verified, as we shall presently see, in all those systems which are composed of plane films.

With regard to the instability of a system in which more than three films should terminate at the same liquid edge, I will recall, as another proof, the curious phenomenon presented in the production of the laminar system of the regular octahedron, when I was experimenting with oil in the interior of the alcoholic liquid. When, as I have said, (2d series, § 35,) after having formed the full octahedron, we gradually withdraw oil from it by means of the small syringe, the eight faces grow concave equally and at once, and when the films begin to form, but are still joined by thick masses, they are all directed towards the centre of the figure, so that the system tends towards the arrangement which I here present anew, (Fig. 20,) an arrangement in which four films terminate at one liquid edge; but when the thickness of the masses of junction is diminished to a certain limit, a spontaneous change is effected, and the system takes definitively another form. I will now add that, in this latter form, no more than three films terminate at each liquid edge. In fine, the laminar system of the quadrangular pyramid offers another analogous ex-

Fig. 20



ample equally curious. I have given, in the second series, as the outline of that system, the one which I reproduce here (Fig. 21,) in which the liquid edge *a b* is common to the four films which proceed from the four oblique edges of the frame-work; but what then deceived me was that, with oil and in the interior of the alcoholic liquid, this edge preserves so considerable a thickness that it is impossible to raise it without occasioning a rupture of one of the films—a thickness which maintains the stability of the system; now when we realize the laminar system of this frame by means of the glyceric liquid, the edge in question is found to be replaced by an additional lamina, (Fig. 22,) and then each liquid edge is common to no more than three films.

Fig. 21

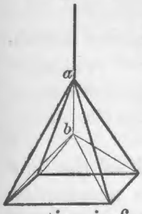
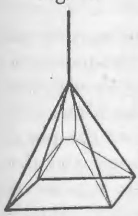


Fig. 22

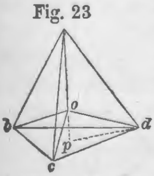


The pyramid of figures 21 and 22 has more height than that which is represented in the second series; it is because with this last the laminar system obtained by means of the glyceric liquid always presents, from its formation or shortly after, a slight irregularity in the position of the four liquid edges directed towards the summits of the base—an irregularity which is not produced with a higher pyramid. I shall recur in the sequel to this irregularity, which connects itself with an order of facts hereafter to be examined, and of which I shall then indicate the cause.

§ 17. Let us at present remark that in the systems of figures 15 and 19 there are four edges terminating at the same point, namely, the three which unite two by two the spherical caps and that which unites the three partitions. Now, as has been seen, (5th series, § 19,) the same fact presents itself in all our laminar systems obtained with the glyceric liquid: the liquid edges which terminate at the same liquid point are always four in number; here again, then, there is a general law of laminar assemblages. It may be added that if we apply to the point in question the considerations advanced (§ 8) in regard to edges, we shall conclude that these must intersect each other at this point under angles either equal or exceedingly near equality. And, in fact, the junction of the small concave surfaces pertaining to the edges produce, necessarily, around the point where they unite, four small surfaces strongly concave in all directions—surfaces whose capillary equilibrium requires equal or very nearly equal curvatures, which implies the absolute equality, or very nearly such, of the angles which we are considering.

Let us seek the value of these angles, supposing their equality to be absolutely exact. When the liquid edges are curved, it is proper to replace the arcs by their tangents at the point where they terminate, so that it suffices, in all cases, to imagine four right lines terminating at a single point under equal angles. In order, therefore, to follow the most simple method, let us consider a regular tetrahedron *a b c d*, (Fig. 23.) Let *o* be the centre of this tetrahedron; draw the lines *oa, ob, oc, od* to the four summits; these lines will evidently form with one another equal angles, whose value will consequently be that which it is proposed to obtain. This being premised, let us prolong the line *ao* to *p*, where it attains the base, and join *pd*; the triangle *o p d* will be rectangular at *p*. Let it be remarked now that the point *o* is the centre of gravity of the tetrahedron, and that the point *p* is the centre of gravity of the base *b c d*; now we know that if, in any pyramid, we join the summit to the centre of gravity of the base, the centre of gravity of the pyramid is situated on that right line, at three-quarters of its length reckoning from the summit; *op* is, therefore, the third of *oa*, and as the point *o* is at an equal distance from the four summits, *op* is also the third of *od*. In the rectangular triangle *o p d* we have consequently cos $dop = \frac{1}{3}$; whence results cos $doa = -\frac{1}{3}$. Thus,

Fig. 23



when the four right lines terminate at the same point under equal angles, each of these angles has for its cosine $-\frac{1}{3}$; we find thence that this angle is 109° 28′ 16″, that is to say, very nearly 109 degrees and a half.

Such, then, in laminar assemblages, is the value of the angles under which four liquid edges terminate at a liquid point. Each of these edges evidently unites three films, and, at least in the case of rectilinear edges, it necessarily results from the symmetry of the assemblage that these films must form between them equal angles.

Conversely, if the films which unite three by three along each of the four liquid edges form between them equal angles, these four edges also necessarily form between them equal angles. In effect, supposing the films to be plane, the system evidently constitutes an assemblage of four trihedral angles, in each of which the three dihedral angles are equal; now, in virtue of a known theorem, this equality implies, for any one of these trihedral angles, that of the plane angles; but each of these last being common to two of the trihedral angles, it follows that, in the system, all the plane angles, that is to say, the angles which the four edges form between them, are equal. If the films, and consequently also the edges, are curved, it is clear that we may, at the point common to these edges, replace the films by their tangential planes and the edges by their tangents, which will be intersections of these planes; whence it results that, in all cases, the equality of the angles under which the films terminate at their common point, and the equality of the angles under which three films unite at the same edge, are, at least in the immediate vicinity of the point in question, necessary consequences one of the other.

§ 18. Let us apply this principle to the laminar assemblage, the base of which is represented by Fig. 17. The graphic construction of this base is founded on the equality of the angles under which the films and the partitions terminate three by three at their common edges, and has been found (§ 15) to be fully verified by experiment; the four edges of the system meet then necessarily at their common point under equal angles. The principle evidently applies alike to the two points of the assemblage of Fig. 19, at which four edges terminate.

§ 19. We will recur now to the systems formed in frames of iron wire. Those of them which are composed of plane films enable us to verify this equality of the angles between the three edges which terminate at the same liquid point, for we can generally apply direct measurement; they consequently enable us also (§ 17) to verify the equality of the angles between the three films which terminate at the same liquid edge. The systems of this nature which were realized in my frames are three, namely: that of the regular tetrahedron, that of the equilateral triangular prism, and that of the regular octahedron. I here reproduce the drawings (Figs. 24, 25, and 26.) It must not be forgotten, for the

Fig. 24

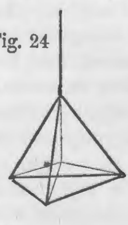


Fig. 25

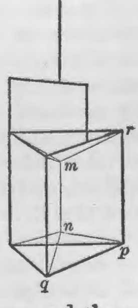
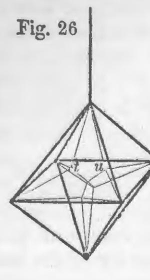


Fig. 26



understanding of these delineations and the greater part of those which follow, that, in the laminar systems which they represent, a film proceeds from each of the solid edges, directing itself towards the interior of the frame, and attached to the other films by the liquid edges which the drawings represent in fine lines. In the first the equality in question is evident, the whole being perfectly symmetrical in every direction; it will further be seen that the four liquid edges occupy precisely the positions of the right lines *oa, ob, oc, od,* of Fig. 23. In the second, as the angle between the verticle edge *mn,* and the oblique edge *np,* for example, must have $-\frac{1}{3}$ for cosine, it follows that the vertical height of the point *n,* above the plane of the base, is the third of *np,* whence it is easily deduced that, designating by *a* the edge *pq* of the base, this vertical height is equal to $\frac{a}{2\sqrt{6}}$. Accordingly, if *b* designate the lateral edge *pr* of the frame, we have $mn = b - \frac{a}{\sqrt{6}}$. In the frame which I employed, the edge *b* was $70^{mm}.70$, and the edge *a* $69^{mm}.23$;* by substituting these numbers in the above formula we find $mn = 42^{mm}.44$; now the measurement by the cathetometer gave $mn = 42^{mm}.37$, a value which differs from the preceding by only $0^{mm}.07$, that is, by less than two thousandths of the one or the other.

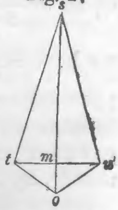
As to the system of the regular octahedron, it is composed of plane films when obtained with glyceric liquid, as I have already stated in the fifth series, while it is formed of curved films when realized with oil in the interior of the alcoholic liquid (2d series, § 35;) further on I shall indicate the cause of this difference, but at present I shall occupy myself only with the system of plane films. This system is both elegant and remarkable, consisting, as is shown by Fig. 26, of the following parts: 1. Six quadrilaterals elongated in the direction of one of their angles, having each the summit of that angle at one of those of the frame and the summit opposite to the centre of the figures; in each couple

*It is here necessary to explain in what manner these valuations have been obtained. A frame of iron wire being incapable of geometric exactness, each of the lateral edges was measured separately and the mean of the results taken; the same thing was then done in regard to the edges of the bases; but these measurements were not made directly on the edges, because their terminations were more or less masked by the soldering. The mode adopted was as follows: the frame having been placed vertically, and one of its lateral faces in front of the cathetometer, the distance comprised between the two horizontal edges was determined, by taking sight as near the vertical edge of the right as the soldering would permit, and then doing the same near the vertical edge of the left; afterwards the prism was turned on its axis, so as to present successively to the cathetometer the two other lateral faces, and the same operations were repeated on each of them; thus were obtained, for the length of the lateral edges, six valuations differing but little, the mean of which was found to be $69^{mm}.83$.

This being done, the frame was placed horizontally on suitable supports in such manner that, while one of its lateral faces still looked toward the cathetometer, the two edges of the bases which bounded it to the right and left were vertical, and the same operations were conducted as in the former case; then turning the prism on its axis, the two other faces were examined, so that, for the edges of the bases also, six values were obtained, the mean of which was $68^{mm}.36$. But these two means must undergo a slight correction; the liquid films do not in fact terminate at the solid wires according to the generating lines of which the distances have to be measured, but at other generating lines still more within the frame, whence it follows that the two above numbers are a little too small. To make the correction in a simple manner, it should be remarked that when a liquid film rests on a solid surface moistened, it is necessarily through the intervention of a small mass with concave transverse curvatures, and that, if the film be plane, it is necessary, for equilibrium, that these curvatures should be identically the same on both sides, which evidently requires that the plane of the film should be perpendicular to the solid surface. It thence results that if we prolong in thought the films of our system, each of them will pass by the axis of the wire on which it rests, and that consequently the prolonged liquid edges will alike terminate at the points where these axes intersect; we may, therefore, without altering in any manner the laminar system, and consequently the length of the liquid edge *mn*, substitute, in idea, for our frame, the assemblage of axes of the wires which compose it, which evidently amounts to the addition of the diameter of these wires to each of our two numbers; now this diameter, determined by means of the cathetometer, was found, as the mean of ten measurements taken on the different wires, equal to $0^{mm}.87$; it is by the addition of this quantity to our two numbers that we arrived finally at the values of *b* and of *a* given in the text.

terminating at two opposite summits of the frame, the planes of the two quadrilaterals are at a right angle one with the other. 2. The twelve films proceeding from the solid edges, films each of which is triangular and limited at two of the long edges of the above quadrilaterals. There is in each of these quadrilaterals a dimension easy to measure by the cathetometer: it is the distance comprised between the summits t and u of the two opposite obtuse angles. Recurring to the principle that these two angles, as well as that whose summit is at the centre of the figure, must have $-\frac{1}{3}$ as cosine, it is easily demonstrable that this distance $t\,u$ must be exactly the third of the length of the edges of the octahedron. To show this, let *stou* (Fig. 27) be one of the quadrilaterals; draw the two diagonals *tu* and *os*, which shall intersect at a right angle at m; let the distance *tu* be designated by d, the common value of the three obtuse angles by α, and the length of the edge of the octahedron by a. In the triangle *ots*, the angle at t being α, and the angle at o being $\frac{1}{2}\alpha$, the angle at s will be $180° - \frac{3}{2}\alpha$; according to this, the two rectangular triangles *omt* and *smt* will give $om = \dfrac{\frac{1}{2}d}{\text{tang}\frac{1}{2}\alpha}$, and $ms = \dfrac{\frac{1}{2}d}{\text{tang}(180°-\frac{3}{2}\alpha)}$. Let us remark now that the diagonal *os*, or the sum of the two lengths *om* and *ms*, is half the height of the octahedron, and consequently, as will be readily seen, is equal to $\dfrac{a}{\sqrt{2}}$. We shall have then $\frac{1}{2}d\left[\dfrac{1}{\text{tang}\frac{1}{2}\alpha}+\dfrac{1}{\text{tang}(180°-\frac{3}{2}\alpha)}\right]=\dfrac{a}{\sqrt{2}}$. But knowing that the cosine of the angle α is $-\frac{1}{3}$, we shall find $\text{tang}\frac{1}{2}\alpha = \sqrt{2}$, and $\text{tang}(180°-\frac{3}{2}\alpha) = \frac{1}{5}\sqrt{2}$. These values being substituted in the above formula, we obtain, as before said, $d = \dfrac{a}{3}$. In my frame, the measurements gave* for a the value $69^{mm}.49$, of which the third is $23^{mm}.16$; and for the distance d, the value $23^{mm}.14$. The difference $0^{mm}.02$ between the value calculated and the value measured, it will be seen, is even more insignificant than in the preceding case.

Fig. 27



I have said (5th series, § 19) that a solid frame being given as to its form, it might be proposed, as a geometric problem, to occupy the interior with an assemblage of surfaces subjected to the laws which govern my laminar systems; now this has been done by M. Lamarle in regard to the regular octahedron by supposing all the surfaces to be plane, and he has thus found a system identical with that which he had seen produced in my frame.

§ 20. The laminar systems of other frames—those, namely, which contain curved films, and consequently curved liquid edges—also verify, and in a very curious though somewhat less precise manner, the equality of the angles under which these edges terminate at the same liquid point; they also verify, at least in the immediate vicinity of the latter, (§ 17,) the equality of the angles between the three films which join one another at each of these same edges.

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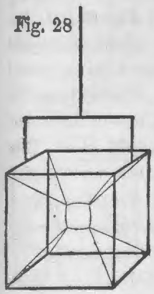
* A regular octahedral frame may be considered as formed of an assemblage of three squares of iron wire whose planes cut one another diagonally, whence the frame was so placed that these squares should present themselves successively in front of the cathetometer with two of their sides directed vertically, and, in the three positions of the frame, the distance comprised between the two horizontal wires was measured as near as possible to each of these vertical wires. Thus were obtained twelve values, whose mean was $68^{mm}.59$; moreover, eight measurements of the diameter of different wires were taken, which gave as a mean $0^{mm}.90$. For the reasons stated in the preceding note, this diameter was added to the former number, and thus was found the value of a indicated in the text. As regards that of d, since small irregularities of the frame might introduce slight differences between the six quadrilaterals, that distance was measured in each of them, and the value of d given in the text is the mean of those six measurements.

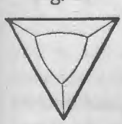
Let us take as a first example the laminar system of the cubic frame, a system whose outline is reproduced in Fig. 28.* Each of the angles of the central quadrangular lamina being 109 degrees and a half, and consequently superior to a right angle, it results that the sides of that lamina cannot be rectilinear and must constitute arcs slightly convex towards the exterior; this, in effect, is what the realized system exhibits.

Fig. 28



I have represented in Fig. 25 the laminar system of the equilateral triangular prism, and have said that, designating by a the length of the edges of the bases, the height of each of the laminar pyramids which rest on those bases is equal to $\frac{a}{2\sqrt{6}}$; but it will be understood that if the height of the prism is less than double that quantity, or, in other terms, less than 0.4 of the sides of the bases, the system in question could not be produced. In this case, analogy, with other systems, of which I shall speak hereafter, had led me to foresee that the system would be composed of an equilateral triangular film parallel to the bases, placed at equal distances from these last, and attached by other films to all the solid edges; but as the angles of this laminar triangle must be 109 degress and a half, and the angles of an equilateral triangle with rectilinear sides are but 60°, it followed necessarily that the sides of our laminar triangle were convex towards the exterior, like those of the lamina of the system of the cube, but that their curvatures were much more decided; now this was fully verified by experiment, except that the curvatures showed themselves very strong only in the vicinity of the summits. The height of my frame was about a third of the length of the sides of the bases. Fig. 29 represents this system as seen from above, which therefore constitute a second example in support of the proposition in question.

Fig. 29



In the third place, if we take for a frame that of a regular pentagonal prism whose height is not too great relatively to the dimensions of the bases, the laminar system presents, at the middle of its height, a pentagonal film parallel to the bases, and to which attach themselves, as to the triangular film of the preceding system, all the films proceeding from the solid edges. Now, the angles of two contiguous side of a regular pentagon being 108°, and thus very near our angle of 109½°, it follows that the sides of the pentagonal film must be obviously straight, and this again experiment confirms. In the frame I employed, the length of the edges of the bases is five centimetres, and the height of the prism six centimetres; the sides of the pentagonal film are about two centimetres, and the eye can there distinguish no curvature. In this system, the films which proceed from the solid edges of the bases and attach themselves to the sides of the central pentagon appear plane, as they should do, since they rest on one hand upon the rectilinear edges of the bases, and on the other upon the visibly straight sides of the central film; it thence follows that the oblique liquid edges which pass from the summits of the bases to those of the central pentagon appear straight. As to the

* In this system, the liquid edges which proceed from the summits of the frame are in reality not altogether straight; but, as was said in the fifth series with reference to this same design, the curvatures of these edges are too feeble to be shown in the engraving.

triangular films which proceed from the solid vertical edges, they are strictly plane by reason of the symmetry of their position. The system in question is represented by Fig. 30.

Fig. 30

In the fourth place, if the frame is that of a regular hexagonal prism, the laminar system is analogous to the preceding in its general disposition, the central film, however, being hexagonal; but as the angle of two contiguous sides of a regular hexagon is 120°, that is to say, considerably superior to our angle of 109½°, the sides of the film in question must be sensibly curved towards the interior, and this also is shown in the realized system. The height of the frame which I have employed is to the distance of two opposite sides of the base, or, in other terms, to the diameter of the circle which might be inscribed at that base, as 7 to 6.

§ 21. The facts I have stated (§ 16) to show that a system in which more than three films terminate under equal angles at the same liquid edge is in an unstable state of equilibrium, bear also upon the systems which present more than four edges terminating at the same time at the same liquid point. The instability might, therefore, be attributed to this latter circumstance, and it is requisite to determine whether it pertains exclusively to the one or the other, or only to their combination. In order to do so, let us take as a solid frame the assemblage of two rectangles which cut one another at a right angle in the middle of two of their opposite sides (Fig. 31.) The most simple laminar system which we can imagine in this frame would be composed of four plane films occupying, respectively, the four halves of the rectangles, and terminating at a single rectilinear edge *a b*, (Fig. 32,) which would join the two points of intersection of these rectangles. This system, by reason of its symmetry, would evidently be a system of equilibrium, and would present no liquid point common to several edges; but the edge *a b* would be common to four films. Now, when we withdraw this frame from the glyceric liquid, it is never found to be occupied by the system just indicated. In that which is realized, instead of the edge *a b*, there is (Fig. 33) a plane film terminated by two curved edges, to which the films proceeding from the solid edges attach themselves; films which, then, are necessarily curved. Here, it will be seen, each of the two liquid edges is common only to three films; and it must be inferred from this, that instability is really a property of laminar systems in which this condition is not fulfilled.

Fig. 31

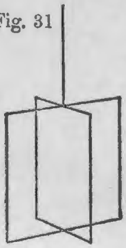
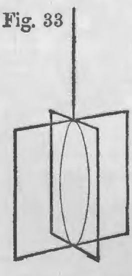


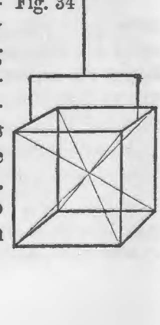
Fig. 32

Fig. 33



As to the second circumstance, I will first remark that if, in the cubic frame, we imagine a system of twelve triangular plane films proceeding, respectively, from twelve solid edges, and terminating at the centre of the frame, (Fig. 34.) this system, because of its perfect symmetry, will necessarily be a system of equilibrium, and it is readily seen that at each liquid edge only three films will termiminate, which, moreover, will form among them equal angles; but there will be eight liquid edges terminating at the central point. Now, we know that with the glyceric liquid this system is not produced, and that we always obtain that of Fig. 28, in which only four liquid edges terminate at each of the summits of the central quadrangular lamina. From this fact we may conclude that instability pertains also to every system in which one liquid point is common to more than four edges.

Fig. 34



In describing (2d series, §§ 31 and 33) the operation by means of which the laminar system of the cube is produced with oil in the alcoholic liquid, it was said that we arrive, with certain precautions, but only during the action of the small syringe, at the system of Fig. 34; and here again is curious proof in support of what precedes. In effect, the extremity of the beak of the syringe, which occupies the centre of the system, constitutes, at that centre, a solid point, and we know that this condition suffices to maintain the stability; hence, as was said in the paragraphs cited, a change takes place when the syringe is removed. If it is withdrawn slowly, we observe the development of the quadrangular additional lamina of Fig. 28, and if quickly, the rapid formation of a small central mass of a certain thickness, and the system is still maintained; but we know (§ 16) that a small thickish mass can give stability to a system which would be unstable without its presence.

We can likewise obtain, and in a permanent manner, with the glyceric liquid, the system of Fig. 34, but it is by introducing into this system also a solid part; it suffices, in effect, to stretch from one summit of the frame to the opposite summit a very fine iron wire; yet, when the frame thus arranged is withdrawn from the glyceric liquid, the system which occupies it is not immediately that in question; it still contains a quadrangular lamina, but this is much smaller than that of Fig 28, and is not placed symmetrically as regards the frame; it rests by one of its summits on the middle of the solid diagonal, but it is soon observed to diminish in extent, spontaneously, until it is completely annulled, so that the system then becomes that of Fig. 34. Here things rest, and the system rema ns perfectly stable in that state, at least when the decrease of the lamina is effected with sufficient slowness. But often this decrease is more rapid, and then another singular phenomenon presents itself. At the instant when the lamina is annulled, another much smaller is seen to be formed, situated on the opposite side of the solid diagonal, having its plane perpendicular to that of the first, and resting, no longer by a summit, but by the middle of one of its sides on the middle of the solid diagonal;* then this second lamina decreases and is annulled like the preceding. In this case, therefore, the system only attains its definitive form by a species of oscillation.

I shall indicate in another series the reason of the instability of systems in which more than three films terminate at a single liquid edge, and more than four edges at a single liquid point; but it will be seen, in the mean time, that stability must exist in the case of three films at the same liquid edge, and of four edges at the same liquid point; for three is evidently the smallest possible number of films terminating at one single edge, and there can be no difficulty in admitting that four is the smallest possible number of edges terminating at the same liquid point.

§ 22. The two laws which we have been discussing must now, I think, be regarded as well established for all laminar assemblages. Now, these laws conduct us to a very remarkable consequence. The froth which forms on certain liquids, as, for instance, on champagne wine, beer, soap-water agitated, white of egg beaten, &c., are evidently laminar assemblages, composed of a multitude of laminæ or partitions, which intersect one another and confine small portions of gas. Consequently, though everything therein seems guided by accident, they must be subject to these same laws, hence their innumerable partitions necessarily unite throughout, three by three, under equal angles, and all their edges are so distributed that there are always four terminating at the same point and forming these equal angles.

I have verified these facts by the following experiment: The bowl of a pipe

* My frame having been accidentally put out of shape and then repaired, the second lamina, when it was produced, no longer placed itself in the manner above indicated. The difference proceeded, no doubt, from a slight irregularity existing in the frame, either before or after its reparation.

held a little obliquely was plunged to the bottom of a vessel containing glyceric liquid, and by continued blowing a numerous series of bubbles of quite large size were made to traverse the liquid. This occasioned, as is done by children with soap-bubbles, the formation of a partitioned mass rising above the edge of the vessel, a mass evidently of the same constitution with froth, but of which the different parts have much larger dimensions. Now, as far as the eye could penetrate, without embarrassment, into this system, it was observed that everywhere the same edge was common to only three partitions, and that there were never more than four edges terminating at the same point. As regards the equality of the angles between these edges, there were certain places where three of those which terminated at one point seemed to be nearly in the same plane; but, on looking more attentively, it was ascertained that these edges were strongly inflected in approaching their point of concourse.

The generation of such a mass, and consequently that of froth, is easily explained. The first bubbles of gas which reach the surface of the liquid give rise to spherical caps, which form groups like those with which we have been previously occupied, and presently the whole surface of the liquid is covered with them; the films produced by the subsequent gaseous bubbles necessarily lift up this first assemblage, by occasioning the formation of partitions beneath, so that, in a short time, there are two systems of superposed films, and the gaseous bubbles continuing to arrive, this assemblage is elevated in its turn, and so in succession, the whole arranging itself with more or less symmetry, according to the differences of volume of the successive gaseous bubbles, and the distribution of the points where they attain the surface of the liquid, and the slight edifice composed of partitions imprisoning within the spaces which they separate all the volumes of gas which respectively constituted the bubbles, acquires greater and greater height. If the bubbles are very minute, the partitioned edifice will be composed of parts too small to be in general distinguished by the eye, and in that case there is froth.

§ 23. Let us return now to the laminar systems of the frames and complete their study. We will first see how the films which proceed from each of the solid edges are generated, and for this let us take a very simple example. Suppose that one of the upper rings of § 14 of the 5th series, that is to say, a horizontal ring of iron wire, sustained, like our frames, by a fork, be plunged into soap-water or glyceric liquid, and then withdrawn with due quickness, while kept always parallel with the surface; as long as the distance from the ring to the plane of the liquid is very small, the liquid will rise a little, by capillary action, presenting, at the exterior and interior of the ring, two small surfaces with concave meridian curvatures. Now, it is easy to see that in proportion as the ring continues to ascend these two small surfaces will become more and more concave in a meridian direction. We know, in effect, that when we raise a solid disk previously brought into contact by its under face with the surface of a liquid capable of moistening it, the portion of this liquid raised by the disk above the exterior level soon presents, in the meridian direction, a concavity which increases in proportion as the disk rises. The same thing, then, must also take place with regard to that one of our small surfaces which faces outwardly, and it is clear that the other small surface, that, namely, which faces the interior space of the ring, must, for the capillary equilibrium of the little mass raised by this ring, undergo analogous modifications.

Our two small surfaces will continue, therefore, mutually approaching as the ring ascends until they nearly touch one another. But they cannot thus approach without expelling a portion of the liquid comprised between them; now, if the ascension of the ring is not too slow, the viscosity and cohesion of the liquid will act here as in the case of § 1, and for the same reasons there will be formed a film, which will extend between the small portion of liquid that remained suspended along the ring and the small annular mass elevated at the surface of the liquid of the vessel. These considerations, it is clear, would apply also to the case in which the ring, while being withdrawn, is oblique or vertical instead of horizontal, and would alike apply to the case where the wire, instead of being circularly curved, should be arranged in any polygon: there would always be formed, by the same causes, a film between it and the surface of the liquid; if, then, we plunge one of our frames into soap-water or glyceric liquid, the iron wires which compose it will, in proportion as they afterwards emerge from the liquid, be connected with the latter by films, as experiment will evince.

§ 24. Let us examine, in the second place, the manner in which the films which are to constitute the system arrange themselves while being withdrawn and immediately after withdrawal. We will begin with the case of a prismatic frame which is withdrawn with the bases held horizontally. When the upper base emerges from the liquid, each of the solid edges of which it is composed will be followed, as just shown, by a film. Now, if the angle comprised between two adjacent lateral faces of the prism is equal or superior to 120°—that is to say, to one of the equal angles which three films terminating at the same liquid edge form between them, the films, which will proceed from all the edges of the base must, as we shall see, remain attached to the vertical solid edges so long as the inferior base shall not have emerged from the liquid.

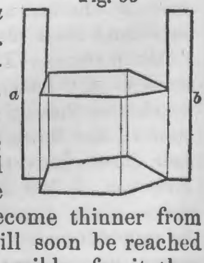
Let us take as an example the frame of a regular hexagonal prism, a prism for which the angle of two adjacent lateral faces is 120°, and consider it when it is only partly without the liquid. Suppose, for an instant, that the films which proceed from the edges of the superior base re-enter towards the interior of the frame, in which case other films will necessarily proceed from the vertical edges to terminate at the liquid edges which will unite the former. All these films will be attached to the liquid of the vessel by small masses raised along their lower borders, (§ 3,) masses which will present, in the direction of their height, strong concave curvatures. Let us, at present, direct our attention to such of these small masses only as furnish the base of the films proceeding from the edges of the base of the frame; if these masses are curved in the direction of their length, this longitudinal curvature will always, as is readily understood, be very feeble in relation to the above transverse curvatures, so that the influence of these last will much predominate; it will be necessary, therefore, for the capillary equilibrium of these small masses, that the transverse curvatures of their two surfaces should be sensibly the same, which evidently requires that the films which rest on their crests should terminate there in a direction very nearly vertical. The films in question, those, namely, which proceed from the edges of the base, must consequently be inflected in descending towards the liquid, and thus will be, in the direction of their height, convex towards the interior of the figure. But as they will be in contact by their two faces with the open atmosphere they can exert no pressure on the air, and thence it results (5th series, § 12) that their mean curvature will be null, or, in other terms, that at each of their points, the curvatures, in two rectangular directions, will be equal and opposite; therefore, since the films in question are convex in the direction of their height, they will be concave in the direction of their breadth. Now, for the two-fold reason of this concavity and of their re-entering direction towards the interior of the frame, our films will necessarily form between them, two by two, angles superior to those of the faces of the prism, and consequently superior to 120°, a thing which we know to be impossible; hence, these films must remain, as I before stated, adherent to the lateral solid edges so long as the whole frame is not out of the liquid. This deduction is fully confirmed by experiment: when the frame of a regular hexagonal prism is withdrawn, in the position indicated, from the glyceric liquid, we simply obtain, until the lower base has emerged, plane films occupying all the lateral faces.

The thickness of the wires, a thickness which, in my frames, approximates to a millimetre, might seem sufficient of itself to establish independence among these films, but this thickness is here of no importance. I have caused a hexagonal prismatic frame to be constructed in which the lateral edges were formed of mere hairs, and yet everything has taken place in exactly the same manner. In this frame, whose arrangement it would be difficult to represent by an engraving, the upper base is raised by small springs, in such a way that the hairs, which are elongated in the liquid, shall continue to be stretched.

§ 25. But it may be readily conceived that in the case of a frame of which the bases have less than six sides the incidents will be different, for then the lateral faces making between them angles of less than 120°, the films which proceed from two adjacent edges of the superior base, and which, along the corresponding vertical solid edge, would be in communication by the medium of the liquid which moistens that edge, must tend to detach themselves from this same edge, and be both directed towards the interior of the frame, to the end of re-establishing between them the angle of 120°; then, also, they will develop a third film proceeding from the vertical solid edge in question, in such manner that these films shall be united by an oblique liquid edge proceeding from the point of junction of the three solid edges. This also experiment verifies: when we withdraw from the liquid a prismatic frame with square base or the cubic frame, we see the films assume a re-entering direction as soon as the superior base has emerged, and the effect is still more decided with the frame of the equilateral triangular prism; it will be observed, at the same time, that these films conform to what I have advanced in the preceding paragraph—that is to say, they become inflected in order to terminate at the surface of the liquid in a vertical direction, and that they are concave in the direction of their breadth.

As regards the regular pentagonal prism, for which the angle of two adjacent lateral faces is 108°, and consequently little inferior to 120°, it may be conceived that the tendency of the films to re-enter must be feeble, and that hence the thickness of the wires of which the solid edges of my usual frames are formed, suffices in this case to establish independence between the films: thus, with the pentagonal frame of Fig. 30, we obtain, while withdrawing it, only plane films in the lateral faces; but I have caused to be made a frame in which the lateral edges were of very fine iron wire, and then the films re-entered; but, as might be expected, they re-entered much less than in the two preceding frames. This frame with fine lateral edges is represented by Fig. 35. The superior base is sustained by two handles *a* and *b*, by which the frame is to be held in immersing it.

Fig. 35



§ 26. Let us now see how these different systems are completed, when we continue to raise the frames. We will take, first, the case of the hexagonal prism, and suppose that the frame has the relative dimensions indicated at the end of § 20. When the inferior base rises from the liquid, a film will be formed, as with the ring of § 23, extending from that base to the surface of the liquid, a film which will continue to become thinner from above downwards. If we still raise the frame, a point will soon be reached at which the equilibrium of this film will be no longer possible, for it then spontaneously and rapidly contracts, closes in separating from the liquid of the vessel, and becomes a plane film in the inferior base of the prism. But this plane film making right angles with those which occupy the lateral faces cannot, according to what has been said in the preceding paragraph, persist in this manner; things must so shape themselves that it shall make with the lateral films angles of 120°. Now, this is effected in the most simple manner: the plane film in question ascends into the interior of the frame, diminishing at the same time in extent, as if drawing to itself the lateral films, while other films proceeding from each of the vertical wires attach themselves to these lateral ones along the liquid edges which unite them two by two, and equilibrium is established when the central film has attained half the height of the frame, because then the whole is symmetrical; we have thus the system spoken of at the end of § 20.

It will be understood that in this system the oblique films, which direct themselves towards the central film, can only form, two by two, angles of 120° on the condition of being convex, in the direction of their breadth, towards the interior of the figure, which again implies the re-entering curvature of the sides of the central film; but, by reason of the necessity of a mean curvature null, this convexity requires that the films in question should be concave in the direction of their height; in this way all the laws are satisfied, and in the system realized these two opposite curvatures are verified in the oblique films. As to the central film and the films which proceed from the lateral solid edges, these are necessarily plane, because of their symmetrical position in relation to the others. In this system it is to be remarked that the oblique liquid edges which pass to the summits of the central film do not proceed exactly from the summits of the two bases, but from points situated at a small distance from these last summits, on the lateral solid edges; of this we shall see the reason in the sequel.

§ 27. We pass now to the cases of the pentagonal prism with fine lateral edges, the quadrangular prism or cube, and the triangular prism, cases in which the films proceeding from the edges of the superior base take, as has been seen, a re-entering direction when that base emerges from the liquid. When these films begin to show themselves the small masses raised at their lower borders will necessarily describe on the surface of the liquid a polygon of the same number of sides with the solid base—that is to say, a pentagon, a quadrilateral, or a triangle, according to the frame employed. But, as the films in question are vertical at their lower part and are joined under angles of 120°, it is necessary that the sides of the above polygons should also form between them angles of 120°, which evidently requires that they be convex towards the exterior; these sides must, moreover, partake the horizontal curvature of the films, a curvature which we know to be concave towards the interior of the figure, and consequently convex towards the exterior. This convexity of the sides of our polygons will be slight for the pentagon, more decided for the quadrilateral, and still more so for the triangle. All this likewise is found to be verified by experiment.

These first facts being established, we will follow separately the development of the laminar system in each of the three frames, in proportion as it is more and more elevated.

With the pentagonal frame, the curvilinear pentagon, which takes form at the surface of the liquid, at first undergoes a slight contraction, but again enlarges when a sufficiently considerable part of the height of the frame is out of the liquid; and when the inferior base attains the surface the re-entering films attach themselves by their lower borders on the sides of that base. These films, however, do not then occupy the lateral faces of the prism; they slightly re-enter, in the direction of their height, towards the interior of the frame, so as to be united, two by two, by liquid edges, each of which constitutes an arc of feeble curvature resting by its two extremities on those of a lateral solid edge. Finally, some short time after the emergence of the inferior base, the phenomena are completed as in the frame of the preceding paragraph—that is to say, the film produced between the base in question and the liquid occupies this base under a plane form, and then ascends rapidly, drawing to itself, at the same time, the other films, so as to give, in fine, the system of Fig. 30.

With the frame of the quadrangular prism, the curvilinear quadrilateral formed at the surface of the liquid is soon annulled, and is replaced by a small horizontal liquid edge, from the extremities of which proceed two descending liquid edges, which continue to diverge from one another; these three edges limit a vertical plane film parallel to two of the faces of the prism, and attached by other films to the solid edges. No essential change takes place when the elevation of the frame is continued; the plane film just mentioned increases in height and becomes broader at its lower part, until the inferior base of the prism begins to appear at the surface of the liquid: at this time the two descending liquid edges rest by their extremities on the middle of two opposite sides of that base; after the complete emergence of which the film which occupies it is rapidly transformed into four oblique films, two trapeziums, and two triangles, which complete the system. In the particular case of the cube we have thus the system of Fig. 28. If the height of the frame be greater than the length of the sides of the bases, the oblique films which rest on these last are identically the same as for the cube, and the films proceeding from the lateral edges as well as the central plane film have simply more height.

Lastly, with the frame of the triangular prism, the curvilinear triangle at the surface of the liquid decreases more rapidly, and disappears when the frame has emerged by a very small quantity, so that the triangular pyramid which is to rest on the superior base in the definitive system is already completed; if the frame be still raised, we see a vertical and straight liquid edge extend itself from the summit of this pyramid to the surface of the liquid, an edge which is common to the three films proceeding from the lateral solid edges. This state of things continues while the elevation of the frame proceeds, the three films and the vertical liquid edge merely go on increasing in height until the emergence of the inferior base; then the film which presents itself in this base is instantaneously converted into the second triangular pyramid, which thus completes the system of Fig. 25; it should be understood, however, that the frame is supposed to have sufficient height not to give the system of Fig. 29.

§ 28. Let us take now a frame which is symmetrical around an axis passing by a summit, such as that of a regular pyramid, a regular octahedron, &c., and withdraw it by this summit. It is evident that in this case there could not be formed films occupying the faces which terminate at the summit in question, for the space which they would leave between themselves and the liquid would be void of air. It is absolutely necessary, therefore, that the films proceeding respectively from each of the solid edges should be directed towards the interior of the frame.

If there are only three solid edges meeting at the summit in question, and symmetrically disposed, as in the tetrahedron, or the cube when withdrawn by a summit, it is clear that the films proceeding from these three solid edges will be united by a single liquid edge descending vertically from the solid summit to the surface of the liquid of the vase, and this, in fact, is what takes place. With the regular tetrahedron this state of things continues until after the withdrawal of the solid base, when the system completes itself in the same manner with that of the triangular prism, and yields the result shown in Fig. 24. If there are more than three solid edges terminating at the summit which we withdraw, it follows necessarily, from the fact of the instability spoken of in §§16 and 21, that additional films should be formed. Let us take as an example the regular octahedron. It will readily be conceived that the films proceeding from the four solid edges will unite, not by a single liquid edge, but by two liquid edges proceeding from the summit and bounding a vertical auxiliary film, so that at each of these last edges terminate three films, forming between them equal angles. The auxiliary film is destined to form, in the complete laminar system, the superior quadrilateral, (Fig. 26.) Until the square, a common base of the two pyramids which constitute the octahedron, emerges from the liquid, the assemblage of films preserves the same arrangement; then, while the withdrawal of the frame proceeds, modifications are produced, which it would detain us too long to describe, and in consequence of which the system tends towards the form represented at Fig. 36, where the two faces *abc* and *a'b'c* are each occupied by a plane film. This form is completely attained at the moment when the inferior summit of the frame emerges from the liquid, but a change speedily occurs, and the system takes the form of Fig. 26. Although this change is very rapid, we may yet, by proper attention and by repetition of the experiment, observe how it is produced: the two films which occupied (Fig. 36) the faces *abc* and *a'b'c* rise towards the interior of the frame by turning around the solid edges *ab* and *a'b'*, and at the same time there is developed from the inferior summit a quadrilateral, at first very small, but which increases until its superior summit attains the centre of the frame, and which then constitutes the inferior quadrilateral of the definitive system; at the same time the summits *f* and *g* of the curvilinear quadrilateral *sfgc* ascend by a certain quantity, this quadrilateral shrinks, its edges become straight, and it finally forms the superior quadrilateral of the same definitive system. Fig. 37 represents the phenomenon in course of formation, at the moment when the quadrilateral, which is extending, has acquired half of its ultimate height. It will be easily conceived from this drawing how the four other quadrilaterals of Fig. 26 are generated.

Fig. 36

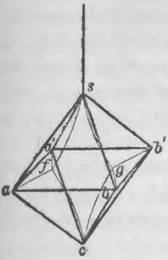
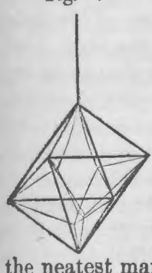


Fig. 37



In order that all these phenomena should, with almost entire certainty, be produced, it is necessary that the frame should be withdrawn quite vertically; it is further necessary that this frame should be well constructed, that the iron wires which compose it should be of the least possible thickness, and, above all, that at the summits of the octahedron they should unite in the neatest manner, at least on the side which faces the interior of the frame; when this is not the case, the laminar figures obtained are often irregular. It should be added that the frame in question sometimes gives, when it is a little inclined in being withdrawn from the liquid, a system wholly different from that of Fig. 26, regular like it, but formed of curved films. This second system contains in the middle an hexagonal film, placed parallel to two faces of the octahedron, and having its sides slightly re-entering (§ 20;) these sides are attached to the summits of the above two faces by triangular films, and to the edges of these same faces by trapezoidal films; moreover, the summits of the film in question are attached by triangular films to the other solid edges.

The different examples which I have given in detail will suffice to make it understood how laminar systems are generated, and to show that theory can render an account of all the particulars which this generation presents.

§ 29. In § 19, of the 5th series, I stated the laws which govern the laminar systems of polyhedral frames, when these systems are formed. Of these laws, which are five in number, three have already been discussed, (§§ 16 to 23:) they relate to the number of films terminating at the same liquid edge, and the equality of the angles between these films; the number of liquid edges terminating at the same liquid point, and the equality of their angles; lastly, the formation of a film proceeding from each solid edge. Let us now consider the two other laws.

One of these rests upon the fact that if care be taken that there shall be no bubble of air at the surface of the liquid of the vessel before the frame is plunged therein, the laminar system will present no space closed on all sides by films, and that hence all the films will be in contact by their two faces with the ambient air. In effect, if, while the frame is being withdrawn, the system, before it undergoes the rapid modification which gives to it its final arrangement, contained a space closed on all sides by films, this space must have originated during, and have increased in proportion to the elevation of the frame; now this is impossible, for the air which must fill it would have had no passage by which it could have entered; for the same reason the system, at this period of its generation, could present no space closed in part by films and in part by the surface of the liquid; in fine, when the rapid modification is effected, the film or films which then ascend into the system finding no space of the second kind to complete its laminar closure, the complete system will necessarily satisfy the law in question.

The last law is the following: when the conditions of other laws can be fulfilled by plane surfaces, the films take that form; when this cannot be, all the films or several of them become more or less curved, but always in such manner as to constitute surfaces of mean curvature null. The first part of this law is a matter rather of evidence than demonstration; the plane being the most simple surface, nature, which always proceeds by the least complicated methods, will not unnecessarily give curved forms to the films. The second part of the law follows from the first, and I have already made application of it in what I have previously stated respecting the laminar systems of prismatic frames.

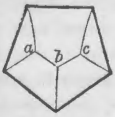
It is proper that a reason should be here given for the special fact that the laminar system of the regular octahedron is formed of curved films when it is obtained with oil in the alcoholic liquid. We have seen (§ 16) that in this mode of producing laminar systems the thickness of small remaining masses has great influence. Now, in the octahedral frame, when, by the gradual withdrawal of oil, the point has been reached at which the system is spontaneously modified, since the masses of junction have still a considerable thickness, and the oil which composes them accumulates, in the definitive system, around the points where four liquid edges would terminate, so as to form masses much thicker than the edges in question, and since, in fine, several of these edges are sufficiently short for the masses which occupy their extremities to be in communication of curvature with one another, it will be readily conceived that there must result from thence an influence on the form of the edges and of the films; and it cannot be doubted, that if we might, without occasioning the rupture of the system, sufficiently reduce the thickness of the masses in question, all the films would become plane.

§ 30. To return to the systems of the prismatic frames: Besides the facts already stated, these systems have presented others equally curious, which I here propose to notice.

The system obtained with the pentagonal frame of Fig. 30 is, as was seen, (§ 20,) composed of films obviously plane. Now, if we consider the oblique films which proceed from two homologous sides of the two bases to unite at one of the sides of the central pentagonal film, and recollect that these two oblique films must form between them an angle of 120°, it will be evidently seen that, for bases of given dimensions, an augmentation in the height of the prism involves a diminution in the extent of the central pentagonal film, and that there is a limit of height beyond which the existence of this film is impossible. It will be found, without difficuly, that the limit in question corresponds to the case in which the ratio between the height of the prism and the diameter of the circle which might be inscribed at the base would be equal to $\sqrt{3}$, that is to say, to 1.732.

It might naturally be asked, What becomes of the laminar system when this limit is overpassed? In order to know this, I had a frame constructed in which the height was about $2\frac{1}{2}$ times the diameter of the inscribed circle, and it yielded me a singular result. When it is withdrawn from the glyceric liquid, as the lateral edges are of ordinary iron wire, all the lateral faces are at first occupied by plane films, and after complete emergence, a plane film is formed also in the inferior base, then ascends among the others, forming a pentagon which continues decreasing, everything occurring as with the frame of Fig. 30; but the pentagonal film decreases much more rapidly, then disappears, and at the instant the system undergoes an abrupt change, assuming an odd arrangement which it would be difficult to represent clearly in perspective, but of which I shall still attempt to give an idea. On the two bases respectively rest two identical assemblages, composed of five curved films; one of these assemblages is represented in projection on the plane of the base by Fig. 38 It will be seen that there are in each of them a pentagonal film, two quadrangular and two triangular films. These two assemblages are connected with each other by films which proceed from the five lateral edges of the prism, and by two other intermediate films, whose direction is also in the length of the frame, and which proceed from the liquid edges *a b* and *b c* of one of these assemblages to terminate at the homologous liquid edges of the other. There is no need of remarking that the same thing would occur if, instead of overpassing the indicated limit, we should simply attain it—that is to say, if we gave to the prism the height which would precisely correspond to the annulling of the pentagonal film; in effect there would then be ten liquid edges terminating at the centre of the system, in the same liquid point, and consequently equilibrium would be unstable.

Fig. 38



§ 31. Although, in the laminar systems of the prisms with a greater number of sides, the oblique films must be considerably curved, it appeared to me probable that there would also be, for each of these prisms, a limit of height beyond which the system would no longer comprise a central polygonal film, and that this limit would differ little from that pertaining to the pentagonal prism. To ascertain this, I tried first the hexagonal prism, with a frame whose height was also about 2½ times the diameter of the circle which might be inscribed at the base. Now, to my great surprise, a central hexagonal film was still formed, though much smaller than with the frame of § 20; but the system had undergone a modification which rendered possible the existence of this film. The points of the solid lateral edges from which proceeded the oblique liquid edges (§ 26) were situated much further from the summits of the bases, so that the arrangement was very nearly such as if, in reality, the frame had been shortened. In this arrangement, therefore, the films proceeding from the sides of the two bases remain, to a sufficently great distance from the latter, adherent to the solid lateral edges, whence it follows that the assemblage should be considered as an imperfect laminar system, resulting from a conflict between the tendency of the films to occupy the lateral faces of the prism, and the kind of traction which these films undergo on the part of the hexagonal film which ascends among them. I say imperfect, because the films which proceed from all the sides of the same base are, in the parts which remain attached to the lateral solid edges, separated from one another and rendered independent by these edges.

§ 32. This becomes more evident with prisms the number of whose lateral faces exceeds six; then the angle of two adjacent lateral faces being superior to 120°, the films have more tendency to occupy all these faces, and the portions which remain attached to the lateral solid edges are, in effect, much more extended. For example, with an octagonal frame in which the ratio between the height and the diameter of the circle inscribed at the base is nearly the same as in the above frames, the central octagonal film, instead of being small, is, on the contrary, very great, and the two oblique liquid edges proceeding from any one of its summits attach themselves to the corresponding solid lateral edge, at two points, of which the distance is but about the sixth of the length of that edge, and consequently a little less than the half of the diameter of the circle inscribed at the base. In this case, therefore, the films which proceed from two homologous sides of the bases in order to direct themselves towards the central octagonal film, only abandon the solid lateral edges on approaching the middle of the height of the frame, and until then they occupy, under a perceptibly plain form, the lateral faces of the prism.

In the hexagonal frame of the preceding paragraph, the distance between the points where two liquid edges proceeding from one of the summits of the central film attach themselves to the same lateral solid edge is about double the diameter of the inscribed circle. In the octagonal frame, as we have just seen, it is somewhat smaller than the half of that diameter; in a heptagonal frame it is, as might be expected, intermediate between those two values, and equal, nearly, to three-fourths of the same diameter. I have tried, also, a decagonal frame, and in this the distance in question is but the sixth of the diameter.

The facts just recited would constitute an exception to our law, according to which one film ought to rest on each of the solid edges of the frame, since, reckoning from either base to the points at which the oblique liquid edges arise, two films are attached to each of the lateral solid edges; but, as I have shown, the laminar systems in question are imperfect systems. These facts are not owing to the thickness of the metallic threads: they still present themselves when the lateral edges of the frames are of the finest iron wire; only, in this case, the separation of the points of attachment of the oblique liquid edges is a little greater.

§ 33 Having afterwards employed an octagonal frame of which the height was a third of the diameter of the inscribed circle, I found that although, according to the value before given for the separation of these points of attachment, all of them should have been at the summits of the prism, it was not so: the points in question were still at a certain distance from the summits, and their separation was not more than the sixth of the diameter of the inscribed circle; hence the octagonal film was still enlarged. The same effect was produced with a heptagonal prism of which the height was half the diameter of the inscribed circle—that is to say, less than the separation of the points of attachment as previously estimated in regard to prisms of that number of sides. The same thing occurs in the hexagonal prism, since with a frame of this kind, (§ 26,) the height of which was but $1\frac{1}{6}$ of the diameter of the inscribed circle, the points of attachment of the oblique liquid edges were still found to be a small distance from the summits.

§ 34. In order to discover the cause of these last facts, let us consider an octagonal or heptagonal prismatic frame sufficiently high for the films which proceed from the sides of the bases to occupy under a plane form considerable portions of the lateral faces. At the places where these films quit the faces in question to direct themselves towards the sides of the central polygonal film, they are necessarily convex towards the exterior in the direction of their length; but in consequence of the necessity of a mean curvature null, it is requisite that at these same places they should be concave towards the exterior in the direction of their breadth. If, then, we conceive the frame to be traversed by two planes perpendicular to its axis and passing by the two series of points where arise, on the lateral solid edges, the oblique liquid edges, these two planes will cut the films by arcs concave towards the exterior, and if we imagine these arcs solidified, the equilibrium of the system will not be disturbed. According to this, if we constructed a frame having for its height the separation of the points of attachment of the oblique liquid edges on the same lateral solid edge, and gave to the wires which form the sides of the bases the curvature of the above arcs, it is clear that the laminar system realized in this frame would have its oblique liquid edges proceeding exactly from the summits; but with a frame of that height or of a less height and the sides of whose bases are straight, the condition relative to the transverse curvatures of the re-entering films, and consequently to the form of equilibrium of these films, can evidently not be satisfied unless the points of attachment of the oblique liquid edges be placed at a certain distance from the summits on the lateral solid edges.

§ 35. If, in the different systems which we have been studying, we compare the central polygonal films with one another, it will be seen that the curvature of their sides goes on increasing from the hexagonal film to the decagonal film, a circumstance which constitutes new facts to be added to those of § 20 in confirmation of the law relative to the angles under which the liquid edges terminate at the same liquid point. The transverse curvature (§ 34) of the oblique films which are directed towards the sides of the central polygonal film being connected with the curvature of these sides, it must be less in the heptagonal prism than in the octagonal, and still less in the hexagonal; it is because of the weakness of the curvature in question in the last prism, that this yields, when it has not too much height, a laminar system almost perfect, with its hexagonal film.

§ 36. In reflecting on the generation of laminar systems, the inquiry has struck me whether there might not be realized, at least in the hexagonal prism and with the frame of § 31, a system devoid of the central polygonal film by withdrawing the frame from the glyceric liquid in such manner that the axis of the prism should be horizontal instead of being vertical, as in the preceding experiments. I have consequently so arranged the fork as to be able to operate in that way, and with complete success: I have obtained, indeed, two different systems, according as the frame was withdrawn in such a way that two lateral edges emerged at the same time from the liquid, or first one and then simultaneously the two neighboring ones. These two systems are of the same kind with that which is realized in a pentagonal frame sufficiently high, (§ 30;) that is to say, they are composed of two assemblages of oblique curved films connected with one another by other films directed in the length of the prism. The projection of one of these assemblages on the plane of the base is represented in the first mode by Fig. 39, in the second by Fig. 40.

Fig. 39

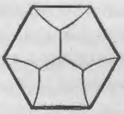
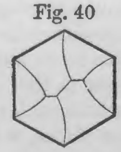


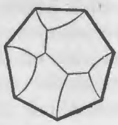
Fig. 40



Nor is this all: to produce the first of these two systems, it is necessary, when half of the frame has been withdrawn from the liquid, to finish the operation with very great slowness; when we operate without this precaution, there is formed a third system of still another kind, a system of which, notwithstanding its simplicity, it is quite difficult to give a clear idea either by description or drawing: it contains two curved hexagonal films, resting, respectively, by one of their sides, on one of those of the bases, and directing themselves obliquely towards the interior of the frame; the other sides of these hexagons have, as usual, a concave curvature; the curved sides of each of these same hexagons are connected with the corresponding sides of the neighboring base and with the homologous sides of the other hexagon by curved films; lastly, at the liquid edges which unite these last films two by two, terminate other films proceeding from the lateral edges of the frame. The sides of the bases on which rest the two hexagonal films pertain to the face of the prism which first emerged from the liquid.

The heptagonal prism yields analogous results, with a frame having its dimensions in the same ratio; only, at first, the three systems are imperfect, in the sense that, in the two former, the films which proceed from the sides of the bases remain, to a certain distance from the summits, adherent to the lateral solid edges, and that, in the third, the films which pass from the curved sides of one of the heptagonal films to the homologous sides of the other are attached, for the greatest part of their length, to the lateral solid edges, presenting, throughout this extent, a form decidedly plane; moreover, by a new singularity the system furnished by the second method is unstable; when barely formed, it undergoes a spontaneous modification: the two assemblages situated near the bases become elongated, at first slowly, then more and more rapidly, reach one another, and immediately there appears the imperfect system with the heptagonal film at the middle. The projection on the plane of the base of one of the assemblages of the system due to the first method is represented by Fig. 41; the projection referable to the second method could not be delineated, because the spontaneous modifications undergone by the system prevent it from being distinctly observed. The facts just described, joined to that stated at the end of § 28, show that with certain frames the results differ according to the manner in which these frames are withdrawn from the glyceric liquid.

Fig. 41



Thinking that the instability of the second system of the heptagonal frame might be occasioned by a want of sufficient length in the frame, I caused another to be constructed in which the length was double the diameter of the inscribed circle; but nothing was gained thereby, besides that the first two systems were produced with more difficulty, and it was the third which was almost always obtained, the system, namely, which contains two oblique heptagonal films proceeding respectively from one of the sides of the bases. Indeed, with a frame still more elongated, it was only this last system which resulted. As regards the octagonal prism, whatever the ratio between its length and the diameter of the inscribed circle, it yields no other than the imperfect systems with the octagonal film in the middle, or with two oblique octagonal films, and again this last is the only one realized when the ratio is sufficiently great.

§ 37. I have said (§ 16) that with the pyramid with square base which had served for the experiments of the second series, the laminar system always presented a little irregularity. This consists in the fact that one or more of the four oblique liquid edges directed towards the summits of the base do not exactly terminate at those summits. I am now able to indicate the cause: if we inquire the height of a quadrangular pyramid in which two adjacent triangular faces make between them an angle of 120°, it will be found that this height is half the side of the base; when we employ, then, a frame having this height or a less one, it is clear that the films will simply occupy the four triangular faces; and this is verified by experiment, such a frame yields nothing else in whatever manner it be withdrawn from the liquid. Now, in the frame of the experiments of the second series, the length of the side of the base was 67^{mm}, and the height of the pyramid 50^{mm}; this height, therefore, was but about three-fourths of the length of the side in question; but, in such a pyramid, the films which proceed from the sides of the base must have but a feeble tendency to separate from the oblique solid edges, and it is to be inferred that they may continue to adhere to them for a certain extent.

§ 38. It has just been seen that with a quadrangular pyramid whose height is at most equal to half the side of the base, none but plane films occupying the triangular faces are ever obtained; but an assemblage of this kind does not constitute a laminar system, for all these films are rendered independent of one another by the intermediate solid edges.

From all that has been stated since § 32 may evidently be deduced the following conclusion: When, in a polyhedral frame, there is a continued and recurring succession of identical adjacent faces forming between them angles greater than 120°, and all arranged after the same manner as the succession of lateral faces of a prism, heptagonal, octagonal, &c., or as that of the lateral faces of a pyramid, hexagonal, &c., that frame gives an imperfect laminar system—that is to say, one containing films which, through a portion of their extent, adhere at the same time to two solid edges; or else it gives no system—that is to say, all the faces but one are simply occupied by plane films; I say but one, for, in this case, it is necessary that one face should remain open to allow the introduction of air; in the quadrangular pyramid mentioned above, for instance, the face which remains open is the base.

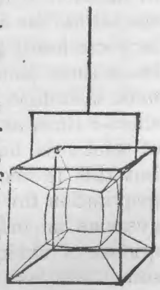
We may make an interesting application of this principle. Among regular polyhedrons, it has been seen that the tetrahedron, the cube and the octahedron furnish perfect laminar systems, and, in these three polyhedrons, the angle of two adjacent faces is, in effect, less than 120°; there remain the dodecahedron and the eikosahedron; now, in the first of these the angle of two adjacent faces is but 116° and a fraction, and consequently less than 120°, while in the second it is 138° and a fraction; it might be seen, therefore, that the dodecahedron would give, like the preceding regular polyhedrons, a perfect laminar system, but that such would not be the case with the eikosahedron, and this is confirmed by experiment; with the eikosahedron, in whatever manner the frame be withdrawn, none but plane films are ever in nineteen of the faces, and the twentieth is void. As to the system of the dodecahedron, there would be difficulty in describing or drawing it.

§ 39. In order to give more precision to these results, I would say: 1st. The frame of any polyhedron in which the dihedral angles are inferior to 120° gives a perfect laminar system; such are the frames of the tetrahedron, the cube, the octohedron, the dodecahedron, the prisms whose number of lateral faces is less than six, &c. There are, however, some rare exceptions, as that presented by the frame of a quadrangular pyramid whose height does not much exceed the half of a side of the base. 2d. The frame of any polyhedron in which all the dihedral angles are superior to 120° gives a laminar system null. Examples: The frame of the regular eikosahedron, that of the assemblage of two hexagonal pyramids, or one containing a still greater number of sides, united by their bases, and such that at the edges of the common base the dihedral angles exceed 120°, &c. Nevertheless, when all the dihedral angles but a little exceed 120°, we obtain, in certain cases, a real laminar system; this, for instance, occurs with the frame of the polyhedron formed by cutting down the summits of a cube by equilateral sections which join one another, so that there shall only be triangular faces and square faces. But in this polyhedron all the dihedral angles are but about 125°; moreover, the real and symmetric laminar system is produced with difficulty, and only when the frame is withdrawn by a triangular face; when withdrawn by a square face it always yields a system null. 3d. A frame of which the dihedral angles are some inferior and other superior to 120°, but in which the faces which comprise between them these last angles are identical, disposed in the same manner, and form a continuous and recurring succession, gives an imperfect laminar system or a laminar system null: for instance, the system is imperfect in the frames of prisms the number of whose sides exceeds six, in that of a hexagonal pyramid when withdrawn from the liquid by its summit, &c.; it is null in that of a quadrangular pyramid whose height is less than half the side of the base, in that of an hexagonal pyramid when withdrawn from the liquid by its base, &c. 4th. When the frame falls within neither of the preceding categories, the laminar system is sometimes perfect, sometimes imperfect, sometimes null. For example, we have a perfect laminar system with the frame of a prism having for its base a lozenge of which the two obtuse angles exceed or equal 120°; we have, as has been seen, an imperfect system with the frame of an hexagonal prism sufficiently tall when it is withdrawn with the axis vertical; with a frame representing the assemblage of two pentagonal pyramids united by their bases and sufficiently tall for the dihedral angles, in each of them, to be less than 120°, while the dihedral angles which have for edges those of the common base are greater than 120°, we have a system null when it is withdrawn with the axis vertical, and an imperfect system with the axis horizontal, &c.

§ 40. Let us indicate some other curious modifications of our laminar systems. In the first place, we know that in the system of the cubic frame (Fig. 18) the central quadrangular lamina is parallel to two opposite faces of the cube, but, because of the symmetry of the frame, it is evidently indifferent, as concerns equilibrium, whether this parallelism exists in relation to one couple of faces or to another; the lamina may, therefore, equally occupy three positions, and it may readily be supposed that a slight cause suffices to decide its destination. Thus, when the frame is withdrawn from the glyceric liquid, the lamina in question is found sometimes parallel to the anterior and posterior faces, sometimes parallel to the faces of the right and left, and it sometimes happens that it is even placed horizontally. Further, it may be made to pass at will, and several times in succession, from one of these three positions to another; for this purpose it is sufficient to blow very gently on one of its edges by the face of the frame on the side of which this edge presents itself; the lamina is then seen to shrink in the direction of the blowing, to be reduced to a simple line, then to be reproduced in its new position. These last phenomena were pointed out to me by M. Van Rees, who was so obliging as to repeat my experiments in Holland.

In the second place a laminar system may be forced to deviate from our law according to which it should not include any portion of air imprisoned on all sides by films, and then, in several frames, we obtain by proper management new and very pleasing results. The process, which was likewise indicated to me by M. Van Rees, consists in producing at first the ordinary system, then again immersing the inferior face of the frame to the extent of some millimetres, and withdrawing it anew; there is thus formed, in this face, a plane film which confines the air between it and the oblique films proceeding from the sides of this same face, and which, immediately climbing up between these oblique films, drives the portion of air before it, giving rise to a new system, which is symmetrical when circumstances will permit. For instance, with the cubic frame, the new system, which is represented at Fig. 42, contains in its middle a laminar cube attached by its edges to the films proceeding from the solid edges; only the edges, and consequently also the faces of this laminar cube are slightly convex, which is easily explained by the law relating to the angles formed by liquid edges with one another. So, too, with the frame of the tetrahedron, the new system contains in its midst a laminar tetrahedron with convex edges and faces. Analogous results are obtained with the frame of the pentagonal as well as that of the hexagonal prism; but that of the triangular prism gives a non-symmetrical figure. That of the octahedron, if one of its faces be re-immersed parallel to the surface of the liquid and be withdrawn in the same way, furnishes a symmetrical result, in which, however, the central laminar octahedron has four faces triangular and the four others hexagonal. These systems are evidently mixed ones, in which a part of the films is of mean curvature null, while the other is of mean curvature finite and constant. Again: if, after having realized one of these mixed systems, we break one of the films which compose the central polyhedron, the whole is seen to return instantly, or in a very short time, to the ordinary system. For instance, when, in the mixed system of Fig. 42, we break off one of the films of the laminar cube, the system immediately resumes the arrangement of Fig. 28. This again is a curious transformation. For breaking these films it is best to use a point of filtering paper.

Fig. 42



In the third place, if, after having formed the ordinary system of the cube, (Fig. 28,) we break, by the means just indicated, the quadrangular lamina, the system immediately assumes a wholly different and equally regular arrangement; the new system presents a void in the middle, but it may still be considered as perfect in the sense that films proceed from all the solid edges. So when, in the system of the quadrangular pyramid (Fig. 22) we break the superior lamina, a new and beautiful system is obtained, which in like manner is perfect, though also presenting a void in the middle. Finally, an analogous result is brought about, but with two vacancies, by breaking, in the system of the octahedron, (Fig. 26,) first the superior quadrilateral, next the film which then replaces the inferior quadrilateral.

§ 41. We will terminate this series by a remark relative to the persistence of laminar systems. All those of the polyhedral frames are less enduring than the figures formed of a single film, such as spherical bubbles or figures of revolution, (5th series, §§ 14 and 15.) The explanation of this fact rests on the considerations stated in §§ 32 and 33 of the 2d series, in reference to laminar systems realized with oil in the interior of the alcoholic liquid—considerations which I shall here reproduce in a more complete manner.

It will be remembered that a liquid film adhering to a metallic thread is necessarily joined to this, or rather to the liquid layer which moistens it, by a small mass with transverse curvatures strongly concave. The two surfaces of this small mass exert, therefore, a capillary pressure less than those which correspond to the two surfaces of the film, and consequently the excess of pressure of the latter must continually drive the liquid towards the former. Hence a new cause for the progressive attenuation of the film, and consequently of the destruction of the figure. This cause exists in every laminar figure adherent to a thread or to wires; but in the systems formed in the interior of polyhedral frames, there is added a cause of the same nature, and still more energetic. All these systems, in effect, contain liquid edges, which are united four by four in liquid points, and of which several are attached by their other extremity to solid threads; now, on these edges, as well as at their extreme points, there are also, we recollect, small surfaces strongly concave. Those which pertain to the edges are, like those which prevail along the wires, only concave in one direction, and consequently their action in rendering the films thinner is of the same order; but, at the points of junction of the liquid edges, and at the points where these edges terminate at the wires, the small surfaces are concave in every direction, so that their capillary suction is about twice more strong, and hence the afflux of liquid towards these same points must be much greater. In the systems in question, the films must therefore become thin much more rapidly, and these systems endure for a shorter time, as experience evinces they do. There is no gradual augmentation of thickness, however, either in the liquid edges or in their points of junction, but this is because the liquid, in proportion as it flows thither, is drawn by gravitation towards the base of the system. The persistence of each of these systems is, indeed, very variable, as may be readily conceived, for, in consequence of their complexity, slight accidental causes may act, sometimes more, sometimes less, to induce their destruction; whence, of all of them, that which in general is longest maintained is the most simple, namely, that of the tetrahedron.

I shall recur anew to the laminar systems in order to consider the theory under a more general point of view. In effect, as I have already shown, (§ 2 and note of § 8,) the liquid films which compose them may be assimilated to stretched membranes, and hence each system will be arranged in such a manner that the sum of the surfaces of all its films shall be a minimum. But I reserve this subject for another series.

REPORT ON THE TRANSACTIONS

OF THE

SOCIETY OF PHYSICS AND NATURAL HISTORY OF GENEVA,

FROM [illegible] 1865, TO [illegible], 1866.

BY PROFESSOR [illegible]

TRANSLATED FOR THE SMITHSONIAN INSTITUTION.

A rule of the society imposes on me the obligation of retracing the principal subjects which have occupied attention during the year just elapsed. In proceeding to acquit myself of this official duty, I should state in advance that the large number of the communications made to the society in its eighteen sessions, while it evinces the scientific interest and constantly increasing zeal which have animated our periodical reunions, leaves me no option but to restrict this notice to the communications in which our members have taken the initiative. This review, however, will at least have the advantage of being more distinctly scientific, since it will be based on accurate minutes kept by our worthy secretary, M. Alexander Prevost. Without changing anything in the plan adopted in the preceding years, I have simply added the date of each communication, with a view of securing to the several authors the priority which belongs to them.

PHYSICAL SCIENCES.

Experimental and theoretical physics.—Professor Wartmann described (September 7, 1865) a pneumatic mercurial pump, for the construction of the tubes of Geissler, and of barometers and instruments requiring as perfect a vacuum as possible; reminding us, at the same time, that the first practical apparatus for the production of this vacuum was due to Mr. Welsh, who applied it in the atelier at Kew. In a recent number of his journal M. Poggendorff has given a modification of Geissler's pump, which requires the use of an ordinary pump and of a great quantity of mercury. No detailed description of the instrument employed by the [illegible] having been given, M. Wartmann has constructed one, all the parts of which are stationary, and which requires but a moderate quantity of mercury. A system of wrought iron, with cocks of [illegible], connected by an [illegible] with [illegible], serves to fill completely with mercury the receiver which is to be emptied of air. The mercury reaches this receiver by [illegible] that the [illegible] of [illegible] suffices to expel the vapor and air adhering to the walls. The tube, when full, is closed by an upper cock, emptied of mercury by the lower cock, and finally closed (after having received by means of a lateral tube any rarefied gas which it is to contain) by the fusion of its two tubulures in contact with the lamp.

A memoir was read by the same member (October 5, 1865) on the *maximum explosive distance between the negative electrode* [illegible] *of the current of Ruhmkorff's apparatus.* He has ascertained that the spark is longest when it passes between the positive [illegible] and the negative [illegible] electrodes. There is here, therefore, an influence of the direction of the

REPORT ON THE TRANSACTIONS

OF THE

SOCIETY OF PHYSICS AND NATURAL HISTORY OF GENEVA,

FROM JULY, 1865, TO JUNE, 1866.

BY DOCTOR GOSSE, PRESIDENT.

TRANSLATED FOR THE SMITHSONIAN INSTITUTION.

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A memoir was read by the same member (October 5, 1865) on *the maximum explosive distance between the sequent electrodes for the current induced by the opening of the circuit of Ruhmkorff's apparatus.* He has ascertained that the spark is longest when it passes between the positive exterior and the negative interior electrodes. There is here, therefore, an influence of the direction of the discharge, which does not exist in the electric battery. The explanation of this singular fact is found in the difference of accumulation and tension of the electricity at the two extremities of the inductive wire, and in the elevation of temperature of which the negative pole is the seat.

M. Wartmann also communicated (November 16, 1865) some observations which he had made on *the appearance of the spark produced in a vacuum by currents of induction.* This spark is elongated; at the positive pole it sometimes presents a reddish effluence, and at the negative pole it becomes violet. By employing for the electrode a cylinder of soft iron, the reddish effluence was environed by a violet aureole. With reference to these researches, he announced (December 7, 1865,) that he had made use of a tube of glass, having two concentric chambers without communication with one another, and each enclosing traces of a gas different from the gas of the other. The positive electrode was in one of the chambers, the negative in the other. When, in this case, a powerful coil is employed, the course of the current may be reversed without changing the colored appearance at the two poles. M. Wartmann states that, contrary to what is affirmed by the manufacturers of the tubes of Geissler, he has found that in process of time the tubes, so far from improving, lose something of their efficacy. He remarked also that after new researches he has seen no influence exerted by magnetism on the stripes of the spectrum, which is a confirmation of the conclusions he had advanced several years before.

Professor Volpicelli, honorary member of the society, (present at the meeting of September 7, 1865,) took occasion to observe that, although we have numerous investigations respecting the quantity of atmospheric electricity, the nature of that electricity has been little studied. He thinks that he has observed a diurnal qualitative period in the atmospheric electricity, a quotidian passage from positive to negative. He is of opinion that for this kind of researches it is necessary to return to the method of Franklin.

Professor Marignac, in reference to the researches published in France on *aeronautics by wings,* maintains (October 5, 1865) that their authors have proceeded on entirely false data as regards the quantity of labor necessary in order that a machine for flying should sustain itself in the air. According to his calculations, in a machine of this sort, the ratio of the motive force to the weight raised varies as the square root of the section of the wings. The motive force should augment proportionally to the square of the weight raised, which is an unfavorable condition for the realization of such machines. From this it would result that, if all birds had the same form and structure, the largest would fly with least facility.

M. Cellerier presented (November 2, 1865) a memoir *on the measurement of gravity by means of the inversion pendulum of Bessel.* He showed that, for this kind of observations, it is requisite to attain a relative exactness of at least one hundred-thousandth. Setting aside the errors pertaining to the measures of time, there are others which are found to act on the inversion pendulum, so that the alteration which results therefrom in the duration of the oscillations, according to the two suspensions necessary to this apparatus, is constantly in the same ratio, and the alteration which results in the two cases, from the defect of the measurement of distance of the points of suspension, is also in the same ratio By means of the two observations of duration, it is thus practicable to eliminate at once all these small alterations, and to obtain the duration reduced to a vacuum. This result is of great importance, because the accidental movements of the air have never been susceptible of being exactly appreciated. The apparatus of Bessel may thus lead to a rectification of the value assigned to gravitation.

General Dufour (December 21, 1865) gave details of the *direct levelling now in process of execution in Switzerland.* The plate fixed in the stone of the Niton at Geneva will serve as the point of reference; and of course there will be negative numbers, as Bâle is nearly 200 metres lower. Professor Plantamour communicated (December 21, 1865) certain results arrived at in the operations of this levelling, with the excellent instrument constructed at Aarau by Mr. Kern, and of which the level is particularly accurate. These operations, conducted in concert with the international geodetic commission, are connected with the arc of the meridian, the study of gravitation and that of the form of the terrestrial spheroid. In regions where the slopes are not very great, three or four kilometres are executed in a day, and the error scarcely transcends two or three millimetres to the kilometre. Thus far the levelling has been carried from Neuchâtel to Chaumont and the Chasseral; then by the valley of Ruz, the Chaux-de-Fonds and Locle to Morteau, from Nyon to Saint Cergues and the Rousses, and from Geneva to Morges and Neuchâtel. The polygon of levels which connects Neuchâtel with the Chaux-de-Fonds and Saint Imier has a development of about sixty-one kilometres, and was closed with an error of only fourteen millimetres. In proceeding to connect the stone of Niton with the *limnimetre* on the quay, it was found that the column must have sunk more than five centimetres in eighteen months. This is owing to the subsidence of the ancient bed of the lake on which the new quay is constructed. The same member communicated to the society (March 15, 1866) the continuation of his researches *on the determination of the value of gravitation by means of the pendulum of Repsold.* He has succeeded in further eliminating some causes of error in considerably prolonging the duration of each experiment. Instead of observing only series of 500 oscillations during six or seven minutes, he has continued his observations for some forty minutes, which have given him nearly 3,000 oscillations. These new observations have raised the value of gravitation at Geneva to 9 metres .80381, a result which may be considered as exact to nearly $\frac{1}{4900000}$.

M. Perrot announced (January 4, 1866) that he had succeeded in causing to be constructed a furnace for melting substances such as gold and silver, heated by the ordinary lighting gas. M. Philippe Plantamour called the attention of the society (January 10, 1866) to a fact of interest in horology, observed by himself, namely, that in clocks with an anchor escapement, it is the pallet of steel which is worn by the wheel of brass, and that in watches, the pallet of ruby is worn by the teeth of the wheel made of steel.

Professor de la Rive gave an account (March 1, 1866) of researchés made by Professor Dufour of Lausanne, in behalf of the commission of the Helvetic Society charged with *the study of terrestrial electric currents* After explaining the difficulties met with in the observation of these currents, as well as the expedients adopted by M. Dufour to surmount them, M. de la Rive announced that there is an electric current almost constantly passing from Berne to Lausanne, that is to say, from north to south, though sometimes it exists in the inverse direction. M. Dufour has clearly shown that this is no thermo-electric current. The nearly constant existence of these currents, proceeding from the pole to the equator, connects itself with the continual production of the aurora borealis. When these phenomena augment in intensity and become visible in lower latitudes, the terrestrial currents also become more intense. The same member having afterwards (April 5, 1866) resumed an investigation of the phenomena which he had observed in plates of glass submitted to a simple discharge of Ruhmkorff's apparatus, was led to repeat the well known experiment of compressed laminæ of glass, which exhibit, as long as the compression subsists, certain optical properties. In one of these experiments, the compression having been too sudden, a splinter of glass was detached from the plate, which, although withdrawn from the compression, preserved the optical properties which had been communicated to it. At the same sitting M. de la Rive gave some details on the experiments which he had made in seeking to explain *the phenomenon of the vibratory movements determined, in the conducting body, by the combined action of magnetism and the discontinuous currents.* He has succeeded in demonstrating that conducting bodies transmit electricity by molecular discharges, analogous to small voltaic arcs. The sound produced, when use is made of discontinuous currents and a powerful magnet, is not due to the vibration of the conducting body, but to the vibrations of the molecules of that body, which are under the influence of the magnet; hence, to reproduce the phenomenon, we may operate on metallic powders, or even on a column of mercury.

Dr. Dor (April 5, 1866) drew attention to the sinking of the waters of the lake which occurred at the close of the winter, and which appears to have followed the removal of the dam of the Rhone at Geneva. This subsidence, however, is now general. It was perceived first at Nyon, then successively at Rolle, at Ouchy and at Vevay. Colonel Gautier, (May 3,) in reply to the observation of M. Dor, said that he had compared the heights of the water marked on the limnimetres of Geneva and Vevey, during the months of February and March. He thinks, with General Dufour, that the effect of the dam on the level of the lake could be only very restricted.

Meteorology.—Professor Gautier read a note (September 7, 1865) *on the results of meteorological observations made, in* 1864, *at seventy-six Swiss stations,* under the relation of temperatures and quantities of water which have fallen. To this note is added a table of the thermometric mean, both annual and for the four seasons, as well as the annual quantities of rain and snow at those stations. A confirmation is hereby afforded of what Professor Plantamour had already observed, namely that at equal heights the temperature of the Swiss stations to the south of the great chain of the Alps is more elevated, by about three degrees Centigrade, than that of the stations to the north of the chain On the north side of the chain, at very proximate points, there are considerable inequalities of temperature. In the annual quantities of the water which falls there are also great differences between stations quite near one another and situated at nearly the same height.

Dr. Lombard, applying *the rules of statistics to the influence of the seasons on the mortality,* communicated (December 7, 1865) one of the results of his inquiries, namely, that in the habitable temperate zone, cold augments the mortality, and more particularly that of infants in the two years which succeed birth. Thus there are more deaths in winter than summer. At Geneva, the month of February is, in this respect, the least, the month of September the most healthy. There is an exception, however, to the rule, wherever the intermittent fever (malaria) prevails, and the greatest mortality takes place during the hot weather. This exception is observed even in cold countries, such as Sweden and Iceland. Official documents from Australia, Chili, &c., evince the general rule to be the same in the austral hemisphere.

Professor Plantamour (January 10, 1866) referred to the anomaly which was observed at Saint Bernard in the month of December last. During several consecutive days the temperature there was two degrees higher than at Geneva, and he asks whether the beds of fog which then covered the valleys might not have contributed to warm the mountains by reflecting the solar rays.

Professor Favre communicated (February 1, 1866) details respecting the station at Saint Theodule, in Valais, where three guides have been installed since the 1st of August last, in order to make meteorological observations for a year. The present winter seems, so far, to have been there quite mild. There were days in December when the temperature was probably higher than at Geneva.

Chemistry.—M. Louis Soret communicated (November, 2 and 16, 1865) the results at which he has arrived in his researches *on the density of ozone,* a subject on which he had already published a memoir in the *Archives* of the *Bibliothèque Universelle* of 1863. On this occasion he announced two new series of experiments. In the first, he occupies himself with the diffusion of ozone, and proves that this diffusion and the action of the ozone correspond to those of a gas more dense than oxygen. In the second, directing his inquiries to the existence of a body which really absorbs ozone, instead of separating it by absorbing but a part, as is the case with the iodide of potassium, he finds the essence of turpentine to be the most favorable substance for effecting this absorption. Ozonized oxygen, when treated with this essence, undergoes a very considerable diminution of volume, being very nearly double the volume of oxygen absorbed by the iodide of potassium. This would confirm the hypothesis that the molecule of ozone is composed of three atoms of oxygen, and that consequently the theoretical density of ozone is one and a half times that of oxygen. In the sequel he has found in the essence of cinnamon a substance not less favorable than the essence of turpentine for effecting the absorption of ozone, and comparing the diminution of the ozonized gas, treated with these essences, with the augmentation of volume which the same gas undergoes through the action of heat, he has arrived with more exactness at the conclusion that, in effect, the density of ozone is one and a half that of oxygen.

M. Delafontaine, (February 1, 1866,) after recalling the fact that his previous researches have confirmed those of Mosander, on ythria, terbine, and erbine, explained how it may have happened that MM. Bunsen and Bœhr did not recognize these three earths. His last researches, while confirming his previous inquiries, and consequently also the results obtained by Mosander, have led him to see that MM. Bunsen and Bœhr, who deny the existence of terbine, have, notwithstanding, had that substance in their hands, but that they have taken it for erbine.

Professor Marignac (February 15, 1866) called the attention of chemists to *the danger arising from the contact of the double salt of the fluoride of niobium and potassium with the skin.* This salt produces an alteration of the cutis, under the form of rubefactions and eruptions, accompanied with itching, which may last without diminution for six months after the accident.

Professor Wartmann (May 15, 1866) also drew the attention of the society to *the dangers of explosion presented by the oxalate of silver.* MM. Marignac and Perrot suggested that the explosions observed by M. Wartmann might be owing to the presence of organic substances mixed with the oxalate.

NATURAL SCIENCES.

Geology.—Professor Favre, member of the federal geological commission, presented (January 10, 1866) a geological chart of the environs of Brugg, executed by M. Mœsch, and which forms a part of those published by the commission.

M. de Loriol read a memoir (March 15, 1866) *on the upper Jurassic deposits, and particularly those of the environs of Boulogne sur Mer.* Both in England and France the marls composed of the *Ostrea virgula* are usually surmounted by masses of limestone, more or less compact, sometimes coralline, which are covered in turn by deposits of Purbeck. These formations represent the Portlandian, properly so called, and, in England, comprise a very special fauna, of which the *Trigonia gibbosa* is one of the characteristic fossils. At Boulogne are found strata analogous to those of the Purbeck beds, beneath which beds of limestone, sand, sometimes clay, form the equivalent of the English Portlandian, and include several of its characteristic fossils, among others the *Trigonia gibbosa.* In proportion as we descend into the series of deposits, the species of the English portlandian are found to diminish in number, and those of the kimmeridian to increase. Immediately above the *Ostrea virgula* marls occurs a bed of limestone bearing the *Ammonites gigas,* (Zieten,) which terminates the series of strata that may be considered as the equivalent of the English Portlandian, or rather as the upper portion of the kimmeridian group, of which the Portland beds would constitute but a local *facies.* The inferior layers of this mass have been traversed by the railroad from Boulogne to Calais, in which excavations have brought to light numerous fossils in a state of remarkable preservation, among others the *Neritoma sinuosa*, (Morris,) which is very abundant. (See Memoir de la Société de Phys. et d'Histoire Nat., tome xix, first part.)

M. Chaix communicated (April 5, 1866) his observations *on the volcanic rock of the left bank of the Rhine, near Andernach.* This rock, which he has traversed and studied, extends over a surface of 7½ leagues from north to south, by 3¾ leagues from east to west. Thirty cones, rounded at the summit, are the craters of as many extinct volcanoes, rising to a mean level of 700 feet above the surrounding inhabited places, which last are 500 feet above the sea. The plains are formed of deep beds of cinders, lapilli and volcanic tufas. At more than 100 feet below these deposits a thick bed of basaltic lava is worked, near Niedermending, for mill-stones. In the centre of the volcanic region the lake of Laach, circular in its form, has a surface of 338 hectares, a depth of 177 feet, and a level of 847 feet above the sea, since the construction of a subterranean drain has reduced it by 18 feet. Besides this lake, thirty ancient craters may be counted, which have been converted into lacustrian basins, more or less dried, and especially numerous in a second volcanic region lying more to the west than the first, and to the north of Trèves.

Botany.—M. Marc Micheli communicated (July 6, 1865) observations *on the stamens in the family of the Ericaceæ.* In some species of this family the stamens are provided with appendages of very variable forms; these are sometimes long awns, sometimes membranaceous awns, sometimes simply small strumæ. He has found that, when observed with the microscope, whatever their outward appearance, their internal structure is always the same. Under a layer of epidermis is seen a certain quantity of spiral cells. The tissue which these form is prolonged to a certain distance along the walls of the loculaments of the anther. The distribution of the appendages in the species and tribes of the family follows no rule. Two neighboring kinds or species may differ in this respect.

M. Duby, (October 15, 1865,) with reference to a champignon of the vine, on which he read a notice thirty-one years ago, said that he had never met with it since then, but that this year it is very abundant. MM. de Candolle and Muller are inclined to ascribe the development of its spores to the drought which has prevailed. M. Duby, in giving an account of the new researches of MM. Bary and Allié on the Mucedineæ, recalled the fact that when the spores of the *Mucor mucedo* are sown on different substances, completely different forms are seen to be developed in the resulting mouldiness, which have been classed heretofore as different species. It is probable, therefore, that more than twenty species of Mucedineæ should be suppressed. These same spores, sown in water, may even produce algæ. In support of these observations, Professor de Candolle added that his father had already remarked, at Annonay, that the champignons which were developed on old rags or waste scraps varied according to the origin of those articles. At Paris, the dealers in cheese had assured him that the mouldiness varied according to the part of France from which the cheese was brought.

Dr. Muller (November 2, 1865) entered into some details on *the inflorescence of the genus Dalecampia.* This inflorescence is composed of a general involucrum formed by three pairs of decussated bracteæ. The involucrum encloses flowers of two sexes. After having described with care the position of the female and male flowers, M. Muller compares this inflorescence with that of the genus Euphorbia, the involucrum of which is quincuncial, and shows by diagrams that in the Euphorbias the relative position of the two sexes presents the contrary of what exists in the Dalecampias, the summit in the former being occupied by a female flower and not by male flowers. In comparing the male flower of the Dalecampia, which is polyandrous, and provided with a calyx and an articulated pedicle, with the male flowers of the Euphorbias, which are monandrous and destitute of a calyx, and taking account of the arrangement of the flower in the species Dactylostemon and Actinostemon, M. Muller arrives at the conception that the part situated above the articulation of the male flowers of the Euphorbias should be regarded as an axis—that is to say, as a part of the articulated pedicle destitute of a calyx, or rather a prolongation of the receptacle bearing on its summit a sessile anther. There would then be nothing herein very paradoxical, and we should only have a monandrous flower, the filament of which would be as large as and sometimes larger than the pedicle. The same member announced (December 21, 1865) that in the genus Macaranga of the family of the Euphorbiaceæ he has met with individuals, which presented on the same flower stamens with three and with four loculaments. This would suppress the genus Pachystemon, which would no longer be distinguished by any character from the genus Macaranga.

In the last place, M. Muller (May 3, 1866) spoke of *the secondary characters of the aestivation of the calyx*, which enable us, even long after the expansion of the flower, to recognize in what manner the lobes of the calyx or the sepals must have been disposed at the time of the budding. In the valvate aestivation, these lobes or sepals are all equidistant from the arch of the flower, and, before the expansion of the calyx, each lobe from the base to the summit touches its neighbor by the unattenuated edge of its borders. In the imbricated aestivation, on the contrary, the lobes are not equidistant from the centre, one of the two neighboring lobes being always more exterior than the other, and one of the borders covering the other. In the first case, the lobes are necessarily acuminated in order to their meeting at the summit of the calyx, and (if the calyx be straight) they have necessarily the same length, and the borders, touching one another face to face in the same plane, cannot but be entire. Hence M. Muller considers it established that, in flowers which have already opened, each of the five following characters indicates an imbricated aestivation in the bud:

1. Sepals, or lobes of the calyx, being alternately of an equal length.
2. Sepals, or lobes of the calyx, being rounded at the summit.
3. Sepals, or lobes of the calyx, being attenuate, membranous or scabrous on their borders.
4. Sepals, or lobes of the calyx, being variously dentated, lobated, laciniated, or otherwise divided.
5. Sepals, or lobes of the calyx, having marginal (particular) bristles, situated in the same plane with the lobes.

M. Casimir de Candolle communicated to the society (January 4, 1866) a memoir on the *family of the Piperaceæ*, which is to form part of the Prodromus. The author divides the family into two groups, the Piperomiæ and the Piperiæ, according to the organization of the stalk. In treating of the evolution of the leaves, and of their structure, he establishes that the so-termed stipules opposed to the leaves are prophylla; that the mode of evolution of the buds and the arrangement of the leaves explain the irregularity of the limbs. As to the nervation, it is in this family penninervate or digitinervate; but these two systems enter into the same type in the same manner as the intermediate nervations called multiplinervate. If, in effect, sections be made in the leaf at different distances from the petiole, the lateral nervures are seen to proceed from the median nervure, and all the nervures are formed of the prolongation of the external layers of the peripheral fasciculi of the boughs. On comparing the distribution of the fasciculi in the bough and the leaf, certain analogies are perceived between these two organs, which are commonly considered as very different. The leaf offers analogues of the bark, of the ligneous system, and the medulla; it often represents a flattened bough. The author concludes with a table of the classification, in which he materially reduces the number of species admitted by some botanists.

M. Edmond Boissier exhibited (February 1, 1866) specimens of three kinds of plants recently discovered, which are worthy of remark from having been found in regions very remote from those inhabited by all the other species of each family to which they pertain. In the first place, a *Dioscorea pyrenaica*, a European and Alpine species found in the Pyrenees; then a *Pelargonium*, discovered in the chain of Taurus, while almost all the species come from the Cape, except a few species brought from New Holland; lastly, M. Boissier has recently found a *Pilostyle*, or a plant of the prickly Astragalus, which was sent to him from the East by M. Hausknecht, botanist.

Professor de Candolle stated (March 1, 1866) that, in studying the genus *Begonia*, he had recognized therein fifty-seven very natural sub-genera. As a general principle, it will be found that in proportion as the groups of a genus are better established and characterized those groups will be geographically circumscribed. The African species of the *Begonia* are generally very different from those of other equatorial regions, and particularly from those of Brazil. M. de Candolle therefore deems it probable that the deep cavity which separates America from Africa has existed from very remote geological epochs.

Zoology.—Dr. Claparede communicated (July 6, 1865) the result of observations which he had recently made on the *Rotifers*, with a view to explaining the mechanism of their alimentation. He shows that the cilia of the rotary organ do not directly contribute to the afflux of liquids towards the mouth; they merely generate closed currents, which move in something of an ellipsis perpendicular to the plane of the vibratory organ and tangential to the ciliated border of that organ. These currents pass along a groove under the edge of the rotary organ and parallel to that edge. A part of the molecules, carried along by these closed currents, penetrate into the groove, and are thence conducted into the mouth by a row of secondary cilia. This second row of cilia presents, in effect, a contrary movement in the two rotary organs of the Rotifers, and in the two lobes of the single organ in the Melicertæ. Among all those of the animals in question in which the rotary organ is double, as in the Rotifers, the apparent movement is identical in the two organs; that is, the inverse of that of the hands of a watch. The mouth being situated between the two organs, it is impossible that both should convey the nutritive particles to the mouth, and yet direct observation shows that the aliments are precipitated from right and left into the buccal orifice. The same contradiction presents itself in the Rotifers with a single organ, but bilobate, with the mouth under the groove; as, for example, in the Melicertæ. The movement, in fact, passes from the right to the left lobe, preserving the same direction. It is consequently directed towards the mouth in the first, and away from the mouth in the second; nevertheless, the nutritive particles flow from the two sides into the mouth.

The same member, (January 4, 1866,) after having noticed the latest researches of Mecznikoff and Leuckart on the transformations of the Nematoidæ, gave a history of the different states of the *Ascaris nigrovenosa*. He entered also into details on the development of the '*Cucullant*' of the pike and the perch, and on that of the '*Olulant*' of the cat.

M. Lunel read a memoir on the *genus Brama*, (see Mem. de la Société de Phys. et d'Hist. Nat., t. xviii, p. 165,) a genus of fishes heretofore imperfectly studied, and on a new species brought from the island of Cuba by H. de Saussure, to which the author has given the name of *Brama Saussurei*. In this interesting treatise, accompanied with plates and new anatomical details, the author especially sets forth the very remarkable differences presented by the scales in this group of fishes—differences which furnish excellent specific characters.

M. Victor Fatio, (October 5, 1865,) in speaking of experiments which he had made to verify the kind of utility incident to the cavities of air in the bones of birds, stated that these cavities, though perforated, had not perceptibly affected the faculty of flying.

The same member read a memoir (February 1, 1866) *on the changes of coloration in the feathers of birds.* After having studied the structure and development of feathers, the author analyzes the causes which may produce a change of coloration in the same feather, without a moulting having taken place He seeks to explain the variety of colors and reflections by the play of the light falling on parts differently developed. He finds in autumn, in every feather, an apparent color and a latent color, and attributes the brilliant coloration of spring to the internal solution of pigmentary granules existing in the interior of the tissues. The humidity of the ambient air tends to swell the cortical substance of the feathers, at the same time that the fat of the body dissolves the latent pigment, which is diffused and colors the feather from the periphery to the centre. The extremities dry up and permit the portions which become gradually colored to appear. M. Fatio distinguishes three kinds of feathers—the ordinary, which contain a pigment of the color they affect when seen either by transparence or by incident light; the optical feathers, which present reflections and contain a brown pigment; lastly, the enamelled feathers, which, always blue, contain a blackish pigment. Now, in the ordinary and the enamelled feathers it is the barbs which are developed, while in the optical feathers it is the barbules which become swollen. The author attributes the luminous effects to a phenomenon of interference; he compares to colored rings the lines alternately brilliant and obscure, exhibited in incident light by the segments and separating partitions of the optical barbules; and he explains the blue color of the enamelled barbs by the passage, across a colored transparent layer, of rays reflected below by a dark layer tinted in a different manner. He remarks that it would be improper to confound the phenomena of which he has given this account either with those of the discoloration which is observed in some of the palmipeds—a discoloration accompanied by a discharge of the pigment, or with the external coloration produced by rubbing in certain birds, or with the discoloration which takes place in collections by the saponification of the fats. Finally, rejecting all idea of a renewal of life in the feather at the time of its second coloration, the author finds in his observations the explanation as well of albinism as more especially of local varieties. (See Mem. de la Société de Phys. et d'Hist. Nat., 2d partie, t. xviii, p. 249.)

M. H. de Saussure presented to the society a work, which he has published in collaboration with M. Sichel, entitled *Catalogus Specierum Generis Scolia.* He took occasion to observe that among the Scolias the females sometimes vary much from one another, while the males change but little; so that, in some kinds, it might be said that the females were of different species, while the males are all of one species. This may be explained by the fact that the males exist for little else but the accomplishment of the act of generation, while the females, coming into contact with the various incidents which beset them in the accomplishment of their proper functions, undergo modifying effects from this circumstance. M. Humbert made the remark that there are certain groups of crustaceans in which, on the contrary, it is the males which vary, the females remaining the same, a fact which pertains probably to the different part which the males play in the struggle for existence. Dr. Muller adverted to a circumstance of the same nature in regard to the male and female organs of flowers. M. Edouard Pictet, our new colleague, presented to the society (January 4, 1866) his *Synopsis of the Neuropteræ of Spain.*

Personnel.—We have the pain of recording the loss of three of our colleagues—that of M. Perrot, member *emeritus;* of M. Frederick Soret, member in ordinary; and of Dr. Montague, honorary member.

Louis Perrot was born June 30, 1785, of a French family which had settled at Neuchâtel at the epoch of the Reformation. His taste for natural history was early developed by reading the *Spectacle de la Nature* of the Abbé Pluche, a book which, fifty years before, had produced so vivid an impression on the celebrated Bonnet.

So well did Perrot employ his time that, at the age of eighteen, when he came to Geneva, he was welcomed and encouraged by most of the distinguished savants who were there, and especially by the blind philosopher, the venerable Huber. He had the advantage in 1807 of accompanying de Candolle in a botanical exploration of the south of France and of the Pyrenees, and afterwards resided for some months in Paris, where the kind offices of his illustrious fellow-traveller placed him in amicable relations with the savants of that capital. Returning to Neuchâtel at a later period, he became engaged in new pursuits: elected in 1816 a deputy of the grand council of the city, (called the forty,) he formed a part of several committees of public utility, and was named president of that of education. Thus brought into closer communication with men of that specialty, both at home and abroad, such as Pestalozzi and Father Girard, he took upon himself the direction of one of the schools of Neuchâtel, in order to acquaint himself practically with the advantages of the system of mutual instruction, and to qualify teachers for the pursuit of that method. Botany was not the only branch of the natural sciences which he cultivated; his researches on the fishes of the lakes of Neuchâtel and Geneva, as well as of the Mediterranean in the vicinity of Nice, were of great interest; nor less so, those on the habits of a great number of insects, particularly bees, different kinds of wasps, and hornets. Yet he published nothing, contenting himself, when he had finished a memoir, with communicating it to his learned friends, to whom also he has bequeathed his herbals and other preparations. Yet, however modest and severe in regard to his own discoveries, it is probable that many of the observations of Perrot would have been entirely new had he published them at the time when they were made. We will cite but one, which, if confirmed, cannot fail to be of real interest. This relates to the *Dytiscus*, (Ditisque,) its larva, and its nymph. After describing the curious position which the latter adopts in the cavity which it digs in the clay, he describes the very singular effects produced when this nymph is held in the hand, and which consisted of all the physiological symptoms attending discharges of galvanism. After taking the proper precautions and repeating the experiment on persons not forewarned, he satisfied himself that, in this case, an electrical phenomenon really takes place. In 1830 he returned to reside permanently at Geneva, devoting the last years of his life to acts of charity and religion. And, assuredly, he was not now in his apprenticeship; for as early as 1817 he had actively engaged in the organization of the succor extended to the poor, when a dearth was prevailing at Neuchâtel and in some parts of Savoy, and, for several weeks, made experiments upon himself to ascertain the quantity of nourishment strictly necessary for the maintenance of health and the physical forces. He was twice married; first, to a descendant of the celebrated mechanician Droz; and, secondly, to M'lle Rosalie de Pourtalès, by whom he had four children, the youngest of whom, our colleague, M. Adolphe Perrot, much to the satisfaction of his father, has devoted himself to the cultivation of physics. M. Perrot, having reached the advanced age of nearly eighty-four years, died somewhat suddenly June 9, 1866.

Frederick Jacob Soret, was born May 13, 1795, at Saint Petersburg. His father, a distinguished painter in enamel, had established himself in that city, where he enjoyed a high degree of the imperial favor, but was induced, from considerations of health, to return in 1800 to his native home, Geneva. His eldest son, our late colleague, early evinced a love of study and a quick intelligence; and, though destined for the church, gave himself with zeal to the natural sciences, particularly mineralogy and geology. In 1816 he read to the society of naturalists a memoir on the molasse of the environs of Geneva, in which, in contradiction to the views of Saussure, who held these formations to be marine, he was the first to announce the existence of fresh water fossils. At the termination of his theological studies, he composed a thesis, whose subject marked his taste for natural history, and was sustained by an exegesis not less enlightened than rational. He maintained therein that the six days of creation were not periods of twenty-four hours, but eras of great length. These views, which were then new, though now generally admitted, incurred violent opposition and much reproach on the part of some of the examiners. This circumstance and his repugnance for the heated religious controversies of that epoch, led him to renounce the sacred ministry and devote himself wholly to scientific studies. In the autumn of 1819, he repaired to Paris and was received with warm interest by Brongniart, Bournon, Haüy, and Biot. At this epoch he produced a work of great merit on "the relations of the form of crystals to their optical properties," and on his return to Geneva published new memoirs on mineralogy and optics in the *Bibliothéque Universelle*, the *Mem. de la Soc. de Phys. et d'Hist. Naturelle*, and the *Bulletin de la Sociétè Philomathique*. About 1822, his scientific career was somewhat interrupted by his being chosen to direct the education of the hereditary grand duke of Saxe-Weimar. It was at Weimar that he enjoyed the advantage of meeting the illustrious Gœthe, between whom and himself relations of intimacy were soon established. Gœthe was not merely a great poet; the natural sciences, and especially botany, were among his favorite studies. He had thus learned to place a just value on the savants whom our city so highly prized, and when I visited him in 1819 was never weary of extolling the labors of de Candolle and Vaucher. The arrival of the young and zealous naturalist of Geneva could not fail to confirm him in these favorable sentiments, and as he had just published a memoir on the *Metamorphosis of plants*, it was Soret who was charged with making a French translation of it. It will readily be conceived that, in an artistic and literary centre so brilliant as the court of Weimar, the decided taste of our compatriot for poetry and the fine arts received great development. The journals of the time and place contain, accordingly, a multitude of his productions in prose and verse, which attest his success as a writer and poet. Several excursions which he made into Russia and Italy with the prince, his pupil, confirmed these tendencies, and also led him to the study of archeology and numismatics, which he prosecuted with characteristic ardor to the end of his life. Hence we have more than thirty dissertations of his, not only on the coins of Geneva and its environs, but more especially on those of the east, Byzantine, Caliphat, Sossanide, Cufic, &c., published in the *Memoires de la Société d'Histoire et d'Archéologie de Genève*, the *Revues Numismatiques Belge et Française*, the *Revue Archéologique*, and the *Memoires de la Société Imperiale d'Archéologie de Saint Petersbourg*. In this branch of learning he soon acquired a high reputation, and the study of Arabic, which extended even to the publication of a dictionary of that language, contributed to the accuracy of his judgment on the origin and value of oriental coins, the classing and legends of which had been previously so obscure. More than twenty learned societies of foreign countries felt a just pride in numbering him among their associates; but however numerous and flattering these distinctions, they made no impression on the modest and unassuming nature of Soret. (It would be beside the scope of this publication to give a detailed account of the various public offices in which he devoted his talents, experience, and admirable judgment to the service of his country.) In 1836 he had married M'lle Elisa Bertheau, and an only daughter, the worthy offspring of this union, contributed a charm and solace to their mutual existence. The health of our regretted colleague had been good to quite an advanced age, and had permitted him to enjoy in his retreat a life exempt from disquietude and devoted to study; but his last years were overclouded at times by obstinate rheumatic neuralgia, without prejudice, however, to the serene equality of his character, or the vivacity of his intelligence and tastes. Shortly after a visit from his old pupil, the grand-duke of Weimar, an accidental exposure to cold brought on pneumonia, to which malady he succumbed December 8, 1865.

Jean François Montague was born February 15, 1784, at Vaudoy, department of the Seine-et-Marne. At the age of fourteen he took part in the expedition to Egypt as assistant helmsman, and his precocious intelligence as well as his amiability of character soon procured him protectors and friends among the French savants who composed the Institute of Egypt. This circumstance decided his tastes and vocation. At his return to France he applied himself with zeal to medical studies, and in 1809 was nominated chief physician to the Neapolitan army, under the government of Murat. Thenceforth he joined to his public functions the study of the natural sciences, and particularly of botany. Cryptogamy, and especially the cellular plants, till then much neglected, offered to him a vast field of exploration. Frequent voyages and a widely extended correspondence enabled him to publish in the *Annales de Botanique* and the *Annales des Sciences Naturelles* a great number of memoirs and articles on the cellular cryptogams, the lichens, the champignons, the algæ, &c., of Europe, America, and Oceanica. He made also a remarkable application of his researches to the maladies of the silk-worm, the vine and the potato. In 1842 he was nominated an honorary member of our society, and in 1852 was elected a member of the Academy of Sciences at Paris, in place of Professor Achille Richard. An indefatigable and conscientious laborer, Dr. Montague was one of those rare personages who have preserved to extreme age all their mental vigor, and who have honored humanity alike by the qualities of the intellect and the heart. He died at Paris, January 5, 1866.

Elections.—The precarious health of Professor Macaire, not permitting him to attend our sessions regularly, had led him to decline the place of member in ordinary, but his colleagues, not wishing to be deprived of the benefits of his learning and experience, and remembering his distinguished services to the society, have prevailed on him to preserve the title of member *emeritus.* M. Edouard Pictet has been elected member in ordinary, and MM. Bourcart, Laharpe, and Audeoud, as associates at large, so that the number of our *emeriti* is now two, that of members in ordinary forty, that of honorary members sixty-four, and that of associates at large thirty-nine. In the session of January, Professor Favre was proclaimed vice-president, and subsequently president for the ensuing year; M. Philippe Plantamour was named treasurer for a year, replacing M. Favre; Professor Marignac has been re-elected for three years as secretary charged with the correspondence; lastly, MM. Wartmann and Humbert have been appointed members of the committee of publication.

Permit me, in concluding, to recall the kind and delicate sentiments which I cannot but presume to have dictated my nomination to the presidency. Assuredly, I cannot attribute the honor conferred on me to any personal merit of mine, either as physicist or naturalist; neither could my medical specialty have been an adequate title; it is to be placed chiefly to the account of your good will in my behalf; but it must be regarded, also, as an acknowledgment which you desired to render to the memory of my venerable father, who, after having been one of the founders of the Society of Physics and Natural History, was one of the principal promoters of the Helvetic Society of the Natural Sciences, encouraged as he was by the support of his honorable colleagues of Geneva. It was, in effect, by the aid of the members of our society and of the honored pastor, Wyttenbach, of Berne, that he succeeded in constituting, in 1815, the nomadic and fraternal association of the Swiss naturalists, which has subsequently served as a model for those international scientific congresses which have drawn more closely the bonds of union among the savants of Europe. The year 1865 was the fiftieth anniversary of the foundation, and by your presence at Mornex, in the cradle of the Society, you have given a new sanction to that commemorative festival.

It only remains for me to thank you, my cherished and highly esteemed colleagues, for the zealous co-operation which you have never ceased to accord to me as president, a co-operation which has rendered my task as easy as it was agreeable, and to express my ardent hope that our society may continue to be distinguished by its scientific zeal and the inappreciable harmony which has always prevailed among its members.

NOTES ON THE TINNEH OR CHEPEWYAN INDIANS OF BRITISH AND RUSSIAN AMERICA.

1. The *Eastern Tinneh*, from a MS., by BERNARD R. ROSS, Esq., honorable Hudson's Bay Company.

2. The *Loucheux Indians*, by WILLIAM L. HARDISTY, Esq., honorable Hudson's Bay Company.

3. The *Kutchin tribes*, by STRACHAN JONES, Esq., honorable Hudson's Bay Company.

COMMUNICATED BY GEORGE GIBBS.

THE above tribes are embraced in the *Athabascan* group in Mr. Gallatin's classification of the Indian tribes, (Trans. Am. Eth. Soc., vol. ii,) but that name, according to Mr. Ross, is a foreign word, applicable only to a particular locality. The name of *Chepewyan*, given to the eastern tribes by most of the early writers, is merely a compound Cree word relating to dress. Sir John Richardson (Boat Voyage through Rupert's Land) adopts, and upon a more correct and philosophical principle, the name *Tin-neh*, which Mr. Ross says, though given in the vocabularies for "man," means rather "the people." It seems to be the appellation which each tribe applies to itself, other branches being distinguished by a prefix relating to locality, or some peculiarity of dress or appearance. Thus, while the Chepewyans call themselves Tin-neh, they call the "Slaves" *Tess-cho-tin-neh*, or the people of the Great river, (Mackenzie's.)

This family, the most northern in America excepting the Eskimo, is, at the same time, the most widely distributed, its range extending from the shores of Hudson's bay to the Pacific, where it is represented on Cook's inlet by the Kenai and other allied tribes. Several tribes, known collectively as the Tahkali, Tácully, or Carriers, inhabit the upper waters of Fraser river, extending south to Fort Alexandria, in about latitude 52° 30′. Near the mouth of the Columbia two small bands, now nearly extinct, inhabited the wooded country on either side of the river, and others are located on the Umpqua, Rogue river, and the coast of southern Oregon, and on the Trinity or south fork of the Klamath, in northern California. Finally, the same race, as shown by affinity of language, appears in Arizona, New Mexico, and Chihuahua, under the names of Navajoes and Apaches. The papers mentioned at the head of this article, and which follow, all refer to the northern branches, but do not include those of the Pacific coast.

Mr. Ross divides the northern portion of this great family into—

I. The eastern or Tinneh tribes proper.

II. The mountain tribes.

III. The western, consisting, so far as British America is concerned, of the Tahkalis.

IV. The northern, including all the Kutchin or Loucheux tribes.

It is to the first of these that the portion of his own notes here given refers. Mr. Hardisty's and Mr. Jones's relate to the last.

1.—THE EASTERN TINNEH.—*Ross.*

PHYSICAL CHARACTER.

The eastern Tinneh are of middle stature, squarely and strongly built. Although tall men are not uncommon in some of the tribes, the extremes in either direction are far from numerous. The lowest adult whom I have seen was four feet four inches in height, and the tallest, six feet six inches, the former a Slave, and the latter a Yellow Knife. As a whole they are tolerably fleshy, and their weight may be averaged at 140 pounds. The crania of these people are very large, with a tolerably good facial angle, the forehead rather high, and the skull elongated towards the occiput in most cases. The females appear to have the largest heads, and those of both sexes are covered with a matted profusion of black, coarse, and straight hair. They are, generally, long-bodied, with short, stout limbs, but without any disproportion between the lengths of the upper and lower ones. The extremities are small and well-formed, the hands thick, with short, tapering fingers, offering a strong contrast to the narrow, long, and bony hands of the Crees, and resembling a good deal in this particular the Eskimo of the Arctic circle. The most distinguishing feature in the race is the breadth of their faces between the cheek bones; this, with a high and rather narrow forehead and elongated chin, gives them a pear-like appearance. They are possessed of considerable bodily strength, of which, as the Hudson's Bay Company employ them as boatmen, there are excellent opportunities of judging. They can carry 200 pounds, in a strap passed over the forehead, without difficulty, but they are, as a whole, considerably under the average of the European servants in endurance and strength.

There is no particular cast of features other than the large and high cheek bones. Large mouths are universal; the teeth are white and regular, even to old age; the chins are commonly pointed, but cleft ones are not unusual among the Yellow Knives; the usual description of noses are the snub and bottle, with a slight sprinkling of aquiline; the ears, generally large, are placed well up towards the crown of the head; sparse mustaches and beard are sometimes seen, but whiskers are unknown; the eyes are mostly of a very dark brown hazel, varied with lighter, but never clear tints of the same color, and with black; they are often placed obliquely in the head, and although there is no general rule in the case, I think this form is oftener met with among the northern than among the more southern tribes. The prevailing complexion may, with propriety, be said to be of a dirty yellowish ochre tinge, ranging from a smoky brown to a tint as fair as that of many half caste Europeans. The color of the skin is, in all cases, opaque, and its texture close and smooth. In a few instances I have seen the blood through the cheeks, giving a vermilion color to that part of the face. Cases of corpulency, though the rule in childhood, are very rare in old age. The women, if anything, are uglier than the men; of smaller stature, and in old age become positively hideous. The mammæ become pendulous and large, though they never, to my knowledge, attain the almost fabulous dimensions that I have heard are not uncommon among the Carrier women.

Nature certainly does more than art for the rearing of the children of these people. "God tempers the wind to the shorn lamb," and causes them to thrive under numerous disadvantages. Immediately after birth, without washing, the infant is laid naked on a layer of moss in a bag made of leather, and lined with hare skins. If it be summer, the latter are dispensed with. This bag is then securely laced, restraining the limbs in natural positions, and leaving the child freedom to move the head only. In this phase of its existence, it resembles strongly an Egyptian mummy. Cradles are never used; but this machine, called a "moss bag," is an excellent adjunct to the rearing of children up to a certain age, and has become almost, if not universally, adopted in the families of the

Hudson's Bay Company's employés. The natives retain the use of the bag to a late period, say until the child passes a year, during which time it is never taken out except to change the moss. To this practice, continued to such an age, I attribute the turned in toes and rather crooked legs of many of these Indians. A child is not weaned until another takes its place, if the mother has milk to give it, and it is no unusual thing for an Indian woman of these tribes to suckle a child three or four years old, even with a baby at her other breast at the time. Respecting the food of infants, the routine is as follows: If the mother has milk they suck so long as she yields it; otherwise, mashed fish, chewed dried meat, or any other nutritious substance that can be had from a not very extended variety is given. A curious and superstitious custom obtains among the Slave, Hare, and Dogrib tribes, of not cutting the nails of female infants till they are four years of age. Their reason for this is, that if they did so earlier the child would, when arrived at womanhood, turn out lazy, and be unable to embroider well in porcupine quill-work, an art which these Indians are very skilful in, and are justly proud of. Another extraordinary practice is their giving no nutriment to infants for the first four days after birth, in order, as they say, to render them capable of enduring starvation in after life, an accomplishment which they are very likely to stand often in need of.

It is difficult to determine exactly the age of puberty. In boys it commences about twelve. Indeed, they endeavor, as soon as they can, to pay their addresses to the sex, and marry, generally, at from sixteen to twenty years of age. To fix the period for girls is still more difficult. They marry sometimes, but not often, at ten, and have their menses about thirteen. The women are capable of bearing children from fourteen to forty-five, a long portion of their lives, but in it very few infants are produced. Families on an average contain three children, including deaths, and ten is the greatest number I have seen. In that instance the natives found it so unusual that they called the father "Hon-nen-na-bé-ta," or the Father of Ten. Twins I have heard of but once. The proportion of births is rather in favor of females, a natural necessity, as it is the women among these tribes who have the shortest lease of life, and there is from various causes a much greater mortality among the girls than among the boys. The period of utero-gestation is rather shorter than in Europeans, and seldom exceeds the nine months. Premature deliveries are very rare, and the women experience but little pain in child-birth, a few hours repose, after the occurrence, being sufficient to restore nature.

The duration of life is, on an average, short. Many children die at an early age, and there are few instances of the great longevity that occurs not unfrequently in more temperate climates. Rarely does one of the Tinneh reach the "three score years and ten" allotted to man, though an instance or two of passing this age has occurred within my own knowledge. A Slave woman died at Fort Simpson, in the autumn of 1861, who had already borne three children when Sir Alexander McKenzie, in 1789, descended the river bearing his name. Supposing that she had married at sixteen, and was confined once every three years, a high average for this people, she would have been ninety-seven years of age at the time of her death. For some years prior to her demise she was perfectly bed-ridden, and sadly neglected by her relatives, who evidently fancied that she had troubled them long enough. She lay solitary and forsaken in a miserable camp, composed of a rude shelter and bed of pine brush, her only covering a tattered caribou-skin robe. Such was the malignity of her disposition, even in "articulo mortis," that she reviled at nearly every adult, and struck with a stick at all the children and dogs that passed by her den.

The Tinneh are far from a healthy race. The causes of death proceed rather from weakness of constitution and hereditary taint than from epidemic diseases, though, when the latter do come, they make great havoc. Want of proper and regular nutriment and exposure in childhood in all probability undermine their constitutions before they come of age. The most prevalent maladies are influenzas, coughs, bilious affections, dysentery, and indigestion, brought on by gluttony. Scrofulous cases are not uncommon, and all the tribes are more or less subject to a pseudo-syphilis of great virulence, and which is, so far as I can learn, indigenous. Ophthalmic affections are very common, chiefly among the Athabascan and English River Chepewyans. They probably have their origin in syphilis. There are a few instances of total blindness, produced by the snow glare on the great lakes in spring. Lice literally overrun all the natives. Fleas are unknown. The former insects are eaten as a species of relish, and are cracked in the teeth and nibbled, in order the better to enjoy the flavor, which the Indians represent as sweet. The tapeworm (tænia) is rather common. Like all hunter tribes these people have the senses of sight and hearing in perfection, while, owing to the dirtiness of their habits, that of smell is greatly blunted.

RELIGIOUS OPINIONS.

It is a task of no ordinary difficulty to arrive at correct conclusions respecting the mental characteristics and religious ideas of the eastern Tinneh. They are exceedingly averse to laying open their belief, such as it is, to strangers, and their real disposition is exhibited only in the camp, amidst the freedom of social intercourse. Deprived as I am of reference to the works of McKenzie and Hearne, I must, unaided by any gleams thrown on the subject from the past, describe things as they exist, under the light of the present.

These people seem to possess as cold and simple a theology as any known race of mankind. I am not, however, certain that such was the case seventy years ago. Many causes, all of which must have had more or less power, have combined to wean them from the faith of their ancestors. They are great imitators and respecters of more civilized races, and, so far as I can judge of their idiosyncracy, would have been very likely to cast aside their old ideas and superstitions, if ridiculed by the whites, who, being fur traders and not missionaries, were far less likely to impart to them the Christian truths instead. They would thus have gradually and imperceptibly moved downwards to the condition of having no religion whatsoever.

It is now many years since the Roman Catholic priests first instructed the Beavers, Cariboo Eaters, Chepewyans, and Yellow Knives; and although it is only four years since the Slave communities came under the direct influence of the gospel, still, from intercourse with the others, their superstitions had, in a good measure, either faded away or been imbued with a considerable quantity of the ideas derived from the sacred writ. When the Christian religion spreads, as it certainly will in a very short time, among the eastern, northern, and mountain Tinneh, their former faith will become a dream, and all traces of its existence be lost to the inquiring ethnologist. No heathen people, in my opinion, offer an easier field to the enterprise of missionaries. Their teaching will meet with but little opposition from the theological system or superstitions of the natives, and although I have great doubts if many will become sincere Christians at heart, they will at least submit willingly to the outward semblance of religion and conform to its ceremonies in a highly plausible manner. Their knowledge of a First Great Cause, the Maker and Ruler of the Universe, is very faint, yet I think it has always existed; but as they have no idea of a future state of rewards and punishments, this credence, if they possess it, exercises neither power nor control over their actions, and appears to be of about as much use in their mythological system as the Great Mogul was in modern times to the government of Hindoostan. Their religion is one of fear. They deprecate the wrath of demons, but no abstract notion of a single evil principle, antagonistic to and at war with the good one, appears to exist among them. The demons are, among the unsophisticated and unchristianized natives, many in number. They people the woods and streams, haunt desert and lonely localities, and moan among the caches of the dead. To propitiate these spirits, offerings are made of some trifling and invariably very worthless article. This is hung upon a bush or tree, and among the tributes of this kind, which I have seen, may be mentioned strips of cotton, worn-out shoes, tattered robes, pieces of leather, and old belts, whose perfectly worthless character showed plainly that though these Indians have a sneaking, superstitious fear, it is not sufficiently strong to overcome the avarice that forms so predominant a trait in their character.

An inferior species of "totemism" obtains among them. Each hunter selects, as a species of familiar spirit, some animal, and invariably a carnivorous one. According to their custom, the man can then neither eat nor skin, and if avoidable, not even kill the object of his choice. The taking of the "totem" is not, so far as I am aware, the occasion of any religious ceremony, as is the case among some of the plain tribes. Pictures of various animals used in the olden day to be distributed among the natives by the traders, each individual receiving that of his totem. When a hunter had been unsuccessful he pulled this picture out of his medicine bag, laid it before him, and taking some tobacco from the same receptacle, paid adoration to the spirit by smoking and making it a speech. After this proceeding he returned with renewed ardor to the chase, and generally with success.

Fatalism appears to be deeply seated in their minds. They usually accept such luck as is sent them, if not without murmuring, at least apathetically, and make but few struggles to combat adverse circumstances.

There does not appear to be any regular order of priesthood. Any one who feels inclined to do so turns medicine man, but some are much more highly esteemed than others, as possessing greater skill in conjuring away sickness and foretelling future events. The articles by which they affect to perform many remarkable and mysterious operations are very commonplace and trifling; a flint, a piece of mica, a colored stone, or a bullet, being all equally efficacious mediums, through which to hold communication with their tutelary spirits. I have on several occasions, for amusement, tested the soothsaying powers of some of the most celebrated wizards, by requesting information as to the future arrival of boats or letters, and I can confidently state that if they guess correctly once in twenty times, it is as much as their supernatural powers are capable of effecting. As jugglers they hold a very inferior status, and do not approach, even in a remote degree, the really remarkable skill that many of the Algonquin tribes possess in this way. An idea of the powers of conjurors to kill Indians at a distance, simply by the force of their spells, was formerly common to all the race, and still exists with unabated strength among the Kutchin tribes of the Youcon river, who put great faith yet in their medicine men, and pay them liberally for their services in seasons of danger or sickness. Additional facts regarding these "doctors" will be noted hereafter, when I proceed to explain the medical theories and practice of the nation.

MORAL AND INTELLECTUAL CHARACTERISTICS.

Few of the moral faculties are possessed in any remarkable degree by the eastern Tinneh. They are tolerably honest, not bloodthirsty nor cruel; but this is, I suppose, the extent, as they are confirmed liars, far from being chaste, and have but very indistinct perceptions of doing to others as they would be done by. Some tribes are more noted for honesty than others; the Beavers and Chepewyans being at the top of the scale, the Slaves in the middle, and the Hares, Dogribs, and Yellow Knives at the bottom. The two first-named branches will compete in this respect with any European nation. No people in the world are more tenacious of what they possess themselves, or more willing to restore the property of others. On giving up what they may find to the owner, a demand for payment will sometimes be made. If the request be granted, well and good.

If not, it will make no difference afterwards as to the rectitude of their conduct on a similar occasion. In the payment of their debts, also, they evince a much greater sense of justice than the other tribes. They seldom or never dispute their accounts if they be correct, and endeavor to liquidate them to the utmost of their power. The Slaves are tolerably honest, but have great objections to clear off old debts, giving for a reason that the articles purchased are already worn out. The remaining tribes cannot be said to have a very keen perception of the rights of property, and are apt to reverse Prudhomme's celebrated dogma, "la propriété c'est le vol," into "le vol c'est la propriété." Among all the branches of the eastern division, there is no law to punish theft further than restoration; or if that cannot be had, purloining in return an article of similar or greater value. They do not, however, in general, steal much among themselves. The taking of provisions from "caches" in times of scarcity is reckoned perfectly lawful, but only the direst extremity will cause them to plunder those of the Hudson's Bay Company.

In the fabrication of false reports, and in the utterance of lies to serve their own interests, they are great adepts. The former is generally done from a wish to "cram," and is often rather ludicrous, but in the latter they evince a complete disregard for truth, and never appear in the least degree ashamed when taxed with it. There appears, indeed, to be a strong natural proneness to exaggeration in the minds of the eastern Tinneh, and a warped bias towards falsehood, even when a correct statement would equally serve their purpose. The smallest accident becomes in their narration magnified into truly horrific proportions, and on hearing of some terrible case of starvation or disaster from them, it is necessary to take it "grano salis," as I have on several occasions seen the murdered restored to life, and the starved to death jolly and fat.

As a whole, the race under consideration is unwarlike. The Chepewyans, Beavers, and Yellow Knives are much braver than the remaining tribes. I have never known, in my long residence among this people, of arms having been resorted to in conflict. In most cases their mode of personal combat is a species of wrestling, and consists in the opponents grasping each other's long hair. This is usually a very harmless way of settling disputes, as whoever is thrown loses; yet instances have occurred of necks having been dislocated in the tussle. Knives are almost invariably laid aside previous to the contest. Some of the Chepewyans box tolerably well, but this method of fighting does not seem to be generally approved of, nor is it much practiced. On examination of the subject closely, I am disposed to consider that this peaceful disposition proceeds more from timidity than from any actual disinclination to shed blood. These Indians, whether in want or not, will take the life of any animal, however useless to them, if they be able to do so, and that they can on occasion be sufficiently treacherous and cruel is evinced by the massacre at St. John's, on Peace river, and at Fort Nelson, on the Liard river. It may not be out of place here to give a brief account of the latter catastrophe:

In 1811 the post of Fort Nelson, on the Liard river, was in charge of a Mr. Henry, a well educated and clever man, but of a hasty temper and morose disposition. While equipping the Indians in the autumn, he had a violent dispute with one of the principal chiefs of the Bastard Beaver Indians resorting to the establishment, who departed greatly enraged and muttering suppressed threats, which were little thought of at the time. In the winter a "courier" arrived at the fort to inform the whites that there were the carcasses of several moose deer lying at the camp ready to be hauled, and requested dog sleds to be sent for that purpose. Mr. Henry, never in the least suspecting any treachery, immediately despatched all the men and dogs that he could muster. On their way out they met an Indian, who told them that they had better turn back, as the wolverines had eaten all the meat. This information, as it turned out, was given from a friendly motive; but fear of ulterior consequences to himself prevented the man from speaking more plainly. The fort interpreter, who was of the party and who all along suspected something more than appeared upon the surface, took the precaution to carry his gun with him, and when they drew near to the path which led from the bed of the river to the top of the bank where the Indians were encamped he lingered a little behind. On the others mounting the ascent they were simultaneously shot down, at one discharge, by the natives who were in ambush awaiting them. When the interpreter heard the shots he was convinced of foul play; he therefore turned and made for the fort as quickly as he could, pursued by the whole party of savages, whose aim was to prevent him from alarming the establishment. The man was a famous runner, and despite the disadvantage of small tripping snow-shoes, which permitted him to sink more deeply than the Indians, who, on their large hunting snow-shoes, almost skimmed over the surface of the snow, he would have reached the houses before them had not the line that confined the show-shoe on his foot broken. His enemies were too closely upon him to allow time for its repair, so, wishing to sell his life as dearly as possible, he levelled his gun at the nearest Indian, who evaded the shot by falling upon his face, whereupon the whole party made up and despatched him. After perpetrating this additional murder the band proceeded to the fort, which they reached at early dawn. A poor old Canadian was, without suspicion of evil, cutting fire-wood at the back gate. His brains were dashed out with their axes, and they entered the establishment, whose inhabitants, consisting, with one exception, of women and children, were buried in profound repose. They first opened Mr. Henry's room where he was asleep. The chief pushed him with the end of his gun to awaken him. He did so, and seeing numerous fiendish and stern faces around him, made a spring to reach a pair of pistols that were hanging over his head; but before he could grasp them, he fell a bleeding corpse on the bosom of his wife, who, in turn, became a helpless victim of the sanguinary and lustful revenge of the infuriated savages. Maddened by the blood, and demons in heart and act, they next proceeded to wreak their vengeance on the innocent women and children, who expired in agonies and under treatment too horrible to relate. The pillage of the stores was the next step, after which they departed, leaving the bodies of the dead unburied. No measures further than the abandonment of the fort for several years were taken by the Northwest Company, to whom the establishment belonged, to punish the perpetrators of the atrocious deed, yet it is a curious fact that when I visited Fort Liards in 1849, but one of the actors survived, all the others having met with violent deaths, either by accident or at the hands of other Indians. This man, who was at the time only a lad, confessed to have dashed the brains out of an infant, taking it by the heels and swinging it against the walls of the house.

The fear of enemies, when in these peaceful times there are none to dread, is a remarkable trait of the timidity which so strongly influences the minds of the eastern Tinneh. It is, I conjecture, a traditional recollection of the days when the Knisteneaux or Crees made annual forays into the country of the Tinneh, pushing so far as Bear river in search of scalps and plunder, when the Yellow Knives bullied the Slaves and Dogribs, and the Beavers warred with the Sickanies. A strange footprint, or any unusual sound in the forest, is quite sufficient to cause great excitement in the camp. At Fort Resolution I have on several occasions caused all the natives encamped around to flock for protection into the fort during the night by simply whistling, hidden in the bushes. My train of hauling dogs also, of a large breed and great hunters, would, in crashing through the branches in pursuit of an unfortunate hare, frighten some women out gathering berries, who would rush in frantic haste to the tents and fearfully relate a horrific account of some strange painted Indians whom *they had seen.* It was my custom in the spring, during the wild fowl season, to sleep outside at some distance from the fort. Numerous were the cautions that I received from the natives of my foolhardiness in doing so, and when they found that I escaped with impunity, they accounted for the circumstance to their own satisfaction by saying that I had bribed the "bad Indians to leave me alone."

The race under consideration must be regarded as far from chaste, as continence in an unmarried female is scarcely considered a virtue, and its want brings no discredit on the individual. The intercourse between the sexes begins very soon. This is easily accounted for by their hearing and seeing so much that they should not at a very early age, which ripens their instincts at an earlier period than either their temperament or the climate of the country would warrant. Their dispositions are not amatory, and, in the case of the females, the love of gain is a much stronger incitement to immorality than any natural warmth of constitution. The divine and customary barriers between blood relations are not well observed, for, although it is not considered correct by general opinion, instances of men united to their mothers, their sisters, or their daughters, though not common, are far from rare. I have heard among them of two sons keeping their mother as a common wife, of another wedded to his daughter, and of several married to their sisters, while in cases of polygamy having two sisters to wife is very usual. The married state, easily entered upon and involving few duties and responsibilities, is but a slender guarantee for the mutual faithfulness of the sexes. A Tinneh woman, however obedient she may be to her taskmaster as regards labor, considers herself quite at liberty to dispose of her personal favors as she may wish, which latitude is not at all agreed to by her husband, who, while claiming and exercising quite as much freedom for himself, severely punishes his wife if she forgets in a single instance the allegiance due, in his opinion, to him alone. The custom of robbing one another of their wives, or of fighting for them, the facilities for divorce, and the inferior estimation in which women are held, combine to produce a very lax condition of the marriage ties, and to originate a low state of morality, which will doubtless improve gradually as the operating causes are neutralized or done away with by the exertions of missionaries and advance of Christianity.

The instinct of love of offspring, common to the lower animals, exists strongly among these people, but considerably modified by the selfishness which is so conspicuous a feature in their character. In sickness they appear to sympathize strongly and to take great interest in the sufferer, so far as lamenting and crying goes; but their affection is seldom strong enough to induce them to do anything that would either tax their comforts much or require great exertion. On arriving at mature age the bond between relatives is easily broken, and even in adolescence often but scanty deference is paid to parents. The parental instinct, though far more strongly developed in the mother than in the father, would, I am confident, never call forth such traits of self-sacrifice, even to death, as have been exhibited many times among civilized and even barbarous nations. Male children are invariably more cherished and cared for than females. The latter are mere drudges, and obliged on all occasions to concede to their brothers; and though female infanticide, formerly so prevalent, is now unknown, still in seasons of starvation or times of danger, girls invariably fall the first sacrifices to the exigencies of the case. The death of a child is apparently not much regretted, the mourning is short, and although in after years a mother will lament her offspring bitterly, there is far more of custom than reality in the exhibition, and it rarely proceeds from the heart. The relation on the part of the children is still more soulless. Only in early age do they pay much attention to the commands of their parents, and the control of the latter is soon loosened. A curious circumstance is, that children are treated exactly as grown-up people, and talked to as such; but as the character of all ages is decidedly childish, it is not to be wondered at if such a manner suits all parties equally well.

As these people are obliged to lead a very wandering life, in order to procure food either by fishing or hunting, there can be, and in fact is, but little or no attachment to particular localities existing in their minds, though they have a strong bias towards their mode of life. The latter sentiment does not retain nearly so strong a hold on their dispositions as it does on most savage nations. Wedded to ancient manners and customs by much more slender ties than exist in the generality of Indian tribes, they easily fall into the habits of Europeans, and, in cases of servants engaged from among them by the Hudson's Bay Company, willingly abandon the charms of freedom and the chase for the more regular comforts and daily avocations of civilized life. I judge from this that if these tribes were properly instructed and located in a more favorable climate, they would become tolerable husbandmen, and without acquiring the ferocity of their congeners, the Navajoes, soon surpass them in agricultural skill and herdsmanship.

2.—THE LOUCHEUX INDIANS.—*Hardisty.*

The physical characteristics of the Loucheux nation are, with few exceptions, the same as those of the other aborigines of North America. The skin is commonly of a sallow brown tint, in some cases what might be called a yellowish white; the hair is long, black, and lank; the beard scanty, with rare exceptions. They have black deep-set eyes, receding foreheads, high cheek bones, high, aquiline noses and large mouths with tumid lips. The eyes are of a dark hazel color, often approaching to black, frequently small and oblique, though I have noticed particular individuals with very large eyes, while in others the eyes were remarkably small and these invariably oblique.

The Loucheux language is a dialect of the Chepewyan, which it more closely resembles than the intervening dialects of the Hare Indians and Slaves, although a very slight intercourse enables the latter also to understand the former sufficiently for the ordinary purposes of traffic. The Loucheux proper is spoken by the Indians of Peel's river, thence traversing the mountains westward down Rat river, the Tuk-kuth, (Rat Indians,) and Van-tah-koo-chin, it extends to the Tran-jik-koo-chin, Na-tsik-koo-chin, and Koo-cha-koo-chin of the Youcon. All the tribes inhabiting the valley of the Youcon understand one another; a slight difference of accent being all that is perceptible in their respective dialects. The first material change occurs among the "Gens de Fou" or Hun-koo-chin, (river people.) These make use of a great many words in common with the "Gens de Bois," who again understand the language of the "Mauvais Monde" of Francis lake, which is the common language of the Mauvais Monde of Fort Halkett, the Thikanies, the Ah-bah-to-din-ne (mountain Indians) and Nahaunies of Forts Liard and Simpson.

The Loucheux, though sunk in barbarism, are rather more intelligent than the other tribes composing the great Chepewyan nation, owing no doubt to their intellectual faculties being more frequently brought into active play in their traffic and intercourse with other tribes. They are essentially a commercial people, and live by barter, supplying their wants by exchanging their beads, which form the circulating medium, for the peltries of the neighboring tribes, to whom they go on periodical trading visits. They hunt no furs, but are, nevertheless, good hunters, and invariably well supplied with provisions, unless when some very unfavorable circumstances may have occurred to prevent success in the chase. They are great talkers and very fond of displaying their eloquence. They are always making public harangues, and in the figurative language they use, their speeches are not ineloquent nor void of sense. Their delivery is good, but the effect is spoiled by their gradually raising their voices to such a high pitch as to be compelled to stop before they come to the end of their speech from sheer want of breath. After a minute or two they begin again in a lower key, and gradually raising their voices as they proceed and get excited; they finally close their harangues with a most infernal screech, which is particularly disagreeable to a white man's ears.

The Loucheux generally live in large parties, each band headed by a chief and one or more medicine men. The latter, however, do not possess any secular power as chiefs, but they acquire an authority by shamanism to which even the chiefs themselves are subject.

All the chiefs, medicine men, and those who possess rank acquired by property have two, three, or more wives, so that only few of the young men have wives, unless they can content themselves with some old cast-off widow, who, from ill health and the effects of bad treatment, is no longer able to perform heavy work. The consequence is that those who have wives are invariably jealous, and treat their women most brutally. It is one of the principal causes of the great falling off of the Loucheux nation. They are not half the number they used to be. The other causes of the decrease in the population are female infanticide, and premature birth and very frequent miscarriages from over exertion, &c. Infanticide is caused by the misery of the women—at least, this is the only reason they give for it. When questioned on the subject, they invariably give the same answer, "that they love their children, and destroy them only to save them from the hardships and misery to which their mothers are exposed in this life." To preserve them alive is equivalent to the unnatural crime of a mother wilfully placing her daughter in misery. When a young man has acquired the means, he purchases a young girl (perhaps an infant) from its mother, who has the power to dispose of her daughter to whom she pleases, though no doubt she will sometimes consult the wishes of her husband. The fathers and brothers have no voice in the matter by the laws of the tribe. The females are fewer than the men, especially when young, and might be considered pretty, but they get proportionably coarse and ugly as they grow old, owing to hard labor and bad treatment. The very low position which they occupy in the social scale, is a sign of the depth to which the Loucheux are still sunk in barbarism. The women are literally beasts of burden to their lords and masters. All the heavy work is performed by them. When an animal is killed, they carry the meat and skin on their backs to the camp, after which they have the additional labor of dressing the skin, cutting up the meat and drying it. They are the drawers of wood and water; all the household duties devolve upon them; they have to keep up the fires, cook, &c., besides all the other work supposed to belong to the women, such as lacing the snow shoes for the family, making and mending their husband's and children's clothes, &c. In raising the camp, or travelling from one place to another, if, in winter, the woman hauls all the baggage, provisions, lodge poles, cooking utensils, with probably a couple of children on the top of all, besides an infant on the back, while the husband walks quietly on ahead with his gun, horn and shot-pouch, and empty hunting bag. In the summer the man uses a small light hunting canoe, requiring very little exertion to propel it through the water, while the poor woman is forced to struggle against the current in a large ill-made canoe, laden with all the baggage, straining every nerve to reach a particular place pointed out beforehand by her master as the intended camping ground.

They are a lively, pleasant race, and have many rules and regulations, which are strictly adhered to both in public and private life. Their games and pastimes are more manly and rational than those of the dull, apathetic Slaves. They are passionately fond of dancing, wrestling, running, &c., in all which sports the women, especially the younger, take a part. Their dances, which are accompanied by singing, are not void of harmony, as they keep time with their bodies, beating cadence with their feet, and moving themselves in grotesque though not unpleasant postures, which are apparently rather difficult to perform, as they perspire profusely. Their wrestling matches are commenced generally by two little boys. When one of them is thrown he retires and another, a little bigger, takes his place. As soon as he has thrown his opponent he rises up quickly and places himself in preparation for the next, who will make a sudden rush at him so as to get an advantageous hold before he is prepared and while still panting from his previous exertion. Still if he be the stronger or more expert, he may knock down his second adversary, also the third or perhaps a fourth before he is thrown, when he retires and leaves the field to his conqueror, who in his turn will continue to throw as many as he can, one after the other, until he, too, perhaps from exhaustion, is obliged to give way to a fresher or more vigorous opponent. The combatants rise in gradation until all the men have had their turn, and one, the last, remains alone on the ground with the honor of being the best wrestler of the tribe. Afterwards two little girls begin in their turn and so on until all the women also have been thrown, except one who remains to claim the approbation of her male friends. In winter time they have a most amusing, though rather unsafe, game to those who are unacquainted with it. Four trees are selected, forming as nearly as possible a square of about thirty feet, to which strong leather cords are tied diagonally as tight as possible, about twenty feet from the ground. Where the cords cross a piece of leather about eight inches square is tied securely, on which each in his turn is required to stand. The least pressure sends the person up in the air perhaps a couple of feet, when he comes down the second time on the piece of leather. The cords being suddenly distended with his weight, the "contre coup" will shoot him up perpendicularly in the air, perhaps a dozen feet. Now is the time of danger, for if he is not expert, or has not been able to keep himself straight, he may come down a height of perhaps twenty or thirty feet on his head to the ground. The object is to see who will fall oftenest perpendicularly on his feet on the little leather table without breaking his neck, I might say, or tumbling on one side to the ground to the amusement and uproarious laughter of the others.

They are hospitable, but more, I think, because it was a custom of their fathers than from real generosity. After the first day, during which a guest is served with the best they have, and welcome, he may remain for months with them without rising above the salt, as it were, unless indeed he be a chief, or a man of consequence—that is, one with plenty of beads, or more especially a medicine man, but even then only for a time. Avarice is certain to get the better of their fears in the end. Each head of a family is expected to, and does, act the host for the whole band in his turn, day about. Whether they do so in rotation or how it is managed I was never able to find out. Whether invitations are sent, or the fact of any particular person putting all the kettles in camp in request apprizes the others which is to be the general eating-room of the day, I cannot say. At all events, all the males, from the oldest to the youngest, are drawn as if by magic to the point of general attraction. They continue falling in until the lodge is crammed; the more the merrier; the greater the pressure, the better the host is pleased. The favored or principal guest sits on the host's right hand, the next on his left, and so on downwards to the fourth or fifth on each side of him. The sixth downward are considered to be below the salt. The next rule observed is to divide or carve the meat properly according to rule. The best and fattest pieces, the titbits, are piled in a heap before the principal guest, who, after he has satisfied his hunger, sends the rest to his own lodge for his wife and children. The person on his left hand gets the next best pieces and sends what he leaves to his family, and so on downwards to the salt, below which the meat is distributed as it comes, without selection. Every fowl, every animal and part of an animal, must be divided or carved in a particular way, and if any person evince ignorance or inexpertness it excites the laughter and ridicule of the rest. One may be a principal guest with one host, and yet sit fourth or fifth or even below the salt with another. All goes by relationship or the estimation in which the person is held by the particular host for the time being. The host himself does not eat on that day beyond taking one mouthful, tasting the meat before helping the head guest. Should he eat anything more, he would be considered very mean and ridiculed accordingly. He may get his wife to cook something for him after the guests have left, but not before, and it may be some time before they do leave, especially if there be anything to talk about, for after they have all eaten and drank, the host is obliged by rule to cut up tobacco and fill every pipe. The wife cuts the wood and cooks and collects all the pans. During the repast she sits at the door, if she can find room, and outside if not, to hand to her husband whatever he may ask for.

This apparent abnegation of self is perceptible through all their regulations. For instance, unless he is alone a hunter cannot take and appropriate the meat of the animal he kills. Should he do so he would be considered mean. And this feeling is so strong that I could not induce them to abolish the custom during the long time I remained among them, so much do they dread the idea of being thought mean with regard to anything eatable. When two *good* hunters go together, good and well—the one has as good a chance of getting meat as the other; but when one is a bad hunter and the other a good one the former gets all the meat and the real hunter has nothing, and loses his ammunition into the bargain.

Although hospitable to a certain extent as far as food is concerned, their natural character is selfish. (But where will you find an Indian who is not?) They would not part with half a dozen common beads for nought, and are keenly alive to the ridicule attached to a bad bargainer. They will harangue and protest for days against what they consider (all honor and honesty apart, of course) an inadequate payment for what they give. They will have recourse to every subterfuge, even intimidation, to have the best of a bargain, and will do all in their power to fleece their opponent, and boast about it afterwards.

The wife is expected to furnish the skins required for the clothing of the whole family, either by dressing the skins of the animals killed by her husband, or by purchase from others with *her own beads*—that is, her marriage portion, or what she may have had on her person or dress when she was married, and what she may have received from time to time from her husband for good conduct, or, probably, when he happened to be in an unusually good and generous humor. She supplies all the beads or wampum required for ornamenting the dresses of all the family, including her own and even her husband's. *His* beads are the family fortune, the capital which cannot be touched except for purposes of traffic or for payment of *doctor's bills*, &c.—that is, paying the medicine-man in time of sickness and for producing wind and favorable weather in times of scarcity. The first time a Loucheux saw a blacksmith's bellows he, of course, reported to his friends all particulars regarding the ironmaker's blowing machine. Some time after a medicine-man came to me secretly to inquire the truth, whether it would be possible for him to purchase, and the price of this wonderful wind-producing machine of the ironmaker's, and whether it could be turned to account in making wind for hunting moose in cold weather, for, being a medicine-man, he was expected to make wind when it was required, and if he could only get this wonderful wind-maker, which he had heard so much about, his reputation would be at its height, and his fortune made.

The Loucheux have a number of legendary stories, but generally of such an obscene character as not to merit mention here. Even the story regarding caste, or the regulation which divides mankind (the people, Loucheux) into three different grades, is of a filthy character. They believe the heavens to be a walled canopy encircling the world. There are people above this canopy who in former times used to visit the earth, and on several occasions carried off women with them to the celestial regions. The women, however, it seems, did not find this paradise such a place of bliss as to wish to remain there. They regretted the pleasures of this lower world of ours, and after a time hit upon the expedient of boring the heavenly canopy. Then secretly collecting all the cords they could find, they tied the end to a stone somewhat larger than the orifice, and by this means lowered themselves to the earth. The story goes on to say that the cord was just long enough, but, unfortunately, they found themselves over a lake, and might have been drowned after all had it not happened that an Indian was passing in a canoe at the time, and saved them from their perilous situation.

With reference to the story about caste it is difficult to arrive at a correct solution of the matter. The fact, I believe, is that they do not know themselves, for they give various accounts of the origin of the three great divisions of mankind. Some say it was so from the beginning; others that it originated when all fowls, animals, and fish were people—the fish were the *Chitsah*, the birds *Tain-gees-ah-tsah*, and the animals *Nat-singh;* some that it refers to the country occupied by the three great nations who are supposed to have composed the whole family of man; while the other, and, I think, most correct opinion, is that it refers to color, for the words are applicable. *Chitsah* refers to anything of a pale color—fair people; *Nat-singh,* from *ah-zingh,* black, dark—that is, dark people; *Tain-gees-ah-tsah,* neither fair nor dark, between the two, from *tain-gees,* the half, middle, and *ah-tsah,* brightish, from *tsa,* the sun, bright, glittering, shining, &c. Another thing, the country of the Na-tsik-koo-chin is called Nah-t'singh to this day, and it is the identical country which the Nat-singh occupied. The Na-tsik-koo-chin inhabit the high ridge of land between the Youcon and the Arctic sea. They live entirely on the flesh of the reindeer, and are very dark-skinned compared with the Chit-sangh, who live a good deal on fish. All the elderly men fish the salmon and salmon trout during the summer, while the young men hunt the moose, and have regular white-fish fisheries every autumn besides. Some of the Chit-sangh are very fair, indeed, in some instances approaching to white. The Tain-gees-ah-tsa live on salmon trout and moose meat, and, taken as a whole, are neither so fair as the Chit-sangh nor so dark as the Nah-t'singh. They are half-and-half between the two. A Chit-sangh cannot, by their rules, marry a Chit-sangh, although the rule is set at naught occasionally; but when it does take place the persons are ridiculed and laughed at. The man is said to have married his sister, even though she may be from another tribe and there be not the slightest connection by blood between them. The same way with the other two divisions. The children are of the same color as their mother. They receive caste from their mother; if a male Chit-sangh marry a Nah-tsingh woman the children are Nah-tsingh, and if a male Nah-tsingh marry a Chit-sangh woman the children are Chit-sangh, so that the divisions are always changing. As the fathers die out the country inhabited by the Chit-sangh becomes occupied by the Nah-tsingh, and so on vice versa. They are continually changing countries, as it were. Latterly, however, these rules are not so strictly observed or enforced as formerly, so that there is getting to be a complete amalgamation of the three great divisions, such a mixture that the difference of color is scarcely perceptible, and, no doubt, will soon disappear altogether, except what is produced by natural causes. The people who live on the flesh of the reindeer are always darker than those who live on fish, or on part fish and part flesh. One good thing proceeded from the above arrangement—it prevented war between two tribes who were naturally hostile. The ties or obligations of color or caste were stronger than those of blood or nationality. In war it was not tribe against tribe, but division against division, and as the children were never of the same caste as the father, the children would, of course, be against the father and the father against the children, part of one tribe against part of another, and part against itself, so that, as may be supposed, there would have been a pretty general confusion. This, however, was not likely to occur very often, as the worst of parents would have naturally preferred peace to war with his own children.

As a rule slavery does not exist among the Loucheux, but the orphan and the friendless are kept in servitude and treated so harshly as to be really little better than slaves, until such time as they get big enough and bold enough to assert their independence, when they are allowed to shift for themselves.

The Loucheux are very superstitious, and place implicit faith in the pretended incantations of their medicine-men, for whom they entertain great fear. When a death occurs they make loud expressions of grief. They accompany their lamentations with a song or dirge, in which they enumerate all the good qualities of the deceased, and when they have raised themselves to a fit of ungovernable fury and excitement, a medicine-man will adroitly and imperceptibly raise the idea that the person's death was caused by a medicine-man of a neighboring tribe, or if a disinterested person do so for him, so much the better, as it draws away suspicion from himself. On such occasions the relatives of the deceased will immediately take a quantity of beads to the conjuror, and entreat him to find out who the hidden enemy really is, and the particular reason of the death of their friend, so that they in their turn may know in what direction to turn the shaft of revenge. When a person of consequence is sick, he will frequently receive a visit of condolence from a medicine-man of a neighboring tribe. As a mark of respect for the stranger he is invariably employed to recover the sick person, being of course well paid in beads for his trouble, to the exclusion and great displeasure of the native "doctor," who is sure to find some means to be revenged on the intruder for the slight he has received, and the loss he has sustained. On such occasions there is frequently a row, after, if not before, the departure of the visitor, for his opponent will secretly endeavor to make the impression that some medicine-man, or perhaps the favored guest himself is the real enemy of the sick person. When these insinuations and stories begin to take effect, the guest seizes the first favorable opportunity to take his departure, for he has a sufficient knowledge of the Loucheux human nature to be aware that in moments of great grief or excitement the slightest whim or chance may direct the popular fury towards himself. If he gets off safe, he goes on his way rejoicing under a good load of beads and thinking good humoredly on the acquisition he has made to his wealth, and the power and influence it will give him among his own tribe, riches being the talisman with the Loucheux as well as others. The power of the medicine-men is very great, and they use every means they can to increase it by working on the fears and credulity of the people. Their influence exceeds even that of the chiefs. The power of the latter consists in the quantity of beads they possess—their wealth and the means it affords them to work ill to those to whom they may be evil-disposed; while the power of the medicine-man consists in the harm they believe he is able to do by shamanism, should they happen to displease him in any way. It is when sickness prevails that the conjuror rules supreme; it is then that he fills his bead bags and increases his riches. Some near relative of the invalid, or, as often happens, some other person, to court popularity, will give him a quantity of beads to save the sick person or to ascertain his probable death or recovery. Of course the medicine-man, from the symptoms of the malady or from appearances, has already decided on the answer he is to give in the event of his being employed in the matter, and from long practice and observation he generally becomes an adept in predicting the final death or recovery; for even if the worst be foretold, he is perfectly aware that the friends of the sick person, so far from sparing their beads and losing all hope, will, on the contrary, rather give even more to avert the doom.

But the medicine-man has other ways of increasing his means. When practice becomes low, and the people seem to forget that their prosperity, their health, and even their lives are in his hands, among other tricks he will probably take a pretended nap during the day, and when he awakens will inform those near him that such and such a person will, in his opinion, soon die. This he does in an ambiguous way, without particularly mentioning the person's name, but in such a manner that it is perfectly well understood by all who is referred to. As soon as it is dark, for they never conjure in the daytime unless in cases of great emergency, the doomed person goes to the doctor with his beads, makes a short speech, in which he extols the power and ability of the doctor, laments the fate which threatens himself, and finally presents him with his beads and entreats him to retard, or, if possible, prevent the doom which awaits him. The doctor replies that he is sorry to give him pain and would not wish to take his beads for nothing, that it is probably a mistake, and may even refer to some other person. In his wanderings among his medicinal spirits, or familiars, he merely observed a shade which overhung a particular individual, still it may not indicate anything serious, but he will ascertain correctly during the night and let him know. In the mean time he retains the beads with a secret determination that they shall not leave his possession in future. I have known several fall sick and actually die from the effects of such stories on the imagination, while in other cases it was with the greatest difficulty I could remove the impression produced on their minds by these threatened calamities. The prediction invariably told most fearfully on the imagination, producing first low spirits, languor, then sickness, particularly in bilious subjects, and very frequently even death. When such things occur, the character and power of the cunning rogue has reached its height, and he is ever after looked upon with fear and respect, and consulted with confidence. No hunting excursion, no voyage, nothing, in fact, is undertaken without consulting him. Often have I known a party of Indians on the eve of starting to pass the winter or summer at some place favorable for hunting, when a medicine-man would suddenly set all their plans at naught by circulating the idea that starvation, sickness, death, or other misfortune awaited them in that particular direction, while he would cunningly recommend them some other place, which, from knowledge of the country, proximity to his own lands, or in some way or other was more suitable to his own views.

When any of their relations die all their beads which have not been given to the medicine-man, or otherwise destroyed or disposed of to show their grief, and the estimation in which the deceased was held, are either buried with the body or broken up, and the fragments sprinkled about the grave, or, what of late has been customary, they are kept to be finally distributed among the Indians at the dance for the dead, which takes place nine or twelve months after interment, when their mourning and all outward tokens of grief are supposed to end. All the beads they have on their persons are also distributed in this way, or destroyed, together with their clothes. Their hair is cut close to the head or singed, which certainly gives them the appearance of miserable, grief-stricken wretches. Sometimes too they will cut and lacerate their bodies with flints, or, as sometimes happens, they will, in a fit of revenge against fate, stab some poor, friendless person who may happen to be sojourning among them. Those who bury the dead receive a quantity of beads in payment, but fear of the lifeless body makes them averse to the office, and they generally endeavor to evade being selected to perform the service, owing to the restrictions imposed by their rules on all those who are selected to perform that duty. For instance, they must not eat fresh meat, unless the absence of every other kind of food renders it absolutely necessary to preserve life, and that only when it is cold. They must tear the meat with their teeth, the use of a knife being prohibited. They must drink out of a gourd, carried for the purpose, as they are not allowed to slake their thirst out of any drinking or cooking vessel. Those, too, who have handled a dead body wear peeled willow wands round the arms and neck, or carry peeled willow wands, about two feet long, in their hands. These are supposed to keep off infection, and to prevent any evil effect which might follow the handling of a deceased body. After a certain time subsequent to the death of a relative, the nearest of kin to the deceased, if a man of wealth, makes a general festival for the dead—the "dead dance"—when he distributes the rest of his beads—his whole fortune—to his countrymen, half of what each receives to be returned either in beads or furs after a year, to enable the person who makes the festival to begin the world afresh after he has completed his term of mourning. In the mean time he makes every exertion to collect a quantity of good meat. Invitations are sent to all the neighboring tribes; a level piece of ground is fenced round, and the beads are strung and neatly hung up on painted cross poles within the enclosure. During this time, also, he composes the songs to be used on the occasion, in which all the good qualities of the deceased are enumerated, his abilities as a hunter are extolled, and any good or praiseworthy act he may have committed during life is held up as an example for the imitation of others. When the guests have assembled, early on the following morning, every one cleans and paints himself; fires are lighted within the enclosure; several are set to cook, others to cut up tobacco, while the rest are dancing to the songs of the host and his wives, who, all the time, beats cadence on a piece of painted wood he holds in his hand. After they have had their repast and smoked their pipes the singing and dancing recommence, in which they all join. They then throw a bladder of grease among the crowd. The first who seizes hold of it runs away as fast as he can, pursued by all the rest. When he finds himself hard pushed he endeavors to secure at least a piece for himself, but this is not easy to do, as the grease is mixed up with sinew, which makes it very difficult to break, so he must either endeavor to outrun his pursuers or be content to part with it to the hungry multitude behind. By this time he is getting exhausted, and he tries to double on the others; but, among such a number, it is hardly possible to escape, and he will either stop or throw the grease on one side, when there is a general scrambling for it, accompanied by screams and a noise that is deafening. After going on in this way for a time they will quietly eat the grease, and then return to the enclosure, when a moose skin will probably be thrown among them. The smartest will seize and run away with it in order to secure it for himself, doing as was done with the grease; but this time every one that can catch hold of the skin, while one seizes a knife and cuts away between the hands, until each finds himself possessed only of what he was able to grasp. This goes on for several days, accompanied by wrestling, pushing on a strong pole, fifty or sixty against an equal number, racing, &c. After this the beads are distributed as before stated, the fence is pulled down, harangues are delivered, strong professions of eternal faith and good will are made, when each party takes its departure for its own land, and the term of mourning is at an end.

Their knowledge of a Supreme Being, if they have any at all, is very limited. They know nothing of the soul. They say man has reason, acquired from education, imitation, or experience, which increases with age; for instance, they say a child has no education, no experience—that is, no reason; or if he has, it is so weak or imperfect that he will crawl straight into the fire without the slightest fear of the consequences. If he had a soul, which is part of the Great Spirit himself, he would be as wise when born as at any time of his life; more so, in fact, for he is purer, having just come from his Maker. Neither would he require education or experience to guide him through life. They believe in a future state of rewards and punishments—that is, they believe they will be successful or unfortunate in the world to come according as they have acted well or ill in this; that those who have been poor and miserable in this world, if they have committed no heinous crimes, will be happy in the next; also, that the relative states of a wicked and prosperous man, and that of a poor, despised, ill-treated though innocent person, may be reversed hereafter; that the two will change places, as it were.

They have an imaginary person, a good angel, common to all, who is supposed to guard them from evil and supply their wants. This good angel is supplicated when they start on a hunting expedition, and is supposed to have the power of changing his shape and appearance. The story goes that an old woman found him as a little boy, brought him to her camp and took care of him. This boy made a pair of large hunting snow-shoes for himself, which excited the ridicule of the men at the idea that a ragged, miserable little urchin like him should pretend to require and use such a thing. The boy, however, paid no attention to their scoffs, but continued to be kind and attentive to the old woman, his grandmother, as he called her. His origin was unknown, and he could not give any account of where he had come from. Altogether there was something mysterious about the child which kept him apart from the rest. Whenever they were in distress for want of food and their best hunters could kill nothing, some of them would fall on a fresh track, which, following up, would invariably lead them to a freshly killed animal. From this spot the track and all vestige of the unknown hunter disappeared. This continued for some time until at length suspicion fell on the strange boy and his large hunting snow-shoes. People were set to watch him, and it was found that he was in the habit of leaving the camp secretly, when the others were asleep or otherwise occupied, and returning again in the same mysterious manner. In this way he was discovered to be the unknown hunter and their benefactor. This, however, did not improve his condition with the others. He still continued to be the poor neglected and despised boy he was when they found him. After a time, in winter, the Indians killed a great number of deer. The boy asked them for a piece of fat, which in their arrogance they refused to give. That night he disappeared, and no vestige of him could be found but his clothes, which were discovered hanging on a tree. About a month after he again appeared among them as a grown-up man and well dressed. He told them that he had gone to live in the moon, from whence he would continue to afford them his protection so long as they deserved it; that when they were in distress they were to supplicate his aid, and he would send them relief, with this reservation, that in consequence of their having refused him a piece of fat when he asked them, all animals would in future be lean in winter, and fat only in summer. Since then he has continued to live in the moon, and is ever ready to answer the prayers of the hunter who demands his aid before going on a hunting expedition.

They believe in a future state of bliss, where they are to live forever, in the same bodies they occupied while here. The principal features of this paradise are pleasant hunting grounds, where there is an eternal summer, fat animals, no sickness, no death, with exemption from all labor beyond preparing the meat of the animals they kill for food; but they have, notwithstanding, a great fear of death, and a particular aversion to being buried in the ground. The idea of their bodies being destroyed by worms is horrible. For this reason they enclose the body in a neatly hollowed piece of wood, and secure it to two or more trees about six feet from the ground. A log about eight feet long is first split in two and each of the parts carefully hollowed out to the required size. The body is then enclosed and the two pieces well lashed together preparatory to being finally secured, as before stated, to the trees.

The widow or widows of the deceased are obliged to remain near the body for a year to protect it from animals, &c. When it is perfectly decayed, and nothing but the bones remain, they are burned and the ashes collected and secured in a small box, which is hung up on the end of a painted pole, with a piece of painted wood fixed in the ground to mark the last resting-place of their departed friend. After this the women are allowed to marry again. They begin to dress their hair, and put on beads and other ornaments to attract admirers, to go through the same observances again, should they a second time become widows.

Great or heinous crimes with the Loucheux are thieving—that is, wilful theft—and murder of the innocent by shamanism; also lying; yet they are much given to telling lies and speaking scandal. Employing wealth (beads) as a means of taking away life—that is, paying away beads to a medicine-man to take away the life of another, especially if he be innocent—is a great crime, but the killing of an enemy in a fair stand-up fight is honorable, although they seldom act up to their principles. A Loucheux prefers the safest side of valor, and hardly ever makes an attack unless he is pretty certain of coming off without harm.

Formerly the young women had their chins tattooed in perpendicular lines from the corner of the mouth to the chin. Latterly the practice has been discontinued. Until the introduction of fire-arms by the Company, they made use of bows and arrows in the chase, also of twisted deerskin thongs for snaring the deer and moose. Their arms of defence were the bow and arrow and the knife; their clothing is of dressed deerskin in the summer, and in winter the same with the hair on. They live in conical lodges, rather flat at the top, made of deerskins dressed with the hair on, as well described in Sir John Richardson's work.

3.—THE KUTCHIN TRIBES.—*Jones.*

The Kutchin may be said to inhabit the territory extending from the Mackenzie, at the mouth of Peel's river, latitude 68°, longitude 134°, to Norton's sound, living principally upon the banks of the Youcon and Porcupine rivers, though several of the tribes are situated far inland, many days' journey from either river. The Kutchin nation is very numerous, and is divided into about twenty-two different tribes, each speaking a dialect of the same language, and bearing a very great resemblance to each other in habits and customs. The dress is the same among all the tribes. According to their traditions they were created here, but their account is so intensely obscene that I fear to write it.

Character.—In this they differ entirely from the Tinneh tribes of the Mackenzie, being generous, honest, hospitable, proud, high-spirited, and quick to revenge an injury; in short, bearing a much greater resemblance to the Plain tribes than any other of the northern Indians. They were once very numerous, but wars among themselves, disease, and famine have reduced their aggregate very much. One or two of the tribes are nearly extinct.

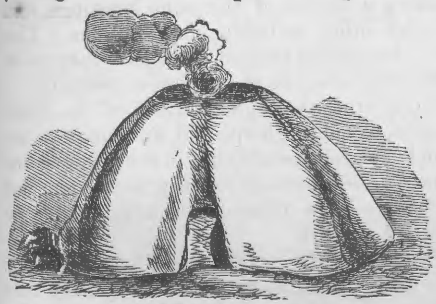
Physical appearance.—The average height of the men is about five feet eight inches, though there are numbers six feet high. The women average five feet three inches, and are very strongly made. The color of the skin is dusky, the hair and eyes black. The men are completely destitute of beard, and both men and women are intensely ugly.

Dress.—The men's summer dress consists of a shirt, pointed before and behind, the point nearly reaching to the knee; trousers, and shoes, both sewed together, all made of dressed deer-skin without the hair. The shirt has a broad fringe of beads across the breast, and there is a broad band of beads down the front of the legs of the trousers. Both fringe and band were in former times made of Hiagua shells (*Dentalium*) or of wooden beads made from willows. The dress of the women is nearly the same, differing only in the shirt reaching below the knee and not being pointed. The winter dress is the same, but is made of deer-skin, with the hair on and turned inside. Sometimes the shirt is made of muskrat or rabbit-skin, but in this case the hair is turned outwards. Mittens of deer or sheep skin, with the hair inside, and a cap of rabbit-skin, with the hair outside, complete the winter dress. The children are dressed in the same way, but have the mittens sewed to the shirt sleeves, instead of being fastened to a line passing over the neck as in the case of the men and women, and their hood is fastened to the shirt, and draws off and on like the hood of a Canadian capote. The men paint themselves with vermilion in lines across the face; they use also a kind of powder from the mountains exactly resembling black lead; they powder their hair with goose down and a kind of red earth during their feasts. The women tattoo their chins with lines from the mouth to the throat by puncturing the skin and rubbing in the black powder mentioned before. The men always, and the women sometimes, bore a hole in the end of the nose, between the nostrils, and insert an ornament into it. Among the Kut-cha-Kutchin, Vondt-way-

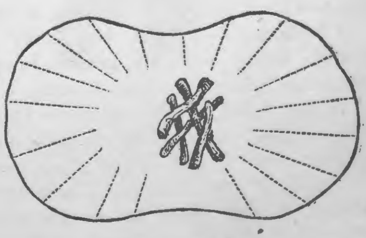
Kutchin, Nat-sit-Kutchin, this ornament consists of four Hiagua shells fastened together, but among the Hong-Kutchin and other tribes a metal ring is used sometimes instead. Making an incision in the under lip, or flattening the heads of infants, are quite unknown among them.

Food.—This consists for the most part of venison or fish, though they eat the mountain sheep and goat, rabbits, partridges, wild fowl, and, in the winter, bears. The bears are not often eaten in summer, as their flesh is not good at that time. The country is full of game of all kinds; moose abound in one part, deer in another.

Dwellings.—These are movable, and are thus constructed: deer skins are dressed with the hair on, and sewed together, forming two large rolls, which are stretched over a frame of bent poles. The lodge is nearly elliptical, about twelve or thirteen feet in diameter, and six feet high, very similar to a tea-cup turned bottom upwards. The door is about four feet high, and is simply a deer skin, fastened above and hanging down. The hole to allow the smoke to escape is about four feet in diameter. Snow is heaped up outside the edges of the lodge, and pine brush spread on the ground inside, the snow having been previously shovelled off with snow-shoes. The fire is made in the middle of the lodge, and one or more families, as the case may be, live on each side of the fire, every one having his or her own particular place.

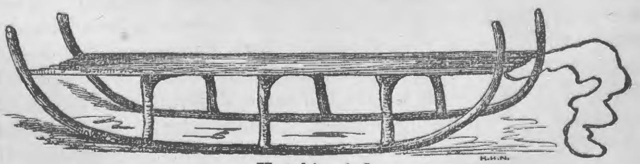


Elevation of hut.



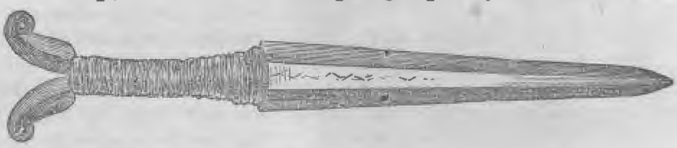
Ground plan of hut.

In travelling, the women haul the lodges, poles, rolls, blankets, kettles, &c., upon wooden trunnions, something similar to the American sleigh, only the runners are turned up behind as well as before, thus being equally fitted to move backwards or forwards. When the day's journey is finished, the men put up the lodges; but when a lodge has to be removed only a few yards, the women do it. When a number of lodges are placed together, no regular form of arrangement is observed, except that the doors are all turned one way, that is, to the leeward. They have no lodges or buildings set apart for public purposes, though they certainly have an enclosed place for medicine dances, feasts, &c., for the dead.



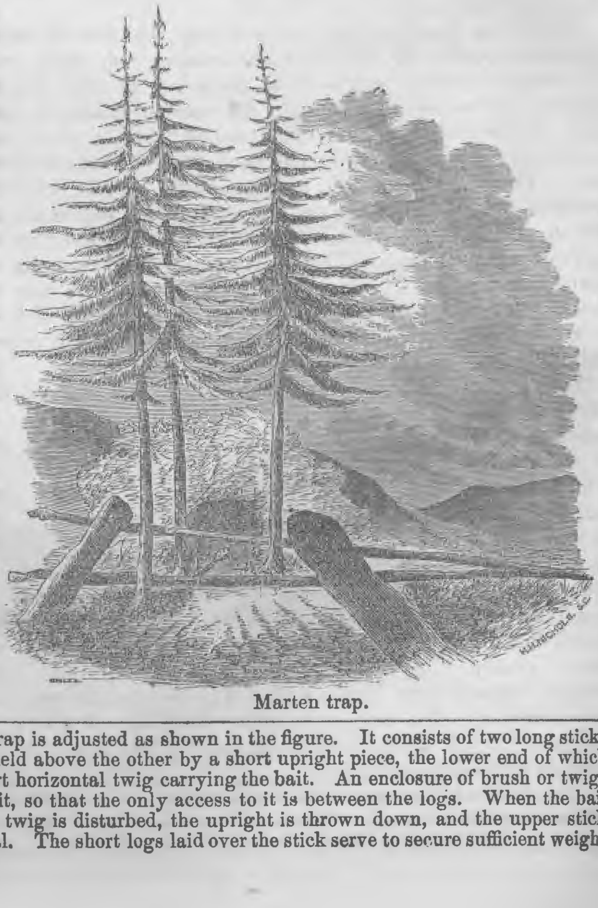
Kutchin sled.

Arts.—There is little to say upon this head. They have no pottery; and their only vessels were constructed of bark, wood, matting, or sheep horns. The birch bark vessels are usually square or oblong; wooden troughs are used as dishes, and wooden or horn spoons are large enough to hold a pint. They are never made so small as a table-spoon. The kettles were, and still are made, by the Hong-Kutchin at least, of tamarack roots woven together. These kettles are very neat; hair and dyed porcuqine quills are woven into them. The water is boiled by means of stones heated red hot and thrown into the kettle. The arrow-heads are of bone for wild fowl, or bone tipped with iron for moose or deer; the bow is about five feet long, and that of the Hong-Kutchin is furnished with a small piece of wood, three inches long by one and a half broad and nearly one thick, which projects close to the part grasped by the hand. This piece catches the string and prevents it from striking the hand, for the bow is not bent much. There are no individuals whose trade it is to make spears, bows, or arrows. They make knives out of 8-inch or 10-inch files; these are long and narrow, pointed and double edged; one side has a ridge running from the handle to the point, the other side is slightly hollowed. The blade and handle are made of the same piece of steel, and that part grasped by the hand is covered with dressed deer-skin, and the top of the handle is curved. They have no means of spinning. They weave kettles of tamarack roots, shirts of strips of rabbit-skin, and caps of the same material. For dyeing they use berries and a kind of grass growing in swamps. Foxes, martens, wolves, and wolverines are caught in traps; moose deer, lynxes, rabbits, and marmots are taken in snares. The general mode of killing moose is to stalk them. In the spring they sometimes run them down on snow-shoes, and in the fall, when the moose are rutting, the hunter provides himself with a shoulder blade of the same animal; he then approaches the male as close as possible, and rubs the bone against the trees. The moose charges at once, mistaking the sound for that made by another male rubbing his horns against the trees.



Kutchin knife.

They sometimes surround an island where the moose are known to be, and kill them

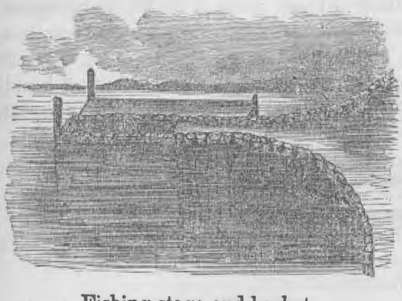


Marten trap.

NOTE.—The marten trap is adjusted as shown in the figure. It consists of two long sticks of wood, the end of one held above the other by a short upright piece, the lower end of which rests on the end of a short horizontal twig carrying the bait. An enclosure of brush or twigs is built up behind the bait, so that the only access to it is between the logs. When the bait is touched the horizontal twig is disturbed, the upright is thrown down, and the upper stick falls, crushing the animal. The short logs laid over the stick serve to secure sufficient weight to kill the marten.

when they run out on the ice or plunge into the river, though this mode is very seldom used, the general way being to stalk them.

Deer are chased on snow-shoes, the hunter loading and firing as he runs. They also make deer pounds, and kill numbers of deer at a time in them, with snares, of which there are several hundred in one pound. When there are a large number of Indians together, they sometimes surround a herd of deer.

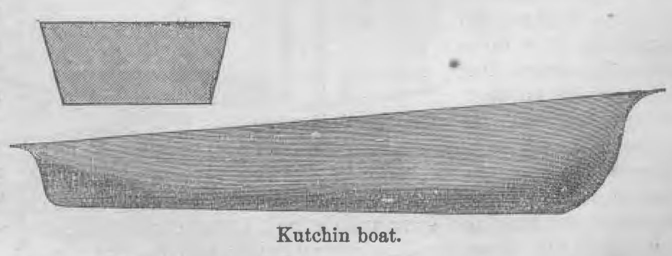


Fishing stage and basket.

They kill fish in bars, terminating in a basket, by the side of which is a stage upon which the fisherman stands. The bars and the basket are made of willows, bound together with babiche, (deer parchment,) wetted and cut into lines, and then dried, and are fastened to poles driven into the bed of the river. The basket is nine or ten feet long, by about four broad; the mouth reaches to the bottom, and the other end floats on the top of the water. When the fish enter the mouth of the basket they are immediately pushed to the upper end of it with scoops, made like rackets for playing tennis ball, and then killed with a blow of a stick. When the basket gets inconveniently full, the fish are carried to the shore in a canoe.

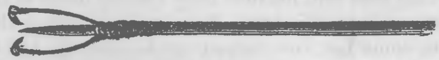
The Hong-Kutchin have another way, but this is only used for killing the big salmon, while the bar is for the smaller fish, such as pike, white fish, &c. The largest salmon weighs from forty-five to fifty pounds, the smaller from eighteen to twenty-five pounds. In salmon fishing a stage is erected on the bank of the river, and a man stationed upon it gives notice when a salmon is passing; this he knows by the ripple it makes when ascending the strong current. The other men, each in the middle of his small canoe, push out, all provided with a bag at the end of a pole; the bag is about five feet deep, and has an oblong frame around its mouth three feet long by one broad; the pole is eight or nine feet long. The Indian paddles his canoe in front of the fish, and pushes his net to the bottom right in front of it; as soon as the salmon enters the bag the man pulls it to the surface and stabs the fish with a knife fastened to a pole about five feet long; he then either lifts the salmon into his canoe, or drags it ashore in the net.

This mode of killing the salmon requires very great skill in the management of the small canoe, as will be easily seen when I say that the canoe is flat-bottomed—is about nine feet long and one broad, and the sides nearly straight up and down like a wall. The fish makes the water foam when it is first hauled up; if it strikes the canoe it will knock a hole in it; if it goes under the canoe it will upset it; and as none of the Kutchin can swim, the consequences might be unpleasant.



Kutchin boat.

The Taitsick-Kutchin make nets similar to ours in shape, constructed of willows instead of twine. The outer bark is scraped off, and the inner taken off and twisted into thread. The Youcon Indians do not make this kind of net, nor do they know how. Their implements for fishing are the bag for salmon, the bar for the fish in the small rivers, a hook and a spear. They also make a small fish out of bone and hang it upon a line in the water; when the pike approach it they spear them. To make a spear a pole about nine feet long is taken, a spike driven into the end, on each side of which is a flexible piece of bone or wood, with a nail or sharp piece of bone attached to it, both pieces of bone pointing inwards and upwards. When a fish is struck, the two jaws, if I may call them so, are forced open, and the spike driven into the back of the fish, and in jerking up the spear the two nails or pieces of bone in the jaws either stick fast in the sides of the fish or meet under its belly, thus preventing it from falling off the spike. The hooks are made and baited in the following manner: The pinion of a goose is taken, and the smaller bone is sharpened and fastened hook-shape to the larger; a piece of fish-skin is cut the shape of a fish and sewed on the hook; that part representing the head is at the point of the hook; that representing the tail is where the bones have crossed each other; a line is then knotted to the larger bone, and all is complete. Muskrats are taken in a scoop, after breaking the rat-house, and beaver with a gaff or net bag, after breaking into their houses, or shot swimming down the rivers.

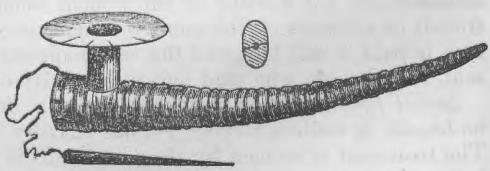


Fish spear.

Baited fish hook.

There are several kinds of berries eaten here, principally the cranberry and a kind of blue berry. They also eat a kind of root; I do not know the botanical name for it, but it grows on sandy ground, is sweet, and when roasted tastes like parsnips.

The Kutchin do not practice agriculture at all, and their only *domestic animals* are their dogs, miserable creatures no larger than foxes. They do not make any intoxicating drinks whatever, but are passionately fond of tobacco; this they of course learned from the whites. Most of the Kutchins smoke in the same manner that we do, but some of the tribes use the same pipe as the Esquimaux, and swallow the smoke. This kind of pipe has a wooden stem twelve inches long, slightly curved upwards; the bowl is well represented by the half of a reel for winding sewing cotton upon, and the hole in the pipe is about the same as that in the spool. A pipe is of this shape; the bowl is made of metal; they do not smoke pure tobacco in it, but mix it with scrapings of willow.



Pipe.

The Kutchin still retain the bow, which is of the same shape, through all the tribes, with the exception of the small guard in the Hong-Kutchin bow, mentioned before. The quiver is the same, and worn under the left arm; it is furnished with two small loops to hold the bow, thus leaving the hunter both hands free to use his gun. The arrows are placed in the quiver with the notch downwards. The Kutchin are not expert with the bow; no doubt they were better shots before fire-arms were introduced among them. The bow is made of willow, and will not send an arrow, with sufficient force to kill a deer, more than from fifty to sixty yards. The arrows are made of pine.

Trade.—The Kutcha-Kutchin, among whom the fort is built, are traders; they make very little for themselves, but buy from the other Indians; their standard is called a naki eik, (bead clothing;) it consists of long strings of beads joined together at the distance of a foot; the lines are seven feet long. The whole naki eik is equal to twenty-four made beaver, and one of the lines is one or more beaver-skins, according to the value of the beads.

Before the arrival of the whites they had no religion, but they believed in a Supreme Being who would do good to them, but they knew of no evil spirit. One man told me that they had no devil at all before the whites came. They have many superstitions. For instance, when the fire made a hissing noise they threw in some fat, and asked to be able to kill some animal; if a crow passed they asked it for meat, and promised to share it with the crow. There are several rocks that they used to make offerings of beads to, in order that they might be able to kill some animal soon.

Medicine.—As they had no religion, so they had no priests, as with the southern Indians; nor had they any sacred fire. They had, however, magicians, who could do wonderful things. If you were to believe their own story, they could make wind, prophesy, and when a storm of rain was coming, by putting their medicine bag on a pole at the side of the lodge next to the storm, they could make the clouds turn and the rain fall in another place.

The medicine man, whose profession it is, or, rather, who professes to cure all diseases whenever he pleases, is rather an important man among them. His treatment consists of singing barbarous songs over the sick person and performing all kinds of antics; he is also a magician; in fact, there is little or no difference between them. They practice blood-letting also, *ad libitum,* and for every complaint, from a headache to a pain in the big toe. As for plants, they have no knowledge of them whatever, except one which they eat and another which is poison; this last is never used for any purpose.

Government.—They are governed by the same chiefs in peace and in war. The authority of a chief is very limited, for the Indians are very unruly, and not at all disposed to submit to authority. The chiefs are chosen either on account of their wisdom or courage, and not at all on account of birth. They have no insignia of office, and as for privileges they have all that they can take, and none that the others can withhold from them. The chiefs and old men are all who are entitled to speak in council, but any young man will not hesitate to get up and give his seniors the benefit of his wisdom.

Law.—They have no law; or, rather, the injured party takes the law in his own hand. For theft, little or no punishment is inflicted; for adultery, the woman only is punished, being beaten and sometimes thrown off by her husband, and instances are not wanting of the woman being put to death; for murder, the friends or relations of the murdered man revenge his death; but if a medicine man is paid to kill him, and the man happens to die, the medicine man is innocent, and the one who paid him is the guilty one.

Social life.—Slavery is practiced among them. Any poor creature who has no friends is made a slave. Female chastity is prized, but is nearly unknown. The treatment of women by their husbands is very bad; they are, in fact, little better than slaves. If a Kutchin is eating he does not allow his wife to eat with him, but after he is satisfied he throws her some meat just as he does to his dogs. She cuts and hauls his fire-wood; she hauls his lodge, kettles, and property when the camp is moved; she hauls the meat to the camp in winter and carries it in summer. During the warm weather she dries the meat, carries him water, makes his clothes, laces his snow-shoes, and, indeed, does all the drudgery of the camps; but in travelling the men do a little—just a little; they go before, making a track, and stop at such a place as they think the women will be able to reach about nightfall. They choose a level place just large enough for a lodge, scrape off the snow, line it with pine brush, cut some few armfuls of dry willows, and the women put up the lodge when they arrive.

The men always cook. If a wife will not obey her husband she gets a good beating. Children are generally well treated by their parents.

They have no regular festivals, but when a man of consequence dies his friends make a dance, as the whites call it, in his honor. A space some twenty yards square is railed in, a fire lighted in the middle, and various games are played, such as putting a pole across the fire, one party trying to push the other to one end of the enclosure, or one takes a dressed deer-skin and runs off with it; the fellow, if he is nearly caught, drops the skin, when another takes it and is chased by all the rest in his turn. At last the deer-skin is seized by as many as can find room to take hold; it is then cut up, each retaining the piece in his hand. Sometimes bladders of grease are used instead of deer-skin. Now and then they all gather in the enclosure, and, standing round the fence, sing mournful songs and make speeches. This continues for ten or twelve days, when the fence is thrown down, and the beads and other things provided by the person making the dance are divided, each person receiving a present in proportion to his rank; but this present is not entirely gratis, for some months afterwards the giver will come and say: "I gave you thirty 'made' beaver; pay me fifteen and keep fifteen;' which has to be done, of course. The same way when a person dies, if he is a great man among them. Four men make his grave, or, rather, either burn him or hang him up in a coffin. These four are paid as follows: The first gets thirty, and pays ten made beaver; the next twenty-five, and pays ten; the next fifteen, and pays five; the next twelve, and pays three. The coffin, when the body was to be buried that way, was supported upon a stage, with a knife, bow and arrows, a flint fastened to a stick, a stone to strike it on to make fire, and a piece of the fungus that grows on a birch tree for tinder, with some touch-wood also. The body was dressed in the best they had and painted, and was placed in the coffin with the various things mentioned above. The men who made the grave or buried the corpse lived apart for two moons. A man was put on the stage if he was well liked; and they used to burn them to keep the maggots from eating the corpse.

There is no ceremony observed at marriage or birth. A man will sometimes take a small girl ten or twelve years old for his wife; but this is merely a precaution to secure her, as she cannot live with him as a wife at that age. A man may take a wife of the same band to which he himself belongs; but if he take a wife from another tribe, the children belong to the tribe of their mother.

A woman must live apart from her husband during her monthly terms. They are in the same lodge, but a partition made of willow is between them. A young woman must live entirely apart in a separate lodge during her first two terms, or she will spoil the hunting of the men. All the Kutchin are divided into three castes, called, respectively, Tchit-che-ah, Tenge-rat-sey, and Nat-sah-i. It used to be customary for a man belonging to one of these castes to take a wife from one of the others, but this has fallen into disuse.

With the Kutchin the *father* takes his name from his *son* or *daughter*, not the son from his father, as with us. The father's name is formed by the addition of the word *tee* to the end of the son's name; for instance, Que-ech-et may have a son, and call him Sah-neu. The father is now called Sah-neu-tee, and his former name of Que-ech-et is forgotten. They sometimes change a woman's name from Toat-li to Sah-neu-behan, or Sah-neu's mother.

War.—The murderers—it would be ridiculous to call them warriors—array themselves in paint and put three eagle feathers in their hair. Before setting out they join in a dance similar to the one for the dead; but at the end of it the men get into a line on one side and the women on the other; the men then run at the women, the latter lie down, the men jump over them, and the man who falls will be killed in the fight. The dance over, the party set out, killing everything they meet—foxes, crows, and every living thing—so that it may not give notice of their approach. When they meet the enemy they pretend to be very great friends. After some time—perhaps days after their arrival—they seize the opportunity when their hosts are off their guard and plunge their knives into their hearts. Their weapons were the bow, arrow and knife. They murdered men, women, and children, except such of the women as took the fancy of their brutal conquerors, whom they took and treated no worse than the women of their own tribe.

They have no knowledge of scalping, nor do they shave their heads.

The changes introduced among them by the whites are as follows :

There is far less murdering than in former times. The women do not kill their female infants. The young men do not strangle their parents when they are too old to be of service, and become a burden upon them.

They use the gun instead of the bow in hunting, and iron axes and knives instead of stone, and they treat their women better.

I forgot to mention in the proper place, that in war, when a man kills his enemy, he cuts all his joints.

Indian cradle.

THE AMERICAN MIGRATION.

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PREFACE.

THE following pages are occupied with a subject but little considered and, as far as I am aware, not yet specially treated of. Limited, however, as are the sources of our information respecting aboriginal American history, the present essay can prefer no claims to completeness in having exhausted the materials actually before me. But my fellow-laborers, of whom it is to be regretted there are but few in Germany, and scarcely any within a more immediate circle, will perhaps meet herein with some views not before promulgated. I have scrupulously endeavored to collate, as far as possible, whatever relates to the American migration and constitutes the sum of our present knowledge respecting it, and my aim will have been attained if I shall succeed in drawing the attention of the public to this wide and uncultivated field of the ancient history of America.

In order to facilitate the necessary preliminary studies of any one to whom this subject may offer serious attractions, I have not deemed it superfluous to supply in the notes an array of the most remarkable and important authorities which furnish the material to be employed in the elaboration of primeval American history, although it be necessary with this view to cite many works which have no direct bearing on the subject treated of. In each of the cited works there will at least be found some detached contribution to our knowledge of the migration in question; it was, in fact, from such notices that my own studies have been drawn, since, as above mentioned, there was no special treatise to which I could have recourse. Where occasion offered, I have compared with one another the phenomena of migration in both continents, even when no ulterior consequences were to be deduced; prompted in this by the reflection that it is always interesting to bring into juxtaposition the parallel incidents of history.

My point of view, which I have deemed it necessary to indicate in advance, is frankly opposed to that adopted by the greater number of inquirers in this and kindred provinces; the hypothesis of an immigration by which America was peopled finds able and influential supporters; the conception of an autochthonous population is shared only by the few. It will be understood that only extensive and analytical researches, lying beyond the scope of the present publication, can have forced upon me an opinion which I cannot alter until its untenableness shall have been clearly established.

Taking this opportunity to express my warmest thanks to the accomplished and friendly individuals, both far and near, who have in the most liberal manner promoted my studies, I commend the following pages to the indulgence of the learned and the kindness of the reader.

VIENNA, *February* 6, 1866.

As the geographical description of the theatre on which the most important human transactions have taken place supplies a peculiar value to the representations of the historian, and seems an almost indispensable background to the historical picture, so are isolated historical events of high interest to the geographer who undertakes to investigate and explain certain remarkable phenomena, especially in relation to the geography of races and languages. One of these events which enters most deeply into the geographical relations of the world, which offers accordingly a point of contact as well to the historian as the geographer, is incontestably that vast phenomenon of the migrations which are authentically recognized as having taken place in all parts of our earth. *

In the following pages it is the migration in America which is to be especially kept in view. Some brief preliminary remarks, however, seem to me indispensable in order to indicate the point of view from which the following studies were prosecuted. Although it cannot be denied that, in the earliest epochs, migrations of tribes everywhere took place, I am forced to dissent from the opinion so often revived in latter times, which supposes, for the sake of explaining all that is otherwise inexplicable, a general peregrination which is itself involved in the most mysterious obscurity. In assuming such migrations the utmost circumspection should be observed, and the assumption can only be vindicated when shown to rest on firm historical foundations. For this reason I am unable to give in my adhesion to the theory which assumes that the original seat of the human races must be sought in higher Asia or somewhere else,† whence mankind are supposed to have spread themselves gradually over the whole globe; an assumption which is contradicted, in the most decisive manner, by the peopling of the New World. It is impossible to enter here into all the hypotheses ‡ which have been framed for the explanation of a fact so perplexing to the biblical students of the sixteenth century, and of course later times; it is enough to say that thus far not one of them has been found to correspond

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* We know that even the South Sea islanders migrate. See on this subject: Dummore-Lang; *View of the Origin and Migrations of Polynesian Nations.*—Quaterfages: *Les Polynesiens et leur migrations* (*Revue des Deux Mondes*, Feb. 1864.) —O. F. Peschel: *Die Wanderungen der Südsee-Völker* (*Ausland*, 1864.)

† Galindo, for example, transfers the original residence of the human race to America. and supposes civilization to have thence migrated into the Old World.

‡ I content myself with mentioning, besides the writings of Delafield, J. McIntosh, Bradford, Zestermann, Castaing, Feyjoo, J. Perez, Mitchell and the Abbé Brasseur de Bourbourg, the following interesting and rarely cited works, for which I am, for the most part, indebted to the kindness of Hr. George Schwarz of Vienna, who has allowed me to make unrestricted use of his rich collection of books relating to America: Hugo Grotius: *Dissertatio de origine gentium Americanorum, Amstelodami*, 1642.—Jean de Laet; *Notæ ad diss. H. Grotii de origine gent. Americ.*, 1643.—Jean de Laet: *Responsis ad H. Grotii diss. de origine gent. Americ.* 1644.—Poisson: *Animadversiones in originem Peruvianorum et Mexicanorum*, Parisiis, 1644.—Georgius Hornius: *De Originibus Americanis*, Hagæ, 1652.—Gottfried Wagner: *De Originibus Amer. Dissertatio* Lipsiæ, 1669.—Thom. Thorowgood: *Jews in America, or probabilities that the Americans are of that race*, London, 1650.—And Rocha: *Tratado unico y singulare del origen de los Indios occidentales del Perú, Mexico, Santa Fé, y Chile*, Lima, 1681.—Engel: *Essai sur cette question: comment l'Amerique ast-elle ete peuplée d'hommes et d'animaux*, Amsterdam, 1767.—*Philosophische Untersuchungen uber die Amerikaner*, Berlin, 1769.—Corn. de Pauw; *Recherches sur l'Amerique et les Americains*, Berlin, 1774.—Vater: *Untersuchungen uber America's Bevolkerung aus dem alten Continent*, Leipzig, 1810.—McCulloch: *Researches philos. and antiquarian concerning the aboriginal history of America*, Baltimore, 1829.—Josiah Priest: *American Antiquities*, Albany, 1834.

even approximately to the demands of science; and that theory is probably, in every point of view, the most tenable and exact which assumes that man, like the plant, a mundane being, made his appearance generally upon earth when our planet had reached that stage of its development which unites in itself the conditions of man's existence. In conformity with this view I regard the American as an autochthon; consequently there can no question be here entertained respecting an immigration into America.

The idea of the migration of American tribes is, for the most part, and by early writers nearly always, confounded with this immigration into America. While the American continent exhibits monuments, not to be mistaken, of a migration of the native tribes within the same, the idea of a foreign intrusion has been strongly sustained for three hundred years, during which these monuments have received a false interpretation. On the other hand, the surprisingly high culture witnessed by the first European visitors in a part of the world assumed to be inhabited by savages remained so inexplicable, and the knowledge at that time of the degree of civilization existing among the further Asiatic nations was so limited, that recourse was had to hypotheses which later inquiry has by no means tended to substantiate. Only in the most recent times and with difficulty has some progress been made in favor of the opinion which regards the American autochthons as a people who had attained a form of civilization by modes of their own, a conclusion which enriches the philosophy of history with a fact of no inconsiderable import.

Were we disposed to reduce into one comprehensive system all the fallacious hypotheses advanced, and which would, in fact, scarcely exclude any nation from the honor of having peopled, or at least civilized, America, we should find that they fall into two categories, which were only too often advocated by one and the same author, thus introducing into the question a truly embarrassing complexity. To the first category belong those hypotheses according to which America remained unoccupied and waste until, in process of time, some immigration, no matter on which side, took place from the Old World; a consequence, perhaps, of the general movement by which men separated themselves more and more widely from their original abode in Central Asia. This is the theory of a simply *populating immigration*. To the second category pertain those opinions which, in order to account for the singular degree of culture in America, suppose an immigration, no matter again on which side, of a peopleal ready civilized, though in times, of course, considerably nearer to our own. This immigration is also regarded by many as the occasion of the actual and acknowledged American migration; a view which obtained especial credit from the partial acquiescence of A. von Humboldt, who, indeed, avowed that he considered an ancient intercourse between western America and eastern Asia as more than probable, though he could not determine the manner in which the connexion of these Asiatic branches of the human family took place.* As distinguished from the first, I would call this last the theory of a *civilizing immigration*. But this, also, receives no countenance from the results of the latest investigations.

Freed from all these theories of immigration of whatever name, it remains to contemplate the migration of the American indigenes as an historical phenomenon, which stands alone and independent, like those observed in the Old World, with which we have, of course, far better means of being acquainted. To ascertain the causes of these, as well as of the migration of populations beyond the ocean, is a hitherto unachieved task; everything in relation thereto is mere conjecture. Whoever, like myself, entertains the persuasion that man in his development, and hence, indirectly in his history, is potentially influenced by

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* *Ueber Steppen und Wüsten*, in the *Ansichten der Natur*, Stuttgard, 1859, vol. I, p. 151. See, also, numerous passages of Humboldt's masterly but little read work, *Vues des Cordillèras et Monumens des Peuples Indigènes de l'Amérique*, Paris, 1816, vol. II.

the geographical conditions of the soil which he inhabits, and therefore stands in a much more dependent relation to its nature than is commonly supposed, may advance the indeed sufficiently hazardous hypothesis that this secret cause is to be sought in some natural phenomenon to us unknown; yet, to this supposition can no other than an entirely problematical value be attached.

If we seek, however, to establish for historical events a basis in geographical relations—that is to say, if we carefully compare them together, analyzing the former and investigating their possible causes, studying the latter and deducing as far as possible the resulting consequences—we shall find that certain generally valid laws, which resolve in the simplest manner many an unexplained riddle, are evolved from such a study through the remarkable correspondence of facts. Thus, in reference to the migrations of mankind, it seems to result from the geographical structure of continents that, as by virtue of an *historical law*, we are not to look for men of comprehensive and deeply penetrating intellect in Lapland or Malta, in Bosnia or Asturias; so, conformably with a strict *geographical law*, the *direction of the migratory stream will be found always to lie in the axis of the greatest longitudinal extension of the continent.** In fact no example from history informs us that the Tchapogires, Tunguses, Jakoots, or people from the banks of the Amour have ever descended into the Deccan or Malacca; that the Ethiopians have ever migrated into Senegambia, or the Finns into Greece. As a new proof how much nations and men depend on geographical circumstances, and even when they believe themselves guided by their own will, merely obey a great natural law, the fact is of much significance that the American tribes form no exception to this general rule, for here, also, the procession of the migratory races is in the longer axis of the continent, namely, from north to south. This implies, of course, no mathematically exact line of transit, which, in America, apart from all collateral circumstances, would be simply impossible, from the fact that the southern part of the continent projects considerably further to the east than the northern. By a progress from north to south we understand no more than that the migrating tribes always tended towards more southerly regions.

That America, as well as Europe and Asia, was already inhabited before this great migration, and in many parts was possessed of an ancient civilization, admits of no doubt. Occasional traditions of those early periods of culture have penetrated to us, and I cannot forbear soliciting the attention of the learned world to this legendary cycle of America, which is certainly worthy, in every respect, of a critical scrutiny; for to judge from so much as is yet known, the inquiry cannot but yield interesting and valuable disclosures respecting the cosmogonic views of the American aborigines and the general tendency of their ideas; perhaps endow even the historian here and there with a fact of value.† But to determine, from our present knowledge of the mystical traditions of these races, which of the tribes in America may have been the oldest, seems to me as impossible as superfluous. Upon this soil multitudes of nations have moved

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* I first advanced this proposition in the session of the Imperial Geographical Society, at Vienna, 14th March, 1865.

† It is wholly impossible, within the compass of this work, to enter into details or recur to all the sources which it has been necessary for me to take advantage of. I think it proper, however, in the interest of this in general but little cultivated study of the past ages of America, to name, from time to time, a part, if but a small one, of the principal works from which further information may be derived, and on which the views here developed are founded. With regard to legends and myths, see A. Rossaeus: *Der gantren Welt Religionen, oder Beschreibung aller Gottes, und Goetrendiensta in Asia, America, und Europa*, Amstelodamie, 1667—Ternaux Compans: *Essai sur la Theogonie Mexicaine*, Paris, 1840, (Annal. des Voyages.)—Carl Rafn: *Cabinettet for Americanske Oldsager*, (Antiquarisk Tidskrift, Kiobenhavn, 1852-54.)—J. G. Müller: *Geschichte der Americanischen Urreligionen*, Basel, 1855.—Brasseur de Bourbourg: *Popol Vuh, la livre sacré et les Mythes de l'Antiquité Mexicaine*, Paris, 1861.

and have sunk into the night of oblivion without leaving a trace of their existence; without a memorial through which we might have at least learned their names. Those nations only, which, by tradition, written records, monuments, or whatever other means first guaranteed the remembrance of their own existence, belong to the domain of history, and history which, to be true, accepts nothing but what is actually known, points to those as the primitive races which first transmitted a knowledge of themselves; time begins for us where the chronology of such nations takes its rise. But all these so-called aborigines might be only the remainder of previously existing races, of whom, again, we know not whether they were indeed the first occupants of the land.* In truth, we meet in America, at more than one point, with traces of a rich civilization, proceeding demonstratively from much earlier epochs than the tribal migration itself; as, for example, in upper Peru the gorgeous structures of the Aymaras, near Tiahuanuco, on the beautiful shores of Lake Titicaca; the mysterious monuments of Central America, between Chiapas and Yucatan, of which the buildings of Palenque constitute the most celebrated representative; the earth and stone works of a people distinct from the above, on the banks of the Mississippi and Ohio.

These facts, however, should not surprise us. We need only cast a glance at Europe itself. We know that the migration in middle Europe encountered races which had been preceded by the powerful Celts, a people widely spread, and endowed with a certain degree of culture, yet destined almost wholly to disappear, except at a few points whither the stream of migration failed to penetrate. But those great stone monuments which we find scattered from the most northern districts of Scotland to Portugal, from Courland to the western parts of France, and which, under various denominations, according to their form, are known as dolmens, menhirs, cromlechs, cairns, and giants' graves, whose occurrence in Africa has been recently demonstrated, and is asserted as regards even Syria and the Deccan. These all point to a still older pre-Celtish people, who, less cultivated than the preceding Celts, remain to us as great a mystery as the "mound-builders" of the Mississippi. The Celts, on their part also, wandered out of Asia into Europe, and probably met with these dolmen-builders, respecting whom we are at a loss to know whether they are to be referred to the Danish kiökkenmöddings and the Swiss pile-buildings, or whether these most ancient memorials of human society belonged to a still older people.†

These obscure periods of man's history in the old world have, as is well known, been distributed by European archeologists, in conformity with the most important remains of their industry, into the epochs of stone, of bronze, and of iron. Since heaps of shells, like those of the Danish coast, are found on the seashore in Newfoundland, at St. Simon's island on the coast of Georgia, and recently near Guayaquil in Equador, it is not without just grounds inferred that the same gradations of development which have given names to the above epochs will be also recognized among the aboriginal Americans. The duration of these periods would of course be very different, and their termination and commencement, respectively, be later than was the case in Europe. In Asia, undoubtedly, at least among some nations, the bronze epoch was preceded by a period of copper, which seems to have been wanting in Europe. The admirable

* Manuel Orozco y Berra: *Geografia de las Lenguas y Carta Éthnografica de Mexico*, Mexico, 1865. I call the attention of all the friends of linguistic science to this elaborate and most meritorious work of my colleague, and add, that for the investigator of American history it is quite indispensable.

† Upon this interesting subject, which has in late times given rise to renewed discussion, recourse may be had to the suggestive writings of the Swedish naturalist, E. Desor, in the Augsburg and Cologne Gazettes, 1865, wherein he propounds his well-known Tamhu theory, which supposes the dolmens to have been the work, not of the Celts, but of the Tamhu tribes of Africa. Opposed to this are the opinions of my esteemed friend, Dr. Carl Andree, of Bremen, which he has communicated to the public through the Globus, edited by himself, and which present views worthy of the highest consideration.

researches of Squier and Davis * regarding the remarkable antiquities of the valley of the Mississippi have disclosed in America a civilization which preceded the bronze era, and was marked by the use of a pure red copper, which was hammered in a cold state, and whose sources are to be sought on Lake Superior, where it still occurs in large quantities. When this American age of copper existed, is not now to be determined; we only know that it must at least reach back to a thousand years, since such a period would certainly be requisite for the growth of the venerable forests which now cover the remains of that ancient civilization.† The end of the bronze age, on the other hand, may be determined through the beginning of that of iron, which made its appearance with the first settlement of Europeans in America, and is characteristic of the existing era of civilization.

The question might here suggest itself, in view of the existence of a considerable degree of culture in America before the migration, whether, in general, tribes in a state of civilization readily accommodate themselves to a migratory condition. If this question cannot be unconditionally answered in the affirmative, it cannot still be wholly negative, for experience has taught us that tribes who address themselves to migration, as, for instance, the above-mentioned Celts of Europe, always possess a certain if not very high degree of civilization; at all events, if not actually civilized, they are susceptible of becoming so. Even those hordes which, in the dawn of the middle age, overran Europe and overthrew Rome, the Goths, Vandals, Heruli, and Huns, were by no means devoid of all culture, possessing, as they did, even more of it than the races which they found and subjugated in Germany and Gaul. For these countries the migration was no retrogradation; neither had that of the Celts been so, who had previously wandered out of Asia. That a people in the full enjoyment of civilization, with political institutions, and the manifold resources of social life amply developed, should deliberately quit their abode in obedience to some inexplicable impulse of migration, calls for no long discussion. In single individuals—in numbers, indeed, of single individuals—the migratory impulse, through social, political, or other motives, may still be awakened, as we witness constantly in the case of European emigration. But whole races of men never set themselves in motion when they have once transcended a certain limit of civilization. It is, perhaps, not superfluous to observe that, if we speak here of an advanced civilization in ancient America, the phrase is by no means to be taken in an European sense. The sumptuous structures of Chichen-Itza and Palenqué sustain no comparison with the simplest monuments of Greek and Roman cultivation. The American drawings and sculptures are very far from indicating any truly artistic insight, while to any further advances in civilization in an European sense, the total absence of phonetic writing,‡ familiar as this was to the

* *Ancient Monuments of the Mississippi Valley*, (Smithsonian Contributions to Knowledge, vol. i: Washington, 1848, 4^to^.) The antiquities of this valley constitute already a considerable body of literature, which, for want of space, I cannot here further discriminate. Suffice it to say, that the work just cited is the most complete of those now existing.

† See upon this subject the views developed in the "Natur," by the learned Dr. Otto Ule, of Halle. (*Blicke in die vorgeschichtliche Zeit des Menschen*, 1865.)

‡ Several nations of America were possessed of written hieroglyphical signs, but it was in Mexico that these had been most cultivated, and indeed the Mexican historical inquirer, Abbé Brasseur de Bourbourg, believes in the existence of a sort of phonetic writing. But a correspondence which I have held on this subject with the distinguished linguist, Don Francisco Pimentel, Count of Heras, has not resulted in a confirmation of this assertion. Don Pimentel writes to me, 21st September, 1865: "I have yet seen nothing to convince me that the ancient Americans had written characters like ours, although M. Brasseur is of a different opinion. They appear to have had only a *representative* and a *symbolical* mode of writing: the first a mere copy of the objects; the second expressive of ideas which admit of no material representation, as by placing a serpent to express *ime*. Something phonetic has been surmised in the characters employed to convey the pronunciation of the names of places and other proper names, but in my view this proves nothing, because all these names are significant, and might very well be expressed in a direct manner."

Assyrians, Persians, and Indo-Bactrians, nations which stood otherwise at a similar grade of development, must have operated as an effectual obstruction. Abstracted, however, from all purely cis-atlantic modes of thinking, the culture originally found in America must appear so much the more advanced as we compare it with the grade of development of other people in a state of nature—with that of the present Indians of America, for instance. This is the criterion which should be applied to all relating to the ancient civilization of America, and we shall thus be always justified in speaking of it as surprising in its advancement.

The people who, in due course of time, set themselves in motion in America, and sought new abodes in a southern direction, had not attained the degree of civilization which the Spanish conquerors marvelled at in the red men who inhabited the land at the epoch of their arrival, and which, notwithstanding, as above stated, fell far behind the ideas of European civilization even then entertained. I hold it, therefore, as by no means improbable that races, susceptible of improvement and bearing with them the germ of development, did really take part in the great American migration, but must decidedly regard it as an error to believe, as the advocates of the theory of a civilizing immigration do, that civilization was introduced into America simultaneously with the migration. That the course of civilization could not be wholly independent thereof is readily conceived. The event is considerable enough to have exerted in America, as in Asia and Europe, an influence on the development of civilization, the advanced condition of which, however, is very far from being identical with that of the migration.

To one who contemplates the features of the migration which in our older continent swept like a lava stream, in the fourth and fifth centuries, over Asia and Europe, hurling to the dust the decayed fabric of Roman domination, the migration of America must, in its whole form and nature, appear highly surprising. Here we deal not, as in Europe, with the relatively short period of some century, during which the different races, rapidly following one another, crowded together upon the same theatre, but, rather, a slow and perhaps intermitting progression, which required for its complete accomplishment a period of at least five or six hundred years. Modern inquiry, to be sure, has taught us that in further Eastern Asia, and many centuries before our chronological era, the stream of migratory tribes set itself in motion, and the attack of the Turkish race of Hiong-Nu on the Usün, a fair, blue-eyed, perhaps Indo-Germanic people, was the first check to the general movement which propagated itself from the great Chinese wall to the west of Europe. So far, certainly, is a similitude established between the migrations of the two continents, since to a certain point the advance of the Asiatic hordes was very slow, and only in the end, on its arrival at the boundaries of Europe, did it become a precipitate one. But in America it would be unquestionably more correct to consider the migration of tribes as a *shifting* of place, for it bore throughout this character. The circumstance that the American movement began about the time when the Asiatic-European ended, has led many to infer that it was the latter, which was simply continued into America, and of course coincides with the immigration theory which I have before more particularly spoken of.*

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* The total unacquaintance with a milk diet in the new continent, and the consequent absence of pastoral pursuits, which forms in the Old World the transition between the nomadic hunting tribes and the agricultural populations, (certainly a striking phenomenon,) induced Humboldt to derive the supposed immigrants from those races of Northeastern Asia, of whom we know that the use of milk is to them equally a stranger. (Ans. der Nat, I, 52, and Vues des Cord., I, 34.) It further follows that he who admits the emigration to have proceeded from the northern regions of America, where climatic influences forbid the rearing of herds, will arrive at the perhaps more correct conclusion that a diet of milk was unknown to the Americans *a priori*. Were this not so, the immigrating tribes would very soon have learned from the original inhabitants the use of milk as well as of the farinaceous gramineæ.

The wide region east of the ranges of the great Rocky and Cascade mountains, and west of the Alleghanies, traversed by the mighty waters of the Mississippi, Missouri, Ohio, and their tributaries, this region, the proper home of the mound-builders, preserves no trace whatever of an immigration or emigration. The smooth prairie, belonging to a lacustrian formation,† embosoms only those enigmatical hillocks of earth which remain as yet the sole monuments of a widespread but vanished people. Nevertheless, it is more than probable that from hence the first emigrants moved towards the south. These lake regions, in their southern part, reach the latitude of 42° north, and according to Humboldt, the mysterious Aztlan, whence tradition alleges the Aztec races to have issued,‡ is not to be sought further south than the same latitude. In still higher latitudes appears the Esquimaux with his *karalit* language, wholly distinct from the rest of the American idioms, as his physical structure also varies from that of the other American autochthones, whence the Esquimaux is properly to be identified with the polar man, the original and peculiar occupant of the arctic circum-polar regions of all parts of the world.* It may be assumed, therefore, with reasonable probability, that the region from which the migration proceeded is to be found between the limits of the karalit dialect on the north and the 42° of north latitude on the south. Without deducing consequences from this, I must yet signalize the singular fact that the tract of Asia in which lies the probable point of exit of the migration of the old continent, between the great wall, namely, on the south, and the Amour river on the north, falls with nearly mathematical exactness under the same geographical degrees of latitude (the southern boundary coincides within 30′) and the same climatic conditions as that of America. May there not be herein some circumstantial confirmation of my above proposed theory respecting the original causes of the respective migrations?

Although in the luxuriant pastures of this region no traces of the path followed remain, yet I am of opinion that observation of the geographical conditions of the land may guide us to a knowledge of its course. It may be assumed with certainty that such a migration always follows the most commodious conformations of the country, avoiding all obstacles which would oppose themselves to the easy transit of the great human multitude. Thus we see in Europe the advancing hordes first sweep unimpeded over the level steppes and gentle slopes of Sarmatia, then appear on the Danube, and, following the valley of that stream as the easiest route, penetrate into the heart of middle Europe and to the gates of Italy. In fact, the earlier Celts had before chosen this commodious road for their migration. If, then, it is permitted us to conclude from analogy, which is no uncertain guide when like causes operate under like conditions, the populations of the copper age of America, which had already dawned in the region of the lakes, would have followed the valleys of the Ohio and Mississippi, and then have directed their steps through the present States of Louisiana and Texas, probably along the edge of the gentle acclivity which, under the name of the Sierra Guadalupe, stretches from the Rio Grande to the Rio Brazos, towards the banks of the great Rio Grande del Norte.

There are many indications, moreover, which lead us to believe that this was not the only route by which the northern tribes made their way to the south. A part of them seem to have become detached in the Mississippi valley, and to have projected themselves towards the southeast into Florida, the seat of a higher civilization, whence they eventually proceeded to Cuba and Yucatan;

† According to the researches of Professor Alex. Winchell, (Silliman's American Journal of Science and Arts, November, 1864, No. 114.)

‡ See, as regards this, Humboldt, *Vues des Cordillères* in different places.

* M. Cl. Markham, secretary of the Geographical Society of London, delivered, 27th February, 1865, a discourse "on the origin and migrations of the Greenland Esquimaux," in which he supposes them to have proceeded from Upper Asia! So I have read, at least, in an account of the discourse, the original of which I have not been able to see.

while a branch of them, traversing the whole length of Cuba and the great arch of the Caribbean islands, descended finally to the banks of the Oronoco. This fraction of the migratory population may, of course, have been small and its impetus inconsiderable, since the necessity of maritime transport, though only from island to island, would naturally impair its force. In the opinion of others, and among them Humboldt, even the Rocky mountains in their extension northward may have led similar branches of emigrants to adopt a different path in their progress towards the south. Whether these branches originally issued from the lake regions, though it is not impossible, is difficult to determine. They must at any rate, in departing from their homes, have taken a directly west or at least southwest direction. Although no substantial reasons can be assigned why any race of those latitudes should have given a preference to the toilsome defiles of the Rocky mountains, when the fair and commodious plains and prairies of the south lay before them, yet too many points of apparent connexion present themselves to admit of our consigning their adoption of such a route to the category of impossibilities.

It is from the Rio Gila onwards that we are first enabled to perceive definite traces of the course of the migration into the regions of the south; the indications of the different stages of its progress increase with its entrance upon Mexican territory, but we yet possess only sparingly the means of identification. The first immigrants who appeared in the north of Mexico brought with them the so-called Toltecatl civilization, the work of the races of the great Nahoa family.* The space which this mode of culture gradually occupied is shown by the two great *casas* on the Rio Gila † and in Chihuahua, by el Zape in Du rango and la Quemada in Zacatecas. Whether all these tribes maintained at the same time a separate or subordinate condition cannot now be ascertained; but in proportion to the genera growth and improvement would be the provection of their boundaries to the south and the inducements offered to further migrations.‡

By the term immigration of a race we are not generally to understand a single immigration only; we should rather comprehend under the common name, for example, of the Nahoa immigration, all expeditions of the branches belonging to that family. These expeditions took place successively at intervals, and therefore, from first to last, suppose a considerable length of time, within which a large number of these branches successively arrived on the banks of the Rio Grande and Rio Gila. This consideration is of the more weight, since, through an improper conception of such terms, which involve, as we see, an idea of multitude, a source of confusion is introduced into the already sufficiently obscure history of the American migration.

Over the Nahoas floats still much mystical obscurity; the epoch of their appearance in the northern parts of Mexico admits as yet of no accurate determination; it must have taken place, however, at a much earlier time than the commencement of our Christian era. Our knowledge respecting these partially mythical people amounts to little more than enough to justify us in regarding them as perhaps the founders of the stone-works in northern Mexico. If we admit that the age of the civilization indicated in the region of the Mississippi reaches back 2,000 years, it is not impossible that the Nahoas were also the builders of the earth-mounds in North America, or at least belonged to the race from which these works proceeded. As regards the stone structures of the

* The first volume of the Abbe Brasseur de Bourbourg's *Histoire des Nations Civilizées du Mexique et de l'Amerique Centrale*, (Paris 1857–'59, 4 vols.,) treats at length of the Nahoas. This work is very full and authoritative, though scarcely written with all the discrimination and clearness which was desirable.

† At the confluence of the Rio San Pedro with the Rio Gila, which is itself an affluent of the Rio Colorado, emptying into the Gulf of California, (Mar Vermeio.)

‡ Orozco y Berra; Geogr. d. las leng. de Mexico, p. 125.

great *casas* of el Zape and la Quemada, we cannot but infer that their builders must have been long permanently settled in those districts, which accords much better with later researches than the assumption of many that the immigrating tribes had merely halted in the places for a few years, perhaps a quarter of a century, and in that time had erected these monuments.*

With the appearance of the Toltecs some light begins to dawn on the history of the migration.† This cultivated people, kindred also to the Nahoa family, entered Mexico in the seventh century of our era. The year 648 is generally reckoned that of their appearance; only Clavigero carries it back to the year 596. But here, likewise, is to be understood not merely a single immigration, but several of the same family. This plurality of migrations presents no light difficulties to the historian as well as the geographer. Orozco y Berra, whose linguistic and historical labors merit our highest consideration, has exerted himself to thorw some light on the perplexities of the Toltec immigration. In the hieroglyphic annals relating to this subject we find the record of several itineraries, which all adapt themselves to the immigration in question. These itineraries, partially provided with chronological notices, correspond in some points, but disagree in others, and thus prove that they do not belong to one and the same expedition. It has already been shown that the routes of such migrations are always essentially influenced by the geographical conformation of the country.‡ Hence, at least three different routes of immigration are distiguishable on Mexican soil. The first and most important lies in the western part of the country, and may be traced from Sinaloa to Nicaragua along the coast of the Pacific ocean. The mountain chain which separates Sinaloa from Durango, and stretches to the banks of the Rio Tololotlan, marks the direction of this route for many leagues. First, the course of the Rio Grande de Santiago,§ then that of the Zacatula,‖ diverted a part of the wanderers from the direct route and led them to the lake of Chapala,¶ where they established themselves, while others, following in like manner the valleys of water-courses advanced to Guerrero, and gradually made their progress to the fertile table land of Cuernavaca,** and still later to Puebla and Tlaxcalla.††

The monuments of both the grand *casas*, el Zape and la Quemada, plainly show the second and middle route which led, on the east of the above-mentioned mountain chain, directly through Chihuahua, Durango and Zacatecas, to Jalisco,

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*I had myself long held this opinion till a closer consideration of the facts satisfied me that the space of a quarter of a century could not have sufficed for the erection of monuments which are in fact the scanty remains of extensive settlements, probably of whole cities. It is hardly consistent with the logic of migration that a people in such circumstances should find it expedient to found cities, only to abandon them in so short a time.

†The populations which were settled in Mexico before the arrival of the Toltecs might also be possessed of an ancient native civilization; among these pre-Toltec races are to be numbered the Olmecs, the Otomi, whose monosyllabic speech forms an exception to the polysynthetic idioms of America, the Totonacks on the eastern terraces of the Cordilleras, the Mixtecs on the coast of the Pacific ocean, the Tarasks in the greater part of Michoacan, and the Zapotecs in Oaxaca.

‡ Mountain ranges, river courses and sea-coasts prescribe limitations; while for half civilized, pastoral and agricultural tribes, the fertility, streams, lakes, and dales of new countries offer special attractions.

§ This river, called also the Tololotlan, which is formed at Salamanca by the junction of the Rio Hondo de Lerma and the Rio Laxa, flows through the lake of Chapala and empties into the Pacific ocean.

‖ The same name was borne by a city at the mouth of the Rio de las Balzas, on the Pacific, which was long the capital of a flourishing and to the end of the fifteenth century independent state.

¶ The lake of Chapala is the most considerable of those of the whole elevated plateau, and is distant forty miles in a northwestwardly direction from the capital.

** The ancient Quanhnahuac.

†† Tlaxcalla wa sonce called Chalchinhapan; also Texcalticpac, (*i. e.*, end of the stone houses,) still later Texcallan, (Rock city,) and finally Tlaxcallan, (Breadland, from Tlaxcall—maize-bread,) on account of the fruitfulness of its situation.

on the shore of lake Chapala, whence the immigrants would easily attain the Mexican table land.

On the east we discern along the Rio Grande del Norte the traces of a third route of immigration which, according to Orozco, seems not to have touched upon New Leon and Tamaulipas. Whether the widely strewn ruins of an ancient city on the banks of the Rio Panuco* in the territory of the warlike Huaxtecs,† are associated with this route, I am not able to determine. Further to the south the traces again come to view, and may be followed to their junction with the rest on the high lands of Anahuac.

From these facts it results, in conformity with the accurate indications of Orozco, that, while heretofore but one Toltec immigration into Mexico has been admitted, several have in reality taken place at different times and by different lines of advance. To determine how many there may have been is at present impossible. It is certain, however, that through the supposition of a single immigration of a single migratory race it has been necessary to reconcile expeditions, facts, and discoveries, with unconformable names, with incoherent and contradictory conclusions, with a chronology in the highest degree perplexed, resulting, of course, in the most inextricable confusion. When we come, on the other hand, to analyze the phenomena so as to refer them to the facts as they really occurred, the difficulties vanish, riddles are solved, and all falls into the natural order, which from the first should never have been lost sight of.

After it had lasted four centuries, famine, pestilence, and civil war put an end, about the year 1018, to the Toltec monarchy in Mexico.‡ The larger remnant of those Toltecs who had been spared by the calamities of the times left their country, and, according to the historian Don Fernando d'Alvarado Ixtlilxochitl, took refuge in southern Guatemala and Nicaragua, but few families remaining in their desolated land. Soon thereafter, but in the opinion of others a century later, the barbarous Chichimecs made their appearance on the high lands of Anahuac. Just as the Toltecs had occupied the sites in northern Mexico, abandoned by the Nahoas, in their progress southward, so had the Chichimecs who followed been long established upon the deserted lands of the Toltecs after the withdrawal of the latter to Anahuac. When now again the Toltecs abandoned the land which they had inhabited for four hundred years, the Chichimecs still followed at their heels; nor was it necessary that they should enter as conquerors, since it needed only to take possession of the deserted homes of the Toltecs. These Chichimecs in the mean time, who had so long lived adjacent to the Toltecs, had, through their intercourse with the latter, in some degree discarded the rudeness of their manners at the date of their appearance on the elevated plateau of Mexico; as had been the case with the barbarians who overthrew Rome, but at the same time imbibed the culture of the civilized Romans.

The races which had thus far appeared in Mexico had pertained to one great branch of languages, the Nahoa, a dialect of which was also spoken by the Toltecs; and this, till very recently, was supposed to have been the case likewise with the Chichimecs, although the Mexican historians Ixtlilxochitl and Torquemada

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* *Americanische Altenthümer*, (Westland, edited by Dr. C. Andree, Bremen, 1851, vol. i, p. 129–139,) a very interesting article.

† They inhabit the district known by the name of Huaxteca in the north of Mexico, and, on the Gulf from Tuxpan to Tampico, a tract which embraces the northern part of the State of Vera Cruz and a part of San Luis Potosi bordering thereon.

‡ It is impracticable to enter here into the history of the separate countries; the inquirer will find further and accurate information in the works of Don José de Acosta, H. Benzoni, Benaduci Boturini, Brasseur, Brownell, T. de Bussière, Carlier, M. Chevalier, Fr. Clavigero, Epinosa, Gallatin, Greg. Garcia, Gomara, Humboldt, Ixtlilxochitl, Kingsborough, Las Casas, Mayer, Moke, Motolinia, Munoz, Oviedo y Valdez, Man. Payno, Prescott, Ramirez, Ruxton, Sahagun, Ant. de Solis, Solorzano, Ternaux-Compans, Tezozomoc, Torquemada, Tyler, Uhde, Ulloa, Veytia.

had contested the fact. But Don Francisco Pimentel* has lately vindicated the opinion of these two writers, and proved that the Chichimecs possessed an entirely peculiar language;† consequently they could not have belonged to the Toltec family. In view of this now admitted fact, I can but deem it highly improbable that the Chichimecs arrived in northern Mexico by the same route with the other immigrating tribes, which were all related to one another, being members of the same race, and having probably inhabited a common land of nativity. But if the home of the Chichimecs was a different one from that of the other wandering populations, this circumstance would have naturally prescribed to them a different path to the south. Perhaps they constitute the branch which, in the opinion of many, advanced, as has been already mentioned, on the further side of the Rocky mountains to the banks of the Gila.

The Chichimecs had scarcely established themselves in Mexico when the immigration of those seven tribes took place, which are known by the common name of Nahuatlacas, and which spoke the same tongue as the Toltecs, to whose family also they belonged. Of these seven tribes, six first made their appearance, following closely on one another, namely, the Xochimilcos, Chalcas, Tepanecas, Tlahuicas, Colhuas, and Tlaxcaltecas; while the seventh, the celebrated Aztecs, arrived after a longer interval. In the mean time the Acolhuas also had appeared upon the theatre, but had soon become intermingled with the Chichimecs, who already possessed the country, forming by this union the kingdom of Acolhuacan.

It was in 1090 that the Aztecs, the seventh of the Nahuatlaca race, and speaking the Nahuatl tongue,‡ issued from Aztlan, their original but unknown home, which, as above stated, is to be sought in the region of the great American lakes. The Aztec annals which have come down to us enable us partially to trace this family in its wanderings, even if it be not possible for us to identify the geographical sites to which tradition gives a name. The year 1091 finds the Aztecs at Quahuitl-Icacan. In 1116 they are at Quinehuayan-Oztotl and Quinehuayan-Chicomoztoc. Hence they made their way to Teo-Culhuacan, though it is impossible to distinguish the intermediate places which they visited. Their history begins to clear up with their arrival in Acahualtzinco, where they sojourned nine years, (1143.) Their onward march brought them into the province of Cohuatlycamac, not far from the ancient Tollan, then to Coatepec, which they reached in 1174, and where they settled for some time. Finally, between the years 1186 and 1194, about a century after they had issued from their home of Aztlan, the Aztecs appeared on the table land of Anahuac, never more to forsake it. It falls not within the scope of this essay, which deals merely with the migration, to speak of the further history of the Aztec kingdom, whose splendor and power are otherwise well known.§

In the south of Mexico, as before remarked, there existed an ancient civilization, which we would designate as the *palencan*, and which, agreeably to the latest researches, is to be regarded as the oldest in America. The regions

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* In his valuable work, *Cuadro descriptivo y comparativo de las lenguas indigenas de Mexico*, (Mexico 1862–'65,) of which two volumes have appeared.

† A grammar of the Chichimec language was composed by the monk Diego Diaz Pangua, who was born in Durango, and died in the year 1631; it was entitled *Arte de la lengua Chichimeca.* He was the author also of a Chichimec dictionary, and a catechism in the same language; his works, however, have remained in manuscript.

‡ Brasseur notices the similarity of sound between the words Nahuatl and the English Know-all, their signification, moreover, being the same.

§ Brasseur de Bourbourg, *Hist. de Nations Civ. du Mexique*, vol. I. Since the study of the remains of Mexican constructions is of the highest importance for the history of the country, I enumerate the following places where the most remarkable ruins of this kind occur: The districts of Papantla, Cholula, and Teotihuacan, where terraced pyramids are found; Mapilca, Tusapan, Isla de Sacrificios, Puente Nacional, Misantla, in the State of Vera Cruz, Tezcuco, Tezcocingo, Xochicalco in the State of Mexico, Quiotepec, Zachila, Coyula, S.Juan de los Cués, Mitla in Oaxaca.

where its monuments occur, Chiapas, Guatemala, and Yucatan,* are inhabited by the family which use the Maya-Quiché dialect, which some suppose to be derived from the Toltecs.† But Orozco y Berra contests this supposition, and justly, because the Quiché, with its two related idioms, the Cachiquel and Zultuhi, belongs to a family of languages wholly different from the Nahoa; and the Maya was limited to the little-known peninsula of Yucatan and some contiguous territories. It is impossible to determine the time at which the immigration of the Maya tribes commenced, or which of them first appeared in the country. From the multiplicity of the dialects of this group of languages, we are justified in concluding that Mayas and Quiches had, at a very early epoch, separated from one another, the language of the former changing less than that of the latter, who, at a later period, came into contact with the Toltecs, living north of them. Of the primitive population which erected the numerous monuments‡ of the region in question, history has no further knowledge than that they were the possessors of a civilization far in advance of that of the races which we are inclined to regard as the aboriginal people of Mexico, though it bore no resemblance to that of the northern Nahoas.

As the Toltecs, after the fall of their dominion in Mexico, migrated to Guatemala and Nicaragua, they probably pressed further south the populations which then occupied those countries, and finally founded in Central America itself several small kingdoms, traces of which are not yet extinct. By late and very competent inquirers the hypothesis has been advanced that the surprising constructions of Central America and Yucatan belong not to so early a period as is commonly assumed, but are the work of later times, perhaps even of the civilized Indian races which inhabited the land at the arrival of the Spaniards, or at least of their immediate predecessors. The traveller, Stephens, and the skilful observer, Brantz Mayer, have advanced, in support of this view, many weighty and ingenious considerations; and I was myself inclined to adopt the opinion that at least the migrating Toltecs, who had already earned so much distinction as architects in their own country, should be regarded as the builders of these remarkable monuments, thus assigning to them an age not more remote than the twelfth century. But a more careful comparison of these remains with those of undoubted Toltec origin, added to the recently ascertained fact that the hieroglyphic characters found on them bear not the least resemblance to the rest of the picture-writing of Mexico, place it beyond all doubt that these monuments belong to an earlier and, indeed, very ancient epoch.§ The traces of the mi-

* On the history of Yucatan, see Cogolludo, *Historia de l'Yucatan*, Madrid, 1688, fol. —; Fancourt, *History of Yucatan from its discovery to the close of the 17th century*, London,

† The distinguished archeologist, G. E. Squier, goes so far as to derive the Toltecs from Central America.

‡ In Yucatan alone, thanks to the diligent researches of MM. Norman and Stephens, may now be counted the ruins of fifty-four cities. The most remarkable are Uxmal, Chichen-Itza, Maxcanu, Sacbey, Xampon, Sanacte, Chunhuhu, Labpakh, Iturbide, Mayapan, Ticul, Nochacab, Xoch, Kabah, Sabatsche, Labna, Kenick, Izamal, Saccacal, Tekax, Akil, Mani, Macoba, Becanchen, Peto in the interior, Tulloom, Tancan, and the island Cozumel in the east; in Chiapas, Palenqué, Utlatan, Ococingo; in Guatemala, the ancient ruins of the Quichés in the districts of Totonicam and Quesaltenango; in Honduras, the monolithic pyramids of Copan; and quite in the east of the country, the old city of Olancho.

§ For the history of Central America recourse may be had to the following writers: Aviles: *Historia de Guatemala*, 1866.—Pelaez: *Memoria para la Historia del Antique Reyno de Guatemala*, 1852, 3 vols.—Villagutierre: *Historia de la Conquista y Rendicion de varias Provinciae del Reyno de Guatemala*, Madrid, 1701, fol. —.—Ximenes: *Las Historias del Origen de los Indios de esta Provincia de Guatemala, Traducidas de la lengua Quiché*, Vienna, 1857; an excellent and most important work of my highly honored benefactor, Dr. C. v. Scherzer. Oersted, *l'Amerique Centrale, Kopenhague*, 1863.—Reichard: *Nicaragua*, Braunschweig, 1854.—Reichard: *Centro America*, 1851.—Scherzer: *Wanderungen Dusch die Mittelamericanischen Freistaaten, Nicaragua, Honduras, und S. Salvador*, Braunschweig, 1857.—Squier: *Nicaragua, its People, Scenery, Monuments*, New York, 1852, 2 vols.—Squier: *Notes on Central America*, New York, 1855.—Squier: *The Archeology and Ethnography of Nicaragua*, (Transact. American Ethnol. Society, vol. III, 1852.)—Wagner and Scherzer: *Die Republik Costa Rica*, Leipsig, 1856.

gration are not susceptible of being followed in Central America with the same certainty as in the more northern Mexico. To the philologist alone it pertains to investigate, in the manifold dialects of the former country, the intermixture of the Nahuatl element as it spread southwardly to Nicaragua, and thus to supply incontrovertible proofs of the migration.

A wide space separates the ruins of Palenqué and Olancho from the elevated lands of Cundinamarca. A dense obscurity rests upon the countries of the isthmus, Costa Rica, Chiriqui, Panama, and Choco; but imperfectly explored in relation to geography, they are still more unknown in relation to archeology. On the luxuriant and picturesque shores of Lake Nicarargua we lose, as it were, the bond which might connect the populations of the north and south continents of America. For ten degrees of latitude nearly all traces of the past vanish, and it is only on the South American main land, in the mountains of New Grenada, that we again perceive a gleam of dawning civilization, which was for a long time almost wholly overlooked. Yet a doubt can scarcely exist but that the migration continued to advance in those little known districts of the isthmus. In Chiriqui have recently been discovered and explored some of the burial places of the natives,* which are similar to those known in Peru by the name of huacas. Were these districts more thoroughly explored, it is quite probable that the search would be rewarded by the discovery of sepulchral or other vestges of civilization, calculated to throw more light on the fortunes of these provinces.

On the South American continent the points which offer themselves for the prosecution of our inquiry respecting the migration are of far less frequent occurrence than in the northern portion of America. That part of the Cordilleras of the Andes, whose western foot is bathed by the Rio Magdalena, and which, stretching in a northeast direction, forms the lofty plains of Bogotá and Tunja, which further south rises into the profound solitudes of Paramo de Suma Paz, was inhabited at the time of the discovery of these countries by the Chibcha race,† whom the Spaniards improperly denominated Muyscas.‡ These people still exist in the country, but have almost entirely forgotten their ancient language, which was soft, flowing, and copious.§ As far as is known to myself, nothing has as yet been found to enlighten us respecting an immigration of this race, unless it be their peculiar legend of the mythical personality of Bochica, which certainly possesses a striking resemblance to the Quetzalcohuatl of the Toltecs. There can be little doubt that the obscurity which rests on the past history of the Chibchas will be greatly dispelled by the indefatigable ardor of Dr. Ezequiel Uriocoechea,‖ of Santa Fé de Bogota, who has applied the re-

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* King Merritt: ***Report on the huacas or ancient graveyards of Chiriqui***, read before the Ethnological Society, New York, 1860.

† The land of the Chibchas comprised the lofty plateaus of Bogotá and Tunja, the valleys of Fusagasuga, Pacho, Caqueza, and Tensa, together with the whole of the districts of Ubaté, Chiquinquirá, Moniquirá, and Leyva, and could in ancient times number a population of 2,000 to the square league, thus falling little if at all behind the most thickly peopled tracts of Europe. The Chibchas were a people of somewhat advanced civilization, possessing temples of the sun with stone pillars, remains of which have been found in the valley of Leyva. Monuments peculiar to the Chibcha architecture occur in the Cojines of Tunja, the Calzada of the plain of Pataqué and the ruins of Infieritò. The Chibchas, however, seem never to have attained the degree of culture of the more southern Peruvians.

‡ A. V. Humboldt reproves this use of the term Muysca, which in the Chibcha language simply means "man." (Ans. d. Nat., ii, 270.)

§ The best book which we yet have respecting this remarkable people is that of Joaquin Acosta, ***Compendio historico del discubrimiento y colonizacion de la Nueva Grenada;*** Paris, 1848.

‖ To this writer, who is at present occupied in publishing the "Monumenta Chibcharum," we already owe the valuable work ***Memoria sobre las antiquedades neo-granadinas***, Berlin, 1854.

cources of a rare sagacity to the solution of this difficult problem. Only when we shall have verified the place of the Chibchas in the circle of the other American races, can we hope to detect the traces of any possible shifting of the population. On the neighboring high lands of Quito, on the other hand, we meet with some indications of such an occurrence, if but faint ones. As early as the year 980, Caran Schyri conquered the kingdom of Cara. The Cara race inhabited the coast from the river Charapotó to Cape S. Francisco, and is said to have come from over the sea. History does not distinctly inform us whence it sprung, nor at what time its colonization took place.* Nevertheless, it might be possible to connect its appearance with the common American migration, which, if it generally reached South America, arrived there in a much attenuated condition.

Peru is to the southern division of America what Mexico is to the northern. It is, moreover, the only point of South America in which distinct indications of an immigration present themselves. In the absence of these, the hypothesis might perhaps be tenable that the migration terminated at the lake of Nicaragua without having attained the South American mainland. But the fact that the numerous relics of civilization in Peru clearly point to two different periods of culture, and proceed from two different races of men, one of which immigrated at epochs capable of being historically determined, could scarcely be explained without bringing this incident into connexion with the general migration. While the origin of the Peruvian kingdom, like that of all these races, loses itself in a cloud of myths;† while many extinct tribes, as the Chinchas or Yungas, and the Huancas, may still be identified; while the extensive structures near Tiahuanuco on Lake Titicaca,‡ the ruins of the once gorgeous temple of Pachacamac § on a mountain by the sea, to the southeast of Lima, and perhaps also the ruins of the ancient Caxamarquilla,|| are derived from the once powerful Aymara nation, which inhabited the land in early times, as their chulpas¶ or sepulchral mounds still attest, there arrived on the elevated plains of Quito and Cuzco, in the year 1021 of our era, a warlike people, whom I propose to call, from their rulers, the Inca race. Manco Capac, the first of these lords of the sun, founded, in 1050, the city of Cuzco,** while under his successor, Sinchi Roca, the Aymaras withdrew further west, in order to evade the domination of the Incas. Under the following emperors, Lloque Yupanqui and Mayta Capac, war was made upon the Aymaras, who were compelled still to recede. Finally, in the fifteenth century, the Inca Capac Yupanqui drove the native races before him

* The first king founded the city of Cara on the bay of Caraqui between the years 700 and 800. The Caras conquered under Schyris I, to Schyris VIII, all the small separate states which existed in that region, and continued as the kingdom of Quitu under the dynasty of the Schyris of Cara until the year 1487, when the powerful Inca Hayna Capac conquered their kingdom and reduced it to a province of Peru. (Velasco, *Historia de Quito.*)

† Many of which recall those of the highlands of Anahuac, as, for example, that of Queralcohuatl.

‡ On two islands of the lake are seen the temples of the sun and moon, together with the cloister of the virgins dedicated to the moon. On the genuine ruins of Tiahuanuco is everywhere recognized the symbol of the sun, and I am therefore persuaded that the Incas did not, as is commonly thought, bring with them the worship of the sun, but, rather, found it already in the country and adopted it.

§ Pachacamac (that is, creator of the earth, from patcha, earth, and camac, participle of camanê, I make) was the supreme God of the Indians of Peru.

|| In a part of the valley of Rimac.

¶ The graves of the Changos and Quechuas are called huacas, which means, according to some writers, place of weeping; (see, on the signification of this word, Prescott's Hist. of the Conquest of Peru, vol. i, p. 93;) they occur in great numbers, especially in the hills of Cocotea, Tamba, and Mejillones, in the envirous of Iquique, and in the Morro of Arica.

** Cuzco, the Ccozçco of the Incas, signifies in the Quechua language, "navel"—that is, centre of the kingdom.

as far as Chili, where, at length, the brave tribe of Chalchaqui Indians opposed to the ambitious Incas an effectual resistance.*

If it be considered that the Inca dynasty in Peru presents more than one resemblance to the civilized Toltecs of Mexico; that in the traditions of both many points of coincidence occur, and, what perhaps is of most weight, that the time of the appearance of the Incas in Peru agrees in a surprising degree with that at which the Toltecs, after the subversion of their kingdom, might have reached the country, we shall scarcely be disposed to reject the surmise of A. v. Humboldt, that in early epochs a procession of the later Inca races took place over the table land of Mexico. Their arrival in Peru would thus constitute the last step of the great American migration. The ingenious arguments by which Humboldt weakened without overthrowing his own conjecture are well known, leaving the question open whether the Toltec races which passed into the southern hemisphere proceeded by the way of the cordilleras of Quito and Peru, or through the plains lying east of the Andes to the banks of the Marañon.

In the remaining tracts of the South American continent there occur but few traces of civilization of a quite inferior grade. The migration, as far as is yet known, did not penetrate into these regions, but was confined to the western part of the range of the Andes. Even in southern Chili and Patagonia nothing of such an event is distinguishable. All that is found in the low lands of South America pertaining to human culture consists of a sort of runic characters, either carved or painted on stone, which are of very doubtful signification. We meet with such hieroglyphic symbols not far from Tijuca, in the diamond district of Minas Geraes, in Brazil; further north, near Ceará; in the province of Bahia, on the Serra do Anastasio, and in the scarcely explored wilderness of Piauhy; they occur next in the province of the Alto Amazonas, on the banks of the Rio Hyapura; near Montalegre, in the province of Pará, and on the Rio Negro and Rio Ucagari, or Uaupes. The tradition of the Zome, existing among the Indians of the province of Bahia, and which bears some resemblance to the Bochica of the Chibchas, and the Quetralcohuatl of the Toltecs, would seem to show that an element of culture had penetrated into the country from without.† Some countenance seems to be given to this supposition by the statement of the oldest authorities that, of the two groups of inhabitants who occupied Brazil at the time of its discovery, one regarded the other as being more recent intruders. If it be true, however, that there once existed in the low lands of Brazil, as in the interior of the United States of North America, the seat of a not wholly uncivilized race of aborigines, the fact must at least ascend into such remote prehistoric times that every trace thereof may have well been obliterated.‡ "For,"

* Respecting the history of Peru see the following works—Balboa: *Histoire du Perou.*—Beauchamp: *Histoire de la Conquêta et des Revolutions de Perou*, Paris, 1808.—Bellesteros: *Ordenanzas del Peru*, Lima, 1685.—Colpaert: *Etude sur le Perou*, Paris.—Desjardines: *Le Perou avant la Conquête Espagnole*, Paris, 1858.—Fernandez: *Historia, del Peru*, Sevilla, 1571.—Garcilasode la Vega: *Comentarios Reales*, 1609, and *Hist. General del Peru*, 1616.—Gosse: *Dissertation sur les Races qui Composaient l'Ancienne Population du Perou*, Paris, 1860.—Markham: *Travels in Peru and India*, London, 1862.—Marmontel: *Les Incas*, Francefort, 1792.—Peralta: *Lima Fundada, Poema Eroycs*, Lima, 1732.—Perez: *Notice sur les Guipos des Anciens Peruviens.*—(Rev. Amer., 1864.)—Prescott: *Hist. of the Conquest of Peru*, New York, 1847.—Rivero y Tschudi: *Antiquedades Peruanas*, Vienna, 1851.—Xerez: *Conquista del Peru*, Salamanca, 1547.—Zarate: *Historia del Descübrimiento y Conquista del Peru*, Sevilla, 1537.

† Handelmann, *Geschichte von Brasilien*, Berlin, 1860, p. 7–11.

‡ There is, of course, no literature properly relating to the ancient history of these countries, but scattered notices may be found in the following works, in which, moreover, the whole of their history since the conquest is comprised—Angelis: *Collecrion de obras y Documentos relativos a la Historia Antique y Moderna de las Provincias del Rio de la Plata*, Buenos Ayres, 1836.—Arenales: *Noticias Historicas y Descriptivas sobre el gran pais del Chaco y Rio Bermejo*, Buenos Ayres, 1833.—Baralt: *Resümen de la Historia de Venezuela*, Paris, 1841.—

as an accurate observer of the country writes, "the whole island which lies between the Atlantic ocean on one side, and the river Amazon, along the Rio da Madeira, and the Paraguay and Parana on the other, seems never to have been penetrated by the slightest degree of civilization ; nowhere is there a manifestation of mental activity or aim rising above the merest physical necessities ; and the Indians of the Brazilian forests, who no doubt receded before the peculiar civilization of the western Cordilleras of South America, as they recoil to-day, as long as is possible, from that which approaches them on the east, constitute the most unreasoning and incapable of creatures."* In the absence of other monuments, no hypothesis, I think, dissenting from this statement of facts, can be advanced, much less defended. The scattered inscriptions which have been found seem to afford no sufficient ground for the adoption of sweeping conclusions, which, in the interests of science, moreover, it were better to avoid.

An attempt has been made in the above pages to follow the memorable phenomenon of the migration of tribes in America through its entire course, or, in other words, its geographical propagation, and to present the reader with a comprehensive, if reduced, sketch of it. Should we at last be forced to confess that darkness rather than light has met us at the different points of our investigation, there yet remains the hope that, through the persevering labors of the learned world of America, our knowledge in this hitherto but little favored field of inquiry will ere long receive new and gratifying accessions.

APPENDIX TO THE FOREGOING ARTICLE.

[The investigations given in this paper relate exclusively to the migrations and not to the origin of the American races, and, therefore, the author need not have introduced the vexed question of the latter. But since he has done so, it may be proper to give in this place a brief statement of the scientific considerations which favor the generally received opinion as to the original seat of the human race.

The spontaneous generation of either plants or animals, although a legitimate subject of scientific inquiry, is as yet an unverified hypothesis. If, however, we assume the fact that a living being will be spontaneously produced when all the physical conditions necessary to its existence are present, we must allow that, in the case of man, with his complex and refined organization, the fortuitous assembly of the multiform conditions required for his appearance would be extremely rare, and from the doctrine of probabilities could scarcely occur more than at one time and in one place on our planet; and further, that this place would most probably be somewhere in the northern temperate zone. Again, the Caucasian variety of man presents the highest physical development of the human family,

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Barco Centinera: *Argentina y Conquista del Rio de la Plata*, Poema ap. Barcia.—Beauchamp: *Histoire du Brazil*, Paris, 1815.—Charlevrix: *Geschicte v. Paraguay*, Wien, 1830.—Escilla Zunige: *La Auracana*, Poema Eroico, Madrid, 1733, fol—.—Havestadt: *Chilidugu*, Monast. Westphal., 1777.—Lallemant: *Histoire de la Colombie*, Paris, 1826.—Molina: *Saggio Sulla Storia Civile del Chile*, Bologna, 1787.—Oña: *Arauco Domado.*, Poema, Madrid, 1605.—Ovalle: *Historia del rey de Chile*, Roma, 1648, fol. —.—Oviedo y Baños, *Historia de la Conquista y Poblacion de la Provincia Venezuela*, Madrid, 1723.—*Revista Trimensal del Instituto Hist. Geog. e Ethnographico do Brasil*, Rio de Janeiro.—*Revista de Buenos Ayres*, since 1863.—Smyth: *The Auracanians*, New York, 1855.—Ternaux-Compans: *Notice Historique sur la Guyane Francaise*, Paris, 1843.—Tessillo: *Guerra de Chile*, Madrid, 1617.

* Dr. Avé Lallemant, of Lubeck, in a letter addressed to me, January 27, 1866, on the primitive condition of the natives of Brazil, see further the instructive work, *Os Indigenus do Brazil Perante a Historia*, Rio de Janiero, 1859, by the learned and ingenious Portuguese poet, J. Magelhaes, residing at that time, as Brazilian minister, at Vienna; also that of V. L. Baril de la Hure, entitled *Les Peuples du Brésil avant la Dcouvêrte de l'Amerique.*

and as we depart, either to the north or south, from the latitude assumed as the origin of the human race in Asia, we meet with a lower and lower type until at the north we encounter the Esquimaux, and at the south the Bosjesman and the Tierra Fuegian. The derivation of these varieties from the original stock is philosophically explained on the principle of the variety in the offspring of the same parents, and the better adaptation and consequent chance of life, of some of these to the new conditions of existence in a more northern or southern latitude.

Furthermore, as the author has shown, the migrations on the American continents have principally been from north to south, and it is an interesting fact, fully confirmed by the observations of the explorers of the route for the Russian American telegraph, that the waters of Behring's Straits are frozen over probably every year as late as April, and that intercourse, at present, is constant by means of canoes in summer between the Asiatic and American sides. As another fact relating to the same question, we may state that, while the Asiatic projection near Behring Straits is almost a sterile rocky waste, the opposite coast presents a much more inviting appearance, abounding in trees and shrubs. Moreover, the climate, when we pass southward of the peninsula of Alaska, is of a genial character, the temperature continuing nearly the same as far down as Oregon. The mildness of this temperature, and the descent of the isothermal line, or that of equal temperature along the coast, are due to the great current called the gulf stream of the Pacific, which carries the warm water of the equator along the eastern coast of Asia, thence across to the opposite coast of America, and along the latter on its return to the equator. The action of this current, which does not appear to have been considered by the ethnologist, must have had much influence in inducing and determining the course of the migrations. We may add to the foregoing that the present inhabitants of the countries contiguous to Behring's Straits on the two sides, in manners, customs, and physical appearance, are almost identical. J. H.]

INDIAN POTTERY.

BY CHARLES RAU.

In former times, when the aboriginal inhabitants of this country were still in possession of their own lands, and their mode of living had not been changed by the intrusion of the pale-faced Caucasian, the art of pottery was practised by them to a considerable extent. This branch of industry lost, however, much of its importance among the Indians so soon as they discovered the superiority of the vessels of metal, which they obtained in trafficking with the whites, and the durable kettle of iron or copper soon replaced the fragile and far less serviceable cooking utensil of clay. The beginning of the decline of this aboriginal art is, therefore, of an early date, and at the present time it may be considered as almost, if not entirely, extinct among the tribes still inhabiting the territory of the United States, excepting some in New Mexico and Arizona, who have not yet abandoned the manufacture of earthenware. As late as 1832, when Mr. Catlin visited the nations of the Upper Missouri, he found the Mandans still diligently practising the ceramic art; but the ravages of the small-pox have reduced their number to a few, and it is probable that vessels of clay are no longer made in those regions.

The Iroquois, of New York, those survivors of the once powerful Confederation who have escaped the fate of being driven toward the setting sun, and are still permitted to dwell upon their native soil, have ceased long ago to fabricate earthen vessels. So I am informed by Dr. Peter Wilson, De-jih-non-da-weh-hoh, grand chief of the Six Nations of New York. "The manufacture of pottery," says my correspondent, "has long since been discontinued among our people; like most other utensils, clay vessels have been superseded by utensils of the manufacture of the race who introduced among us the implements which are more durable and convenient. Such implements and other articles used among us only remain, or are being manufactured, as are not superseded by articles which the ingenuity of the pale face replaces." The same remark can probably be applied to the other tribes east of the Rocky Mountains.

That the fabrication of earthenware was once carried to a great extent among the Indians, is shown by the great number of sherds which lie scattered over the sites of their former villages and on their camping places; but they are, perhaps, nowhere in this country more numerous than in the "American Bottom," a strip of land which extends about one hundred miles along the Mississippi, in Illinois, and is bounded by the present bank of that river and its former eastern confine, indicated by a range of picturesque wooded hills and ridges, commonly called the "Bluffs." This bottom, which is on an average six miles wide and very fertile, was formerly the seat of a numerous indigenous population, and abounds in tumular works, cemeteries, and other memorials of the subdued race. Among the lesser relics left by the former occupants may be counted the remnants of broken vessels, which occur very abundantly in various places of this region. These fragments are, however, mostly small; and, according to my experience, entire vessels are not found on the surface, but frequently in the ancient mounds and cemeteries, where they have been deposited with the dead as receptacles for food, to serve on their journey to the happy land of spirits.

About six years ago, while living in the west, I was much gratified by the discovery of a place in the American Bottom where the manufacture of earthenware was evidently carried on by the Indians. The locality to which I allude is the left bank of the Cahokia creek,* at the northern extremity of Illinoistown, opposite St. Louis. At the point just mentioned the bank of the creek is somewhat high and steep, leaving only a small space for a path along the water. When I passed there for the first time, I noticed, scattered over the slope or protruding from the ground, a great many pieces of pottery of much larger size than I had ever seen before, some being of the size of a man's hand, and others considerably larger; and, upon examination, I found that they consisted of a grayish clay mixed with pounded shells. A great number of old shells of the *unio*, a bivalve which inhabits the creek, were lying about, and their position induced me to believe that they had been brought there by human agency rather than by the overflowing of the creek. My curiosity being excited, I continued my investigation, and discovered at the upper part of the bank an old fosse, or digging, of some length and depth, and overgrown with stramonium or jimson weed; and upon entering this excavation, I saw near its bottom a layer of clay, identical in appearance with that which composed the fragments of pottery. The excavation had unmistakably been dug for the purpose of obtaining the clay, and I became now convinced beyond doubt that the fabrication of earthen vessels had been carried on by the aborigines at this very spot. All the requisites for manufacturing vessels were on hand; the layer of clay furnished the chief ingredient, and the creek not only supplied the water for moistening the clay, but harbored also the mollusks whose valves were used in tempering it. Wood abounded in the neighborhood. All these facts being ascertained, it was easy to account for the occurrence of the large fragments. Whenever pottery is made, some of the articles will crack during the process of burning, and this will happen more frequently when the method employed in that operation is of a rude and primitive character, as it doubtless was in the present case. The sherds found at this place may, therefore, with safety be considered as the remnants of vessels that were spoiled while in the fire, and thrown aside as objects unfit for use.

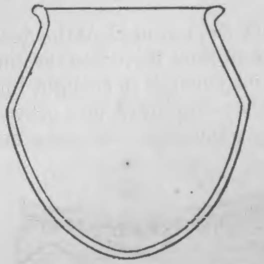
I did not succeed in finding the traces of a kiln or fireplace, and it is probable that the vessels were merely baked in an open fire, of which all vestiges have been swept away long ago. The occurrence of the broken pottery was confined to a comparatively small area along the bank, a space not exceeding fifty paces in length, as far as I can recollect. They were most numerous in the proximity of the old digging, and at that place quite a number of them were taken out of the creek into which they had fallen from the bank. Farther up the creek I saw another excavation in the bank, of much smaller dimensions, and likewise dug for obtaining clay. Among the shells and sherds I noticed many flints which had obviously been fashioned to serve as cutting implements; they were, perhaps, used in tracing the ornamental lines on the vessels or in smoothing their surfaces.

I did not find a single complete vessel at this place, but a great variety of fragments, the shape of which enabled me to determine the outline of the utensils of which they originally formed parts. This was not a very difficult matter,

* This creek runs in a southwardly direction through Madison county and a part of St. Clair county, and empties into the Mississippi four miles below St. Louis, near the old French village of Cahokia.

especially in cases when portions of the rim remained. Figures 1 and 2 represent (in sections through the middle) the prevailing forms of the vessels.

1



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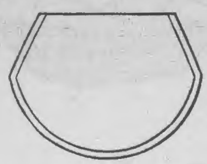

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The rim, it will be seen, is formed into a lip and turned over, in order to facilitate suspension; sometimes, however, it is cut off abruptly, as in Fig. 3. Some of the vessels—more especially the smaller ones—were provided with ears, like Fig. 4;* others had the outer rim set with conical projections or studs, both for convenience and ornament; and a few of the fragments exhibit very neatly indented or notched rims. In size these vessels varied considerably; some measured only a few inches through the middle, while the largest ones, to judge from the curvature of the rims, must have exceeded *two feet in diameter*. The bottom of the vessels mostly seems to have been rounded or convex. I found not a single flat bottom-piece. This, however, may be merely accidental, considering that flat-bottomed vessels were made by the Indians. The appearance of the fragments indicates that the earthenware was originally tolerably well burned, and the fracture exhibits in many instances a reddish color. But, as the art of glazing was unknown to the manufacturers, it is no wonder that the sherds, after having been imbedded for many years in the humid ground, or exposed to rain and the alternate action of a burning sun and a severe cold, are now somewhat brittle and fragile; yet, even when new, this aboriginal earthenware must have been much inferior in compactness and hardness to the ordinary kind of European or American crockery.

The thickness of the fragments varies from one-eighth to three-eighths of an inch, according to the size of the vessels, the largest being also the strongest in material. But in each piece the thickness is uniform in a remarkable degree; the rims are perfectly circular, and the general regularity displayed in the workmanship of these vessels renders it almost difficult to believe that the manufacturers were unacquainted with the use of the potter's wheel. Such, however, was the case. I have already mentioned that the clay used in the fabrication of this earthenware is mixed with coarsely pulverized unio-shells from the creek; only a few of the smaller bowls or vases seem to consist of pure clay. The vessels were covered on the outside, and some even on both sides, with a thick coating of paint, either of a black, dark brown, or beautiful red color, and

* I possess a small food vase of this shape, which was taken out of an old Indian grave on the "Bluffs," near French village, six or seven miles east of Illinoistown. It was, perhaps, made at the very place which I have described.

in some fragments the latter still retains its original brightness. Only *one* color, however, was used in the painting of each article. It is evident that the coloring preceded the process of baking, and the surfaces thus coated are smooth and shining, the paint replacing to a certain extent the enamel produced by glazing.

That the aboriginal potters on the Cahokia creek did not neglect the decorative art in their manufactures, is shown by the ornamental lines traced on the surface of their crockery. The simplest form of ornamentation consists in straight lines running around the vessel parallel to the rim; but they employed also other combinations of lines, of which figures 5, 6, 7, and 8 are examples. In some instances

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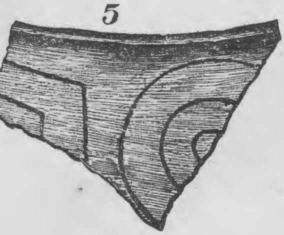

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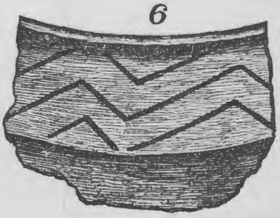

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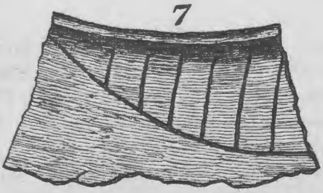

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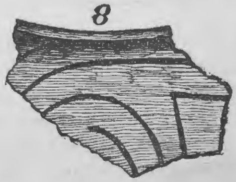
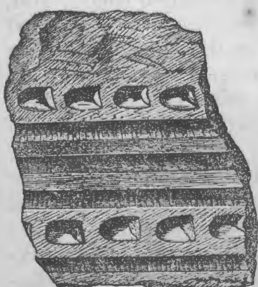


the *inside* only was ornamented. The lines are mostly drawn with great regularity, and sometimes one-eighth of an inch wide, with a corresponding depth. I obtained, however, from the deposit at the Cahokia creek one small fragment, which exhibits a much higher degree of skill in the art of decoration than any of the others found at the same place. Figure 9 represents it in full size. This specimen is about three-sixteenths of an inch thick, and consists of clay with an admixture of pulverized granite, the components of which—quartz, feldspar, and mica—can be plainly distinguished in the fracture. It is well baked and of a light-gray color. The ornamental lines and notches are impressed, or, perhaps, scooped out, with the greatest accuracy, and the vessel, when complete, must have presented a very good specimen of aboriginal ceramic art. Whoever compares the annexed drawing with Fig. 5 on Plate 46 of the "Ancient Monuments of the Mississippi Valley," by Squier and Davis, will find that the originals of the representations are nearly alike in point of ornamentation. The latter drawing delineates a part of a vase found in one of the ancient mounds of Ohio. Having seen the best specimens of "mound" pottery obtained during the survey of Messrs. Squier and Davis, I do not hesitate to assert that the clay vessels fabricated at the Cahokia creek were in every respect equal to those exhumed from the mounds of the Mississippi valley, and Dr. Davis himself, who examined my specimens from the first-named locality, expressed the same opinion.

9



One of the methods employed by the Indians in the manufacture of earthenware was, to weave baskets of rushes or willows, similar in shape to the vessels they intended to make, and to coat the inside of these baskets with clay to the required thickness; the baskets, after being destroyed by the fire, left on the outer surface of the vessels peculiar impressions, resembling basket-work, which produce a very pleasing effect, and replace ornamentation to a certain extent.*

With this method the potters on the Cahokia creek were likewise acquainted, for I found a few pieces of their ware bearing the marks just mentioned. This sort of pottery, however, is not mixed with pounded shells, but with sand, and is much better baked than the other kind; it has a pale-reddish appearance, and is not painted.

Lastly, I have to enumerate among the objects of baked clay obtained from the deposit in the American Bottom, two articles resembling the beaks of large birds, perhaps detached pot or pan handles; a flat piece, forming the base of the figure of some animal, of which, unfortunately, the tail only remains, and the remnant of a toy canoe. The last-named specimen, probably made by some affectionate Indian mother for her little son, was picked up from the bottom of the creek.

The question now arises, who were the makers of these manufactures of clay? I simply ascribe them to the Cahokia Indians, who dwelt, until a comparatively recent period, on the banks of the creek that still bears the name of their tribe. Concerning the antiquity of the manufactures described on the preceding pages, I am not prepared to give an estimate. Only a hundred years may have elapsed since they were made, yet it is also possible that they are much older. The appearance of the fragments rather indicates a modern origin.

The writings of early, and even comparatively modern, authors on North America are not deficient in particulars relating to the art of pottery among the natives. According to their statements, those tribes were most advanced in the manufacture of earthenware, who inhabited the large tracts of land formerly called Florida and Louisiana, which comprise at present the southern and southwestern States of the Union; and their testimony is fully corroborated by the character of such specimens of pottery from those parts as have escaped destruction, and are preserved in the collections of the country.† The Natchez, on the Lower Mississippi, perhaps the most civilized among the North American Indians, and supposed to be related to the Aztecs, were skilful potters. So we are told by the anonymous Portuguese gentleman called the "Knight of Elvas," who accompanied, towards the middle of the sixteenth century, De Soto on his adventurous expedition through a great portion of the North American continent, and became afterwards the chronicler of that bold Spaniard's exploits. In the province of Naguatex, he states, clay vessels were made "which differed very little from those of Estremoz or Montemor." These two towns in Portugal are noted for their earthenware.‡ *Du Pratz* mentions the "Ecore Blanc," on the

* Bartram describes a vessel of this kind which he extracted from a shell-mound on one of the islands near the coast of Georgia.—*Bartram's Travels*, Dublin, 1793, p. 6.

† "In some of the southern States, it is said, the kilns in which the ancient pottery was baked are now occasionally to be met with. Some are represented still to contain the ware, partially burned, and retaining the rinds of the gourds, &c., over which they were modelled, and which had not been entirely removed by the fire. In Panola county, Mississippi, are found great numbers of what are termed *pottery kilns*, in which are masses of vitrified matter, frequently in the form of rude bricks, measuring twelve inches in length by ten in breadth. It seems most likely that these *kilns* are the remains of the manufactories of the later tribes—the Choctaws and Natchez—'who,' says Adair, 'made a prodigious number of vessels of pottery, of such variety of forms as would be tedious to describe and impossible to name.'"—*Ancient Monuments of the Mississippi Valley*, Washington, 1848, p. 195.

‡ Virginia Richly Valued, by the Description of the Maine Land of Florida, her next Neighbour, &c. Written by a Portugall Gentleman of Eluas, emploied in all the Action, and translated out of the Portugese by Richard Haklvyt, London, 1609, (reprint of 1812, Supplement,) p. 750.

Mississippi, as one of the localities where the Natchez obtained clay for their pottery, and likewise *ochre* to paint it. "When coated with ochre," he says, "it becomes red after the burning." Elsewhere, in speaking of the manufacture of clay vessels by the natives of Louisiana, the same author remarks: "The women make pots of an extraordinary size, jars with a small opening, bowls, two-pint bottles with long necks, pots or jugs for preserving bear oil, holding as much as forty pints, and, finally, plates and dishes in the French fashion."*

Dumont, who likewise describes the manners of the people inhabiting the extensive country formerly called Louisiana, has left a more minute account of the method they employed in making earthenware. He says: "After having amassed the proper kind of clay and carefully cleaned it, the Indian women take shells which they pound and reduce to a fine powder; they mix this powder with the clay, and having poured some water on the mass, they knead it with their hands and feet, and make it into a paste, of which they form rolls six or seven feet long and of a thickness suitable to their purpose. If they intend to fashion a plate or a vase, they take hold of one of these rolls by the end, and fixing here with the thumb of the left hand the centre of the vessel they are about to make, they turn the roll with astonishing quickness around this centre, describing a spiral line; now and then they dip their fingers into water and smooth with the right hand the inner and outer surface of the vase they intend to fashion, which would become ruffled or undulated without that manipulation. In this manner they make all sorts of earthen vessels, plates, dishes, bowls, pots, and jars, some of which hold from forty to fifty pints. The burning of this pottery does not cause them much trouble. Having dried it in the shade, they kindle a large fire, and when they have a sufficient quantity of embers, they clean a space in the middle, where they deposit their vessels and cover them with charcoal. Thus they bake their earthenware, which can now be exposed to the fire, and possesses as much durability as ours. Its solidity is doubtless to be attributed to the pulverized shells which the women mix with the clay."†

Adair, more than a century ago a trader with the tribes who occupied the southern portion of the present Union, confines himself to the following remarks:

"They make earthen pots of very different sizes, so as to contain from two to ten gallons; large pitchers to carry water; bowls, dishes, platters, basins, and a prodigious number of other vessels of such antiquated forms as would be tedious to describe and impossible to name. Their method of glazing them is, they place them over a large fire of smoky pitch-pine, which makes them smooth, black, and firm. Their lands abound with proper clay for that use."‡

Loskiel, who describes the manners of the Delawares and Iroquois, states that they made formerly kettles and cooking-pots of clay, which they mixed with finely pounded shells, and burned until they became black throughout. Quite large pieces of their pots, he says, in which the pounded shells could still be seen, were often found in such places where the Indians had dwelt in ancient times; but after the arrival of the Europeans very light kettles of brass had generally been introduced among them.§ Thus we see that these tribes began at an early period to neglect the manufacture of clay vessels.

A very good account relating to the art of pottery, as formerly practised by the western tribes, is given by *Hunter*. "In manufacturing their pottery for cooking and domestic purposes," he says, "they collect tough clay, beat it into powder, temper it with water, and then spread it over blocks of wood, which have been formed into shapes to suit their convenience or fancy. When sufficiently dried, they are removed from the moulds, placed in proper situations,

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* *Du Pratz, Histoire de la Louisiane*, Paris, 1758, vol. i, p. 124, and vol. ii, p. 179.

† *Dumont Mémoires Historiques sur la Louisiane*, Paris, 1753, vol. ii, p. 271, &c.

‡ Adair's History of the American Indians, London, 1775, p. 424.

§ Loskiel, Geschichte der Mission der evangelischen Brüder unter den Indianern in Nord-Amerika, Barby, 1789, p. 70.

and burned to a hardness suitable to their intended uses. Another method practised by them is, to coat the inner surface of baskets made of rushes or willows with clay, to any required thickness, and when dry, to burn them as above described. In this way they construct large, handsome, and tolerably durable ware; though latterly, with such tribes as have much intercourse with the whites, it is not much used, because of the substitution of cast-iron ware in its stead."

"When these vessels are large, as is the case for the manufacture of sugar, they are suspended by grape-vines, which, wherever exposed to the fire, are constantly kept covered with moist clay. Sometimes, however, the rims are made strong, and project a little inwardly quite round the vessel so as to admit of their being sustained by flattened pieces of wood slid underneath these projections and extending across their centres."*

Lastly, I will quote here the remarks made by *Catlin* relating to the fabrication of earthenware among the Mandans. "Earthen dishes or bowls are a familiar part of the culinary furniture of every Mandan lodge, and are manufactured by the women of this tribe in great quantities, and modelled into a thousand forms and tastes. They are made from a tough black clay and baked in kilns which are made for the purpose, and are nearly equal in hardness to our own manufacture of pottery, though they have not yet got the art of glazing, which would be to them a most valuable secret. They make them so strong and serviceable, however, that they hang them over the fire, as we do our iron pots, and boil their meat in them with perfect success. I have seen some few specimens of such manufacture, which have been dug up in Indian mounds and tombs in the southern and middle States, placed in our eastern museums and looked upon as a great wonder, when here this novelty is at once done away with, and the whole mystery; where women can be seen handling and using them by hundreds, and they can be seen every day in the summer also, moulding them into many fanciful forms, and passing them through the kilns where they are hardened."†

The largest vessels made by the Indians, it seems, were those used in procuring salt by evaporation near salt springs. *Du Pratz* mentions a locality in Louisiana where the aborigines collected salt in earthen vessels made on the spot, before they had been supplied with kettles of metal by the French.‡ The "Knight of Elvas" likewise describes the method of salt-making employed by the natives. "The saline below St. Genevieve, Missouri," says *Brackenridge*, "cleared out some time ago and deepened, was found to contain wagon loads of earthenware, some fragments bespeaking vessels as large as a barrel, and proving that the salines had been worked before they were known to the whites."§

I had occasion to examine a fragment of a vessel of this kind sent to Dr. Davis in 1859 by Mr. George E. Sellers, who obtained it at the salt springs near Saline river, in southern Illinois, a locality where salt was formerly made by the Indians. Several acres, Mr. Sellers states, are covered with broken vessels, and heaps of clay and shells indicate that they were made on the spot. They presented the shape of semi-globular bowls with projecting rims, and measured from thirty inches to four feet across the rim, the thickness varying from one-half to three-quarters of an inch. This earthenware had evidently been modelled in baskets. The fragment sent to Dr. Davis is a rim-piece three-quarters of an inch thick, consisting of three distinct layers of yellowish clay, mixed with very coarsely pounded shells. It is solid and heavy, and must have been tolerably well baked. The impressions on the outside are very regular

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* *Hunter's* Manners and Customs of several Indian tribes located west of the Mississippi, Philadelphia, 1823, p. 296, &c.

† *Catlin's* North American Indians, London, 1848, vol. i, p. 116.

‡ *Du Pratz*, vol. i, p. 307.

§ *Brackenridge*, Views of Louisiana, Pittsburg, 1814, p. 186.

and really ornamental, proving that those aboriginal potters were also skilful basket-makers.

It would be erroneous to suppose the art of manufacturing clay vessels had been in use among *all* the tribes spread over this widely extended country; for, though exhibiting much general similarity in character and habits, they differed considerably in their attainments in the mechanical arts. This was the consequence of local circumstances, such as configuration and quality of the soil, climate, and other natural conditions which influenced, or rather determined their mode of life. Some of the North American tribes, who did not understand the fabrication of earthen vessels, were in the habit of cooking their meat in water set to boiling by means of heated stones which they put into it, the receptacles used in this operation being large wooden bowls, water-tight baskets, or even the raw hides of animals they had killed. The Assinaboins, for example, cooked in skins. "There is a very curious custom among the Assinaboins," says *Catlin*, "from which they have taken their name—a name given them by their neighbors from a singular mode they have of boiling their meat, which is done in the following manner: When they kill meat, a hole is dug in the ground about the size of a common pot, and a piece of the raw hide of the animal, as taken from the back, is put over the hole, and then pressed down with the hands close around the sides, and filled with water. The meat to be boiled is then put in this hole or pot of water; and in a fire, which is built near by, several large stones are heated to a red heat, which are successively dipped and held in the water until the meat is boiled; from which singular and peculiar custom, the Ojibways have given them the appellation of Assinaboins or Stone-boilers."

"This custom," he continues, "is a very awkward and tedious one, and used only as an ingenious means of boiling their meat, by a tribe who was too rude and ignorant to construct a kettle or pot. The traders have recently supplied these people with pots; and even long before that, the Mandans had instructed them in the secret of manufacturing very good and serviceable earthen pots, which together have entirely done away the custom, excepting at public festivals, where they seem, like all others of the human family, to take pleasure in cherishing and perpetuating their ancient customs."* Yet, the Assinaboins may, nevertheless, have been acquainted with the art of pottery; for they are a detached branch of the Dacotahs, probably of the Yankton band of that nation, and we have the testimony of *Carver*, for instance, that the Naudowessies—that is, the Dacotahs or Sioux—made "pots of clay, in which they boiled their victuals."†

Some of the tribes of New Mexico and Arizona, as, for example, the Mojaves and Pimas, still manufacture pottery; but the Pueblo Indians of those districts are especially noted for their fictile fabrics. "They manufacture, according to their aboriginal art, both for their own consumption and for the purposes of traffic, a species of earthenware not much inferior to the coarse crockery of our common potters. The pots made of this material stand fire remarkably well, and are the universal substitutes for all the purposes of cookery, even among the Mexicans, for the iron castings of this country, which are utterly unknown there. Rude as this kind of crockery is, it nevertheless evinces a great deal of skill, considering that it is made entirely without lathe or any kind of machinery. It is often fancifully painted with colored earths and the juice of a plant called *guaco*, which brightens by burning."‡

Speaking of that region, I must not omit to allude, at least, to the numerous fragments of ancient pottery which occur on the Little Colorado, Colorado Chiquito, and Gila, especially among ruins, and are often highly decorated and painted with various colors, exhibiting a style of workmanship differing from

* *Catlin*, vol. i, p. 54.

† *Carver's* Travels, London, 1781, Harper's Reprint, p. 154.

‡ *Gregg's* Commerce of the Prairies, Philadelphia, 1851, vol. i, p. 278.

and surpassing that which prevailed on the eastern side of the Rocky Mountains. Descriptions of these relics, however, would exceed the intended limits of this essay, and, moreover, they have been given elsewhere, together with speculations concerning the character of the manufacturers.*

Some years ago, while visiting northern Europe, I had occasion to see many specimens of ancient pottery deposited in the archæological collections of that district, and having previously become acquainted with the character of North American aboriginal pottery, it afforded me great pleasure to trace the similarity in the fictile manufactures of both continents. Where the external conditions of life were similar among men, their inventive powers were necessarily exerted in a similar manner. We have the testimony of *Tacitus*, that the inhabitants of Germany lived, about two thousand years ago, much in the manner of the North American Indians, before the original habits of the latter had undergone the changes resulting from their intercourse with Europeans or their descendants; and it is, therefore, quite natural that both races should have resorted to the same, or, at least, similar means to satisfy their wants. The ancient flint implements of northern Europe bear a close resemblance to those formerly made by the natives of this country, and a like conformity is exhibited in the character of their manufactures of clay.

The aborigines of North America, to recapitulate the general characteristics of their pottery, formed their vessels by hand, modelling them sometimes in baskets, and were, as far as we know, unacquainted with the art of glazing. They mixed the clay used in their pottery either with pounded shells or sand, or with pulverized silicious rocks; mica also formed sometimes a part of the composition. Their vessels were often painted with ochre, producing various shades, from a light yellow to a dark brown, or with a black color. They decorated their pottery with lines or combinations of lines and dots, and embellished it also by notching the rims, or surrounding them on the outside with studs, or in various other ways. Their vessels exhibited a great variety of forms and sizes, and many of them had rounded or convex bottoms. They hardened their earthenware in open fires or in kilns, and notwithstanding the favorable statements of some authors, it was much inferior in compactness to the common crockery manufactured at present in Europe or America, and has even, in some instances, an appearance as though it had merely been dried in the sun.

The same details, somewhat modified, are applicable to the specimens of ancient pottery preserved in the museums of northern Germany, and frequently obtained from ancient burial places, where they had been placed by the side of the dead, or as receptacles of their ashes. Many of these vessels were evidently fashioned by hand; but others, especially the larger ones, bear the unmistakable traces of the lathe, the use of which was, perhaps, known to the German tribes before they had intercourse with the Romans. The clay composing these vessels is strongly mixed with quartz sand, to which very frequently mica is added, probably with a view to impart more solidity to the mass. Ancient German clay vessels, after being exhumed, are soft and so fragile that a somewhat rough handling destroys them at once. The roots of trees and shrubs have often grown through those that are dug up in woods, which obviously shows that they were not sufficiently burned; for well-burned clay, like that composing the pipes of Roman aqueducts and the bricks of the middle age, resists humidity even better than many kinds of stone. When exposed to the air, these vessels become tolerably hard within a few hours; but in rare instances only they have that peculiar ring which characterizes well-burned earthenware. It seems, therefore, that they were not burned in kilns, but merely in strong open fires.† Many

* The reader is referred to an excellent chapter by *Mr. Thomas Ewbank*, entitled "Illustrations of Indian Antiquities and Arts," in the third volume of Pacific Railroad Reports, Washington, 1856.

† *Klemm*, Germanische Alterthumskunde, Dresden, 1836, p. 167.

of the urns are painted with yellow or red earths, or a black color, the latter pigment being sulphuret of molybdenum. May not the same substance, which occurs in many localities of the United States, have been used by the Indians for blackening their pottery? An analysis would easily decide the question. The same parallel and zigzag lines, or rows of dots, which decorate Indian vessels, are also seen on the ancient pottery of the north of Europe, and of other parts of that continent. They constitute the simplest elements of ornamentation, and have, therefere, everywhere been employed by man when he made his first attempts in the art of decoration. On the surface of a few ancient vases or urns found in Germany I noticed those markings which present the appearance of basket-work; I was, however, in doubt whether they were impressions produced by the inside of baskets, or simply ornamental lines traced on the wet clay. Yet, even in the latter case, it would seem that this kind of ornamentation was suggested by the former practice of modelling vessels in baskets. I further saw some apparently very old specimens of pottery with rounded bottoms. The oldest vessels of all nations, who practised the potter's art, probably exhibited that shape, the model of which was furnished by nature in the gourd and other fruits presenting rounded outlines. A flat bottom, therefore, would denote a progress in the ceramic art. Other particular features common to the pottery of both, the ancient inhabitants of Germany and the aborigines of North America, might be pointed out; but the fictile fabrics of the former exhibit, on the whole, more elegance of outline, and therefore indicate a higher state of art. The similarity in the manufactures of men in various climates is greatest when art is in its very infancy among them. In the course of gradual development, the primitive forms common to mankind become more and more indistinct, and finally emerge into those varied and characteristic shapes which reflect the individuality of nations.

ARTIFICIAL SHELL DEPOSITS OF THE UNITED STATES.

BY D. G. BRINTON, M. D., OF WEST CHESTER, PA

In the annual Smithsonian report for 1864, (pp. 370–374,) a description is given of an artificial shell deposit in Monmouth county, New Jersey. As this branch of American Archæology is of importance to both geologists and antiquarians, the following notes upon it may be of value to the students of each of these sciences. The exclusively artificial character of many of these deposits, even of very considerable size, was first prominently brought before the scientific public by Mr. Lardner Vanuxem, in the proceedings of the American Association of Geologists and Naturalists for 1840–'42, (pp. 21–23.) The existence of enormous accumulations of the shells of the *Ostrea virginica* and *Venus mercenaria* on the shores of the Chesapeake and its affluent streams, on the Jersey shore and Long Island, was discussed, and various proofs of their formation by the aboriginal tribes pointed out. These proofs may be briefly summed up as follows: First. Valves of the same animal are rarely found together. Second. Arrow-heads, fragments of pottery, and charcoal are mixed with the shells *in situ naturali*. Third. The shells are broken, and frequently charred. Fourth. The substratum of the deposit is the same as the surrounding soil. Fifth. The deposits are at the mouth and shores of water-courses, where the shell-fish abound. Sixth. There is an absence of stratification and older fossils.

It seems hardly necessary to adduce evidence from the old voyagers to show that in the commissariat of the native coast tribes esculent shell-fish constituted an important item. Cabeza de Vaca describes the accolents of the Gulf of Mexico as dwelling in houses of mats, "built on heaps of oyster shells," (Ramusio Viaggi, tom. III, fol. 317,) and the first settlers of Maryland record, with pleasurable recollections, the "oisters, broil'd and stewed," that the savages offered them in profusion.—(Relation of Maryland, 1634, p. 18, in Shea's Southern Tracts.)

By far the majority of these monuments of a past race are mere refuse heaps, the debris of villages of an icthyophagous population, showing no indications of having been designedly collected in heaps, true analogues of the kjoekkenmoeddings of the age of stone; but in other instances it would appear that the Indians collected them into artificial mounds, forming a class of antiquities heretofore unnoticed by archæologists. They are found in great numbers along the southern coast, especially in Georgia and Florida, where I have examined many of them. At the landing of Fernandina, on Amelia island, the summit of the bluff is covered with a layer of artificially deposited shells, extending about two hundred yards upon the bay and one-fourth of a mile inland, varying in depth from one to four feet. The shells are in many places so rotten as to fall to pieces at the touch, some showing fractures made at the edges, as if in opening, while others have obviously been subjected to the action of fire. Charcoal, bones of fish and animals, and arrow-heads are scattered irregularly through the mass. This is the general character of many such deposits that I have examined in other parts of the peninsula of Florida; for instance, on both banks of the St. John's, at its mouth, on Anastasia island, opposite St. Augustine, on the Manatee river, and along the shores of Tampa bay. The two most remarkable in the State are near New Smyrna, on the Mosquito lagoon, and near the mouth of the Crystal river, on the Gulf coast. Of the first of these I have a description written by a gentleman of intelligence resident on the spot, and of the second a report of an investigation made by the State geologist of Florida, Mr. F. L. Dancy, to whom I am also indebted for the former account. That near New Smyrna, called Turtle mound, is, says my informant, "thirty feet high, composed almost altogether of separate oyster shells, it being rare to find the two valves together. There are, also, conch and clam shells, both of which are, however, exceedingly scarce. That it is artificial there is no doubt on my mind. Some eight or ten years since we experienced a gale in this section of country which caused that portion of the mound facing the river, the steepest part, to fall and be washed away. Being there a few days afterwards, I took considerable pains to examine the face of it, and found, as low as the bottom and as high up as I could observe, numberless pieces of Indian pottery and quantities of bones, principally of fish, but no human ones; also charcoal and beds of ashes. The one on which I reside, opposite New Smyrna, is precisely of the same formation. Having had occasion some time back to dig a hole into it six or eight feet deep, I found precisely the same contents that I have described at Turtle mound, with the addition of some few flint arrow-heads." Of the remarkable mound on Crystal river, four miles from its mouth, Mr. Dancy writes: "The marsh of the river at that point is twenty yards wide to the firm land, at which point this mound commences to rise. It is on all sides nearly perpendicular, the faces covered with brush and trees, to which the visitors have to cling to effect an ascent. It is about forty feet in height, the top surface nearly level, about thirty feet in diameter, and covered with magnolia, live oak, and other forest trees, some of them four feet in diameter. Its form is that of a truncated cone, and, as far as can be judged from external appearance, it is composed exclusively of oyster shells and vegetable mould. These shells are all separated. The mound was evidently thrown up by the Indians for a lookout, as the gulf can be distinctly seen from its summit. There are no oysters growing at this time within four or five miles of it." This is evidently altogether different from the mere refuse heaps referred to elsewhere.

On both banks of the St. John's river, as far south as Lake Harney, the traveller finds shell bluffs, sometimes closely simulating artificial erections, quite regular in outline, not unfrequently twenty-five or thirty feet high. They are composed almost exclusively of the helix, with a few unios. The Brock House at Enterprise is built upon such a natural deposit, and south of it, between the large and small sulphur springs, some two hundred yards from the lake, there is another, and a third on the left bank, near the exit of Lake Harney, about a quarter of a mile from a very large and ancient Indian mound. The native tribes chose the shell bluffs of natural formation as favorite burial spots, and hence ancient relics are constantly found in them, but a practiced eye can readily detect the disturbance of the natural deposition of the shells. This is strikingly exemplified in the shell bluffs on Tampa bay, at the mouth of Manatee river, composed chiefly of a species of *Pyrula*. The sea face has been washed away, and the ancient graves, containing bones, charcoal, and unbroken utensils are distinctly seen cutting through the clean, original strata. On the contrary, in the refuse shell-heaps human bones are never seen, and very rarely unbroken arrow-heads or pottery.

While attached to the army of the Cumberland, during the late rebellion, I had abundant opportunity of ascertaining the prevalence of shell-heaps along the Tennessee river and its tributaries. They are very frequent at and above the Muscle Shoals, and are composed almost exclusively of the shells of the fresh-water muscle, (*Unio virginiana, Lamarck*?) Close to the famous Nick-a-jack cave is the railway station of Shellmound, so called from an uncommonly large deposit of shells, probably left by the Cherokees, who so long used this spot as one of the headquarters of the Overhill tribes. It was taken by our troops as a military post, and embankments thrown up around the summit of the mound. The excavations made for this purpose abundantly proved its wholly artificial origin. In all instances I found the shell-heaps close to the water courses, on the rich alluvial bottom lands. The mollusks had evidently been opened by placing them on a fire. The Tennessee muscle is margaratiferous, and there is no doubt but that it was from this species that the early tribes obtained the hoards of pearls which the historians of De Soto's exploration estimated by bushels, and which were so much prized as ornaments. (See Irving, Conquest of Florida, p. 246.) It is still a profitable employment, the jewellers buying them at prices varying from one to fifty dollars.

The great size of some of these accumulations may furnish some conception of the length of time required for their gradual accretion, and consequently of the period during which these shores have been inhabited. That at the mouth of the Altamaha river covers ten acres of ground, and contains about 80,000 cubic yards. Many of them equal in cubical contents the largest mounds of the Ohio valley. That at the mouth of the Pickawaxent creek, eighty miles from Washington, is so considerable that a kiln was erected many years since for the purpose of burning the shells into lime, and may be still in operation. On account of their occasional very considerable magnitude, some geologists have been inclined to deny or discredit their artificial origin, and have pointed to the existence of similar deposits presenting indubitable characteristics of lacustrine and litoral formation, such as the oyster-shell heaps of southern New Jersey, (see Second Annual Report of the Geological Survey of New Jersey, pp. 76, 84,) and the beds of *Gnathodon cuneatus* at Mobile harbor, (American Journal Science, second series, vol. xi, p. 164,) but the distinctions I have pointed out will dispel any doubt upon this subject.

In estimating the comparative archæological value of these monuments of the aboriginal tribes, with a view to ascertain the amount of light they may be expected to throw on the habits and social condition of their constructors, we must place them far below the similar remains in the north of Europe. These latter reveal to us a race of whom we possess few or no other memorials, and are a principal source of information concerning nations whose very existence was forgotten; but my somewhat extended studies of this class of American antiquities convince me that they are the work of known tribes of Indians concerning whom we possess many and superior sources of information, and that they only serve to illustrate and confirm the knowledge we already have of their social status and means of subsistence.

SKETCH OF ANCIENT EARTHWORKS.

BY I. DILLE, OF OHIO.

THE archæological remains of the Mississippi valley, traced from the lakes south and southwesterly, exhibit a progressive change of structure and outline from the most simple to the most complicated. At the mouth of Chagrin river, on Lake Erie, a bold promontory is surrounded by a fosse and parapet, which were obviously intended for defence. The choice of the site and the character of the structure are such as a military leader of a rude people would be likely to adopt. From the lake to central Ohio the ancient remains are exclusively of a defensive or of a sepulchral character, and no mystery attaches to the purpose for which they were intended. Still further north, on the southern shore of Lake Superior, the working of the copper mines, with extensive explorations, leaves us in no doubt as to the object of the copper implements and ornaments found all over that country. Nothing has been observed which indicates that the makers understood the fusing of metals, or of casting them into desired forms. With the exception of the bowl of a spoon, found in the great mound at Grave creek, on the Ohio, and of doubtful origin, no useful article is known to have been made of copper by the ancient people. This metal was probably regarded as a malleable and durable stone, to which could be given any desired form by the hammer alone, and its use was ornamental, or connected with official or sacerdotal dignity.

But there is a large class of antiquities, comprising fully four-fifths of all these remains found from central Ohio southward and westward, east of the Mississippi, that we cannot refer to any purpose or use of any races of men, or of their institutions, religious, political, or social.

Of this class the ancient works in the vicinity of Newark, Ohio, are the most easterly and northerly. The great extent and variety of these works in Licking and Perry counties show clearly that this section of country contained a large and industrious population. Sepulchral mounds, differing in form and height, abound everywhere, and are especially numerous in the southeastern part of Licking and northeastern part of Perry.

The great stone mound about eight miles south of Newark, and about one mile east of the reservoir on the Licking summit of the Ohio canal, was one of the most remarkable structures in the State. It was composed of stones, in their natural shape, as they were found on the adjacent grounds, laid up, without cement, to the height of from 40 to 50 feet, upon a circular base of 182 feet diameter. This was surrounded by a low fosse, and parapet of an ovate form, with a gateway on the east end, leaving a large open area on the west end of the mound, within the enclosure. In the early settlement of this part of the State the old pioneers say this mound was a great resort for rattlesnakes; and it is told of an old woman who lived near by, that in the spring of the year she used to rake up the forest leaves around the foot of the mound, and when the weather became warm enough for the snakes to come out to bask in the sun she would set fire to the leaves, which so infuriated the snakes that they would, in their madness, rush into the fire and perish.

When the canal reservoir, which is seven or eight miles long, was made, it was deemed necessary to protect the east bank from abrasion with stone, so that it might be used for the purposes of navigation by tow-boats. The nearest stones available for this purpose were those of the great mound. During the years 1831–'32 from 50 to 75 teams were employed in removing this mass of material. It is said that from 10,000 to 15,000 wagon loads were carried away. As the workmen approached the base of the mound they discovered 15 or 16 small earthmounds around or near the circumference of the base, and a similar one in the centre. Nothing else was noticed worthy of attention, as was then thought, and the small mounds were levelled to the ground.

No further examination was made into any of these mounds until about 1850, when some of the farmers in the vicinity devoted a part of a day to an exploration of them. They opened two which were not covered by the rubbish. In one they found human bones, with some fluviatile shells, and in the other they dug down to a hard, white fire-clay. Two or three feet below the surface of this they came to a trough similar to those formerly made in the west for feeding pigs, by hollowing out a log. This was overlaid by small logs of wood to serve as a cover, and in it was found the skeleton of a man, around which appeared the impression of a coarse cloth. Within this trough were fifteen copper rings and a breastplate of the same metal. The wood of the trough and covering was in a state of good preservation. The clay which environed it was not only impervious to air, but also to water. Satisfied with their discovery, the explorers covered up the ancient coffin, and all remained undisturbed until the summer of 1860, when the late David Wyrick, with the aid of two or three men, raised the rude coffin with its covering of logs and brought them to town. The small round logs that overlaid this sarcophagus were so well preserved that the ends showed the axe marks, and the steepness of the kerf seemed to indicate that some instrument sharper than the stone axe, found throughout the west, had been employed to cut them. The woody fibre of the coffin remained firm, but the annulars were partially separated, so as to be split apart.

In the following November Mr. Wyrick, with five other men, met at this spot and made further exploration into this mound. They found several articles of stone, among which was a stone box enclosing an engraved tablet in unknown characters; a mortar, made of the fine-grained sandstone of the country; and several neatly-worked stones, similar to a mason's plumb bob. The engraved stone has given rise to much speculation as to its origin and signification, with much division of opinion.

The central mound in the base of the large stone mound was opened in the presence of the writer of this article. A great number of human bones were found in it, but no other relics worthy of note. These bones were all deposited in, or covered by, the humus peat from the adjacent swamp, now covered by the reservoir. It is remarkable that all of those mounds which have been explored contained earth from a distance. The pipe or fire clay is found in place at Flint Ridge, six or eight miles distant, and none of that peculiar variety is known in place nearer. This ancient work we know was for burial use, but we know not what faith or what personal distinction induced the construction of a mausoleum of so much labor.

Just west of the town of Newark two confluents of the Licking river, the Raccoon and South Fork, unite, and between them is a rich valley of some thousands of acres, which is the site of the most elaborate and extensive ancient works known in the west. From the streams the land rises by natural terraces to the general level of the plain, and previous to the construction of the canal on the second alluvial bench, on the east side of the great works, was a series of low mounds, so low, indeed, that a casual observer would not suspect them to be mounds. The canal embraced these mounds in its range, and the first lock west of the town was constructed on the site of one of them. In excavating the pit for it in the spring of 1828, a deposit of human bones was discovered, of from twelve to fifteen skeletons, upon which were laid large plates, consisting of mica of the finest quality in regard to transparency and size. Now, it is well known that no mica is found in place in the Mississippi valley, and the question arises as to where such plates as those, of from eight to ten inches in length, by from five to seven inches wide, and from one-half to one inch thick, are to be found. It is evident that they must have been brought from a great distance, perhaps hundreds of miles, and since the quantity, estimated at from twelve to twenty bushels in this place alone, was so great, the labor of transporting it could not have been trivial. It would seem that this mineral was highly esteemed by the mound builders, since many large plates of it have been ploughed up for several miles around. To what use these plates of transparent mica were put, or what superstition was connected with them, is another riddle that remains to be read.

The structure and plans of the principal ancient earthworks of the Mississippi valley have been described and delineated by Atwater, Davis, and Squier, and others; yet much remains to be done in determining the precise purposes for which they were constructed. We may assume as an obvious fact that the mound builders, in the peculiarity of every form, curve, and line of their works, had a purpose or design, but what that design was in many cases is yet to be discovered. Wise or foolish, it is locked up in the grave with its projectors, and there it will probably remain. That this people was progressive, or that the ancient works were constructed by different races having each peculiarities of their own, is evident; for, on going south, we find a change in the outlines and plans of these remains. In southern Tennessee the writer has examined several earthworks, and found that the large circular enclosures, the great squares, and the long parallel lines have given way to another class of structures. At Fort Pickering, two miles below Memphis, on a high bluff overlooking a long and graceful bend of the Mississippi, stands what the writer supposed to be an altar, which is one of the most conspicuous objects on the river. It is in the form of a parallelogram, which by stepping was found to be about two hundred and twenty feet long by one hundred and twenty feet wide, and by estimation from twenty to twenty-five feet high, with steep sides and a flat upper surface. In 1849 this mound was in a good state of preservation, and on digging into it the material of which it was composed was found to be calcined clay, changed by burning into a brick color, the clay before being burnt having evidently been mixed with the twigs of cypress, birch, and poplar, which was clearly shown by examining the fragments of charcoal under a magnifying glass.

In the vicinity of this mound were several small circles of fifteen or twenty feet diameter, some of which were circular ridges, and others were low mounds, seldom exceeding one foot in height, and flat on the top.

On the battle-field of Shiloh a great many similar right-angled, flat-topped mounds were observed, varying but little in size, form, and height. The mounds of this class are found for miles along the Tennessee river, and are very numerous near the mouth of a small stream, I think called Bear creek, which was in the very midst of one of the battle-fields of our late contest. One of these mounds is on the bank of the river, a little below the mouth of the stream just mentioned, on the side of a high bluff, which affords a fine view of the great bend of the Tennessee river. On its south side it is about seven feet above the level of the land, but situated as it is on the brow of a hill, its north side is raised from twenty-five to thirty feet above the plain below. In the course of a walk of about a mile back from the river, from twelve to fifteen mounds of this class were counted. Their general length is about sixty feet, width forty feet, and height from six to eight feet. Many of them were made the burial places of the slain who fell in the fearful contest.

As in the case of the mound at Fort Pickering, the surrounding land was marked with small circular mounds, ridges, and crescents, which were so close to each other in some places that I could step from one to the other. No opportunity was afforded to explore any of these mounds, or to form any idea of the purpose for which they were constructed. At the village of Savannah, ten miles below, a parallelogram-shaped mound was observed, similar to those near Pittsburg Landing, and some two hundred yards to the northwest of it a very singular ancient work was noticed. Its form was that of a flattened cone, with a base in the form of a well-marked circle, having a diameter of thirty-five steps, and its apex, attained by a regular slope from all sides to the centre, was not more than three feet above the base. The writer has no means of knowing how much farther south these antiquities extend. No earth-works of any kind were seen by him in Mississippi.

Crossing the Mississippi river, ancient remains of a class and character entirely different attract our attention. The practical succeeds to the fanciful; the purpose is no longer mysterious, but obvious. The ruins of ancient towns and cities occur so frequently in travelling through southeastern Missouri and eastern Arkansas as to leave no doubt that this portion of our country was once very populous.

The sites of those ancient towns and cities are indicated by a series of little square-shaped mounds, raised above the general surface of the land but one or two feet, all ranged in straight lines in two directions, indicating that the streets crossed each other at right angles, and that every dwelling stood upon a street. To be assured that these little mounds were the remains of mud (or adobe) dwellings, several were opened, and, in turning up the earth, in every instance wood ashes and charcoal, broken pottery similar to that found on the shores of Lake Erie and throughout the Mississippi valley, flint arrow-heads, and stone axes were found. In the rainless regions of the world, the sun-burnt brick, or adobe, makes a substantial structure; but in a country of so much rain as Missouri and Arkansas adobe houses would soon moulder down to a heap, unless well protected by a roof covering. In the implements and utensils found in that country no essential difference, either in material or form, was observed, except that some earthen vessels were found entire. The marks of the hand left upon the clay when it was plastic still remained upon the vessel.

Where Fredericktown, the seat of justice of Madison county, Missouri, now stands, was also the site of a considerable city. This town occupies the dividing ridge between the waters of Castor creek and St. Francis river, which are about a mile and a half apart. Several fine and large springs of pure cold water flow out of the sides of this ridge, which, from indications, was the chief object in founding a city at this place. Among those indications is the prominent fact that nearest to the water were the largest mounds—largest not only in height but in horizontal dimensions. Some of these exceeded twenty feet square in base, while those most remote from the water did not exceed ten or twelve feet. It is evident that the chiefs, or men in authority and influence, requiring large and more commodious dwellings, settled nearer to the water, while the poor dwelt further off.

About four miles to the northwest of Fredericktown another site of a town was examined, and this was also well supplied with water, as was every one of the ancient remains which fell under the notice of the writer in Missouri.

This hasty sketch of some of the ancient remains in the Mississippi valley, though the result of the observations of a lifetime, is principally drawn from memory, and no pretension is made to exactness. As, however, the preservation of a record of the site of every mound which has been observed, and of every fact connected with it, is important to the student of archæology, this sketch, brief and imperfect as it is, may afford some data of interest in regard to the character of the race of men who once thickly peopled this country, and who have left such surprising monuments of their arts and industry.

PILE-WORK ANTIQUITIES OF OLMUTZ.

TRANSLATED FOR THE SMITHSONIAN INSTITUTION FROM THE VIENNA "WANDERER," JANUARY, 1865.

IN the course of last summer there were found, as is generally known, in the precincts of the city of Olmutz a variety of articles and implements of bone, stone and bronze, mingled with animal and human relics, under conditions which had till then not been observed elsewhere than in Switzerland and Denmark. The discovery was at once made public and excited great and general interest throughout Germany. Although, from the first, it was impossible to doubt that these remains belonged not to the middle ages, but to a preceding era, it could not but be felt that an opportunity was thus afforded of throwing great light upon the subject of these and similar antiquities by a detailed comparison of the discoveries made on the banks of the March with the remains of the stone and bronze ages of Switzerland. And to whom could this inquiry be referred with so much justice as to the highest authorities in this field of science, the widely known explorers of the historical antiquities of the country just named—a Keller, a Rütimeyer, a Vogt, and others? From Zurich and Basle was first diffused a light over the primeval Celtish epoch of middle Europe, with the modes of life and industrial pursuits of its oldest postdiluvian inhabitants. In Zurich and Basle alone were to be found the materials for a comparison and identification of the various objects of primitive art as well as the animal remains of those remarkable colonies of the earlier human races. Hence, in Switzerland only could the importance of the Olmutz discoveries be rightly decided. To the proposal for a thorough and careful comparison of these with the bog and pile antiquities of Switzerland the readiest assent was yielded by the savants of the latter country, and the most liberal and zealous assistance was rendered for that purpose, above all, by Dr. Ferdinand Keller, president of the Antiquarian Society of Zurich, the first discoverer of the pile constructions, and by Professors Oswald Heer, of Zurich, L. Rütimeyer, of Basle, Karl Vogt, of Genf, and E. Desor, of Neuenberg. To these, therefore, are due the warmest thanks of all the friends of archæology and natural science.

It will of course be readily conceived that but a small proportion of the objects as yet discovered could be submitted to the inspection of the above-named savants, while much remains to be brought to light by future explorations; but thus much may even now with certainty be said: the pre-historical antiquities of Olmutz correspond for the most part in the most exact manner with the industrial and animal remains of the pile-settlements of Switzerland. To show this, we may be allowed to communicate a few facts derived from the statements of the Swiss authorities.

A portion of the Olmutz collection pertains to the age of stone. Dr. Keller recognized, first, a stone knife; second, a hatchet of bone, precisely like those which occur in the Swiss sites, and which must have been prepared from the bones of some large animal, perhaps the ure-ox; a fragment of deer's horn, severed with a stone hatchet from the trunk; fourth, a *metacarpus* of the *Equus caballus*, polished on the anterior side and perfectly similar in this respect to several specimens from Moosscedorf, which antiquarians agree in considering as a kind of skates. (Statement of Professor Rütimeyer, of Basle.)

The greater part, however, of the objects from the banks of the March have their origin in the age of bronze; they are such as, in the words of Dr. Keller, "belong to a very early period, which coincides with the bronze age of Switzerland, and fully correspond with the articles taken from the pile structures of that period." Among other, both entire and broken, articles of bronze, the same savant identified "a portion of a ring in which the joints of the fabrication are still to be distinguished; a piece of a needle; an ear-ring of the same form with those found in the pile buildings and burial mounds," &c.; also, "a spindle whirl, bearing the same close correspondence." Among the earthen vessels are found many fashioned apparently by no unpractised hand, and, among others, Dr. Keller describes one as "a large pot, somewhat coarsely executed, but well baked, and furnished with an uncommonly solid brim; it is more than fifty-six centimetres in width, and from the ornamentation may be assigned to a very early epoch of the bronze age." Again, a drinking-cup is described as "exactly similar to those which we often find in the burial mounds of the Helvetic era (two hundred years before and till the birth of Christ) and executed in the same style with the above."

From the communications of Professor Rütimeyer, respecting the animal remains thus far sent to him, (another remittance has within a short time been despatched to Basle,) we learn that in the settlements on the March there existed the following animals: The wild boar, the hart, the horse, the goat, the sheep, the brachyceros, an ancient race of domestic cattle, two varieties of the dog, the tame hog, and also the extinct turf-hog (*Sus scrofa palustris*, Rütimeyer.) Of this last the professor had before him, among other remains, a nearly perfect skull, which he describes as "differing in nothing from those of Switzerland and Italy."

Professor Oswald Heer has been kind enough to examine the cereal grains which were found mingled in the mass containing the bones of animals and men, as well as two fragments of bronze. He expresses himself as follows: "Besides the wheat, (*Triticum vulgare,*) which occurs in two forms with larger and smaller grains, we find also those of rye, (*Secale cereale,*) which had been as yet nowhere discovered in the remains either of the pile-settlements or of the Roman times. Should it prove on further research that the grains in question are in reality a product of the Roman or rather pre-Roman period, it would certainly furnish an interesting fact for the history of our cereals." This fact, however, is by no means difficult to prove, for the ancient date is satisfactorily evinced: 1st. By the two fragments of bronze found in connection with the grains and examined by Dr. Keller, one of which fragments appears to have been a portion of a large ring, probably for the neck, and the other the handle of some spoon-shaped implement. 2d. By the immediate contiguity in which the grains are found with the skull, determined by Professor Rütimeyer to be that of the turf-hog. 3d. By the close proximity of the bones of the animal, pronounced by the same authority to belong to the ancient race of neat cattle. 4th. By the presence of a human skull, whose high antiquity it is impossible to doubt. This last relic corresponds in a surprising degree with a skull lately described by K. E. Von Baer, (Bulletin of the Petersburg Academy, *Melanges biologiques*, 1863, IV, 3,) which was taken from a burial mound of the bronze age in Zealand, and of which Von Baer had received a plaster cast from M. Thomsen, of Copenhagen. This Danish skull, which would appear to be the first belonging to the bronze period that has been obtained in Copenhagen, presents, equally with that found at Olmutz, "an extremely inclined crown—that is to say, it is considerably elevated along the median line, and sinks away rapidly on both sides. In both crania, the upper and hinder part of the head, as far as the occipital ridge, has a striking degree of projection, while the under portion, below that ridge, approximates to the horizontal. In both a cross line (querlinia) passes from one auricular opening to the other, in front of the *foramen magnum*, &c." Now as, according to Von Baer, this Danish skull of the age of bronze strikingly resembles one discovered in Mecklenburg, likewise another pertaining to a skeleton found at Wiesbaden with thirteen bronze rings, and also a very ancient head from a quarry in the government of Moscow, it follows that in the bronze age, or at least at a certain period thereof, one and the same race of men inhabited the wide tract from Denmark to Moravia, and from the Rhine to the banks of the Volga.

A more detailed and complete account of these discoveries in northern Moravia is reserved for another occasion, but, from what has been here said, the importance of the explorations at Olmutz will scarcely be contested. As little will it be denied that the first proofs of the existence in eastern Europe of antiquities ascending to the times of its primitive inhabitants, under conditions similar to those of the Swiss lakes, emanated from the banks of the March.*

There can be no doubt that signal acquisitions might have been made in addition to those already realized, and which we owe wholly to the judicious encouragement afforded by a single individual from his own modest salary, had money been appropriated by authority for comprehensive excavations. Dr. Keller has expressed his conviction that it would well repay the trouble to explore carefully those points at which the antiquities appear to offer themselves in greatest abundance. The Academy of Sciences of Vienna might, it would seem, have promoted, in no slight degree, the knowledge of the primitive condition of our empire by the appropriation of means for such further explorations. This, however, it has not done, but in the interests of science it has sought to improve the discoveries at Olmutz after a different manner. More than ten years from the first detection of pile structures in the lake of Zurich by Dr. Keller, and after rapidly recurring reports of antiquarian societies since 1853, respecting the annually widening discoveries in the Swiss lakes and in Italy, when, finally, the researches of Desor had, in 1864, extended these discoveries into Bavaria, the Academy of Vienna contented itself with appropriating 1,200 florins to four persons for preliminary studies on the pile-work districts of the empire. Although several of the most distinguished members have taken the warmest interest in the discoveries made at Olmutz and their author, the Academy has pronounced against the expediency of prosecuting further researches on the March. The learned world, however, will assuredly not forget that the first inquiry respecting the existence of this interesting class of antiquities in Eastern Europe proceeded from Olmutz.

* Properly this occurred in the year 1858, at Troppau, through the discoverer of the antiquities of Olmutz. (See Year-book of the Imperial Geological Institution, XIII Band.) At Troppau were found, among other things, artificially detached horns of the *Bos priscus* and the *Bos primigenius*, under circumstances which seemed to point to a condition of human life in remote antiquity similar to that which existed in the pile-colonies of Switzerland. So we are told by the geologist, M. H. Wolf.

THE ANTIQUITIES ON THE BANKS OF THE MISSISSIPPI RIVER AND LAKE PEPIN.

BY DR. L. C. ESTES.

For several years I have given these mounds attention, and therefore hope that the following remarks in regard to them may be of some interest.

The questions regarding certain fortifications or mounds alleged to exist on the banks of Lake Pepin and the Mississippi river was early discussed, and now, although it is true that these remains do exist, yet eighty or ninety years have made great changes in their appearance, or else the descriptions of them furnished to us by early travellers must in a great degree be visionary. We can very easily imagine that they were once used for fortifications, but they have now scarcely any resemblance to modern forts.

These mounds are found in great numbers in various parts of the State, mostly upon the west bank of the Mississippi river. The location of them about Lake Pepin is very marked. A portion of the village of Lake City is built upon and over these remains. Between my residence and the lake there seems to have been a regularly laid out town or city. The streets are regular and the mounds equidistant from each other. In the centre of this city there was a very large mound, much larger than any of the others, which was located in the centre of the widest street, and the only one out of line. It was very probably the "headquarters," the residence of the chief, or it might have been the town hall. Nine years ago I sketched and counted these mounds There were about one hundred of them, occupying, perhaps, a space of thirty acres

My theory, which once differed from every other, as to the original design or use of these mounds, is now, I believe, indorsed by the State Historical Society, and by most persons who have investigated the subject. I am very well satisfied that the large and elongated mounds were designed and used for fortifications. Yet I have never been able to determine whether there were ever any ditches around them.

I am also as well satisfied that the very many round mounds found standing separate from each other were simply turf houses, in which once dwelt a people far above, in point of intelligence, the present race of savages. The common idea that these mounds were receptacles of the dead, or that each one is now or has been a sepulchre, is a theory which none have been able to maintain. I have dug into and seen many of them levelled, but never succeeded in finding human bones but in one, and those in the large mound standing in the centre of the city of mounds before described, and I am convinced that these were of more recent interment, and that they belonged to the existing race of Indians. It was a natural and convenient place in which to bury their dead.

Every investigation proves beyond a doubt, in my mind, that these works were built of turf, and that they are always composed wholly of the upper strata of soil, and that there is no perceptible depression of the earth around their base. Again, the soil in the vicinity is not found as deep as in other places, proving that it had once been removed. If these mounds were originally sepulchral in their design, then in all we should find human bones.

Just in the rear of the city of mounds, near the residence of Colonel J. T. Averill, there was a large mound, which I believe to have been a pottery, or potter's shop. Inside this mound I found numerous remains of pottery, besides large, flat smooth stones which were probably used to grind the clay. For quite a distance around this mound I found large pieces of broken pottery.

The town seems to have been well fortified, for there is, about half a mile in the rear, a regular line of out-posts, each wing extending to the lake. About midway of this line, near the residence of H. K. Terrell, the mounds are very large, either elongated or built near together. There was, beyond a doubt, the stronghold or main fort.

Again, about three-quarters of a mile still back to the west, upon an elevation of the ground, there is a group of the largest mounds I have ever seen. They are built very near together and are perfect in their form. There are no ditches about thier base, and they are wanting in other appearance of fortifications. Yet I believe them to have been used as forts, and that they were placed here to guard the approaches to the town from this direction. A few years ago some large oak trees were standing on these mounds, but I failed to ascertain their ages at the time they were cut down.

There is still another group of mounds located to the south, just below A. Dwelle's farm and near Minnow Lake. These resemble those last described—not so large, but more in number. Here again was another fort, which guarded the approaches from the south.

A PHYSICAL ATLAS OF NORTH AMERICA.

A COMMUNICATION FROM GEO. GIBBS, ESQ.

WASHINGTON, *May* 4, 1865.

MY DEAR SIR: The collection by the Institution of materials for the formation of a physical atlas of the United States was among the objects contemplated by yourself in the original programme of its organization, and it appears to me that the time has now arrived when its formation should be laid on a scale commensurate in magnitude and variety of subject with the scientific progress of America, embracing all the departments of natural, physical, and social science capable of being represented in such a form, and extended in its design to the entire continent, since the boundaries of the United States are accidental and governed by none of the laws which control the operations of nature.

The completion of such a work, and its ultimate publication in a collected shape, would, of course, require years, and the expense might render assistance from Congress necessary. This, however, is a matter for future consideration. The arrangement of the materials already on hand, and what may hereafter be collected, will require but a moderate outlay and should be entered upon at once. It reflects but little credit on our national enterprise that almost the only physical charts of America should be of foreign origin.

As a necessary preliminary it is essential that good skeleton maps be prepared, and these should, in my opinion, embrace the following:

1st. A series of maps of natural geographical regions, exhibiting the hydrography of the country with minuteness, but on which only the principal mountain systems should be given, and those in curves without hachures. These maps ought not to be on one scale, for the reason that, while one certain part of the continent is comparatively destitute of interest, over large tracts of country, others crowd into a small space a great variety and even confusion of details. They should, however, be upon multiples or fractions of a common unit. As a bare suggestion I would mention scales of $\frac{1}{1200000}$ and $\frac{1}{600000}$ for these respectively. It is even probable that particular sections would require still larger ones. The districts of country to be included in these regional maps will necessarily form a subject of particular consideration. Entire hydrographical basins, as a general rule, afford the most homogeneous features, and should therefore be adopted unless other reasons interfere. In the geological series, for instance, it may be necessary sometimes to exhibit both sides of a mountain range instead of vallies bounded by water-sheds. This class of maps will be particularly adapted to natural history and the exhibition of physical phenomena.

2d. A second series, in which the same scales and topography should be used, but in which the divisions should be political, will be needed for another class of subjects, such as population, education, agriculture, and generally all those growing out of the relations of man in society.

3d. Besides these there is requisite a map of the continent on a large scale, certainly not less than $\frac{1}{3000000}$. It should include the entire arctic regions, with the eastern part of the continent of Asia and the intermediate islands, the islands of the Gulf of Mexico and the Caribbean sea, and the northern skirt of South America. This, like the first, should consist chiefly of the hydrography, giving only the chief mountain chains, or, at most, just indicating the lower water sheds. Upon it the results of the former should be generalized. Sections and profiles should, of course, accompany the maps.

As these skeletons are wanted for immediate distribution to commence new collections, as well as for the assembling and reduction of information already obtained, they should at once be lithographed, the engraving of the originals being deferred for the present. As however, lithography does not possess the requisite qualities for finished work, I would not recommend the drawing itself to be made in the first place upon stone. Certain sections of the country where the topography is already sufficiently established, might, indeed, at once and with economy be engraved. The discussion of these questions, however, will be properly referred to the officer to whom the supervision of the work is committed.

The maps designed to illustrate the natural and physical sciences, should not, at least in the first instance, exhibit political boundaries, roads, or a greater amount of nomenclature than is necessary to prevent error in location, and that should be chiefly confined to the names of rivers and natural landmarks; but the parallels and meridians should be displayed. No two subjects should, on any account, be included in any one map of either class.

It is needless here to specify all those subjects, and in fact many will doubtless hereafter suggest themselves, which would occur to no one at present. In their final shape, one set should, of course, give the topography of the continent with the utmost practicable accuracy, minutiæ being however confined to those of particular sections. The map of the country west of the Mississippi, accompanying the Pacific railroad surveys, is an instance of the disadvantage of multiplying details on a general map, the most important part of it being rendered practically useless through this error. A bold and simple style of topography should be adopted for all.

On the maps being ready for distribution, each department of the government should be supplied with copies to be issued to such officers as it may designate. Contributions should be sought from the Bureau of Engineers and the Medical Bureau of the army, from the Coast Survey, the Census Bureau, that of Agriculture, of Indian Affairs and others, as well as from the National Academy of Science and learned bodies generally through the country, from expeditions and surveys, and from individuals pursuing special scientific investigations.

As the atlas is to include the whole continent, with its adjuncts, it will of course be proper to solicit co-operation in its execution not only from our own countrymen, but from Russian and British America and from Mexico. Full credit should be given in each case on the maps themselves to the source whence their contents are derived, while the Institution, as originating the work, and bearing the expenses of its preparation, reserves to itself the title of "The Smithsonian Atlas." As soon as any map or series of maps in any branch or department is completed, it may at once be published and sold at a cost sufficient to defray the expense.

The maturing of the skeleton plan in the study of projections, scales, style of topography, and the districts of country to be embraced in the regional maps, should be submitted to a single person, familiar with the surveys that have been made of the country, and at least grounded in the different natural and physical sciences. The war, now drawing to a close, will undoubtedly detach from active service a number of officers of the Engineers, including of course those formerly engaged on the surveys west of the Mississippi. A connection with a work of this magnitude and importance would naturally be an object of ambition, and I believe that its supervision by one of them would readily be permitted by the Secretary of War and by the chief of that bureau. If the United States, as we all trust, is now to enter upon a new career of mental as well as physical advancement, it becomes us to anticipate the directions in which scientific inquiry can be pushed.

I have the honor to be, very respectfully,

GEORGE GIBBS.

Professor JOSEPH HENRY,

Secretary Smithsonian Institution.

ON ETHNOLOGICAL RESEARCH.

A COMMUNICATION FROM DR. E. H. DAVIS, OF NEW YORK.

NEW YORK, *December* 1, 1865.

DEAR SIR: In your last favor, (nearly a year since,) you suggested that in case my collection went to Europe, it would be highly important that casts be taken of the most interesting objects, and inquired the cost of the same. Hearing nothing more from you on the subject, yet believing it so important to the interest of American ethnology that a suit of casts should remain here, I determined at once to complete the undertaking myself. I have accomplished it by the aid of two artists—one to mould, the other to color—completing two full sets at an expense of five hundred dollars. They have succeeded so well that it reconciles me much to the loss of the collection to this country. With these casts and contributions from the west, with Dr. Berendt's collection, which I have purchased, as well as others sent by my former students from Central and South America, I shall soon again fill up my cabinet with specimens illustrating the development of the arts on this continent.

Some years since I informed you that I was engaged in constructing an ethnological map of the United States, locating merely the mural remains. Since then I have changed my plan by locating the tribes instead of their remains, and for my own convenience have extended it to the whole continent. For instance, the great satisfaction it affords one to be able, on receiving relics from any part of the continent, to refer to a map showing the family or tribe of Indians now or formerly occupying that spot.

It is by a careful study and comparison of this kind only that we may be able to arrive at any reliable conclusions concerning the relative culture of the various tribes.

Formerly the sources for constructing such a chart were scarce and unreliable. But few travellers attempted to locate the tribes they visited, and none pretended to do so with any degree of accuracy. Adair, Carver, and some others accomplished something by accompanying their works with maps giving the names of tribes they visited, but without limits or boundaries.

Albert Gallatin was the first to construct anything like a comprehensive ethnological chart, locating, according to language, most of the tribes of North America. So far as he went, it was undoubtedly much in advance of anything down to his day. Since his time, by the philological studies of Hale and our late lamented Turner, we were able to correct some and add much to his labors. Yet the honor of completing the finest ethnological chart of America has been secured by a foreigner, the learned German anthropologist Waitz, (now dead.) He, by the aid of Gallatin, Ludwig, Turner, and a host of other authorities, has almost exhausted the subject. Still, much remains to be accomplished by local explorers, who may add new facts, or correct inevitable mistakes in a work covering so extensive a field as all America. In illustration of this remark comes the work of Manuel Orozco, "Geografia de los Lenguas, y Carta Etnografica de México," published last year under the auspices of Maximilian. This is a work of immense labor, continued through a long series of years, performed, too, under the disadvantage of working almost alone, cut off very much by the unsettled condition of the country from the results of co-laborers and societies in other countries. Hence our astonishment that so much has been done, and at the same time so well done. It is accompanied by an ethnographic chart, handsomely executed, showing the geographic distribution of all the Indian tribes of Mexico. He descends much more into detail than Waitz, who gives to Mexico but a few families, while Orozco divides them into eleven distinct families of languages, leaving sixteen unclassified, and sixty-two idioms, comprising in all two hundred and seventy-six languages and seven hundred and twenty-nine tribes within the limits of that country.

Notwithstanding all this seeming minutiæ and accuracy of Orozco, we find other explorers adding new light. For instance, Dr. Berendt, who, you are aware, has spent many years in that country, at the same time enjoying the rare advantage of understanding the Maya language, which enables him to add much new information, and also to correct some of the errors into which the patient Orozco has fallen.

I am now convinced that from all these sources a much better ethnological chart can be constructed than now exists—one that would reflect credit upon America. Cannot you accomplish this desirable end by calling to your aid the combined results of modern explorers, such as Berendt, Gibbs, Morgan, and others? Let them, singly or combined, contribute each their mite to the general stock, which must result in benefit to all.

* * * * Some of our army surgeons have contributed considerably to anthropology by the statistics they have collected on the measurements of the different races. But much more reliable information might have been obtained by concert of action, and by adopting a more complete and uniform system of measurements. In examining this subject, at the request of members of the Sanitary Commission, I came to the conclusion that the German method of Scherzer and Schwarz was the most scientific, yet it needs some modification to adapt it to our more practical and less patient people. I have copied it, and shall send it for your inspection, notwithstanding you may have seen it, if you have received Dr. Carl Vogt's Lectures on Man, which have been translated and published by the Anthropological Society of London.

I remain, most respectfully, yours, &c.,

E. H. DAVIS.

Professor HENRY.

TABLE OF MEASUREMENT BY SCHERZER AND SCHWARZ.

I.—GENERAL OBSERVATIONS.

[Name, sex, native country, occupation, shape and growth of beard, &c.]

	No. of the systematic series.
1. Age of the individual measured	1
2. Color of the hair	2
3. Color of eyes	3
4. Number of pulsations in a minute	4
5. Weight	5
6. Pressing power, *(force manuelle,)* measured with the dynamometer of Régnier	6
7. Lifting power, *(force rénale,)* measured with the dynamometer of Régnier	7
8. Total height	8

II.—MEASUREMENTS WITH THE PLUMMET AND METRE-SCALE.

9. Distance of the commencement of the growth of the hair on the forehead from the perpendicular	9
10. Distance of the root of the nose from the perpendicular	10

No. of the systematic series.

11. Distance of the anterior nasal spine from the perpendicular 11
12. Distance of the point of the chin (mental process) from the perpendicular 12
13. Distance from the root of the nose to its tip 13
14. Distance from the tip of the nose to the anterior nasal spine 14

III.—MEASUREMENT WITH THE CALIPERS.

15. Distance from the point of the chin to the commencement of growth of hair 17
16. Distance from the point of the chin to the root of the nose 15
17. Distance from the point of the chin to anterior nasal spine 16
18. Distance from the point of the chin to the vertex 19
19. Distance from the point of the chin to crown of the head 21
20. Distance from the point of the chin to the external occipital protuberance 23
21. Distance from the point of the chin to the external auditory opening .. 25
22. Distance from the point of the chin to the angle of the lower jaw 27
23. Distance from the root of the nose to the vertex 20
24. Distance from the root of the nose to the crown of the head 22
25. Distance from the root of the nose to the external occipital protuberance 24
26. Distance from the nasal root to the external auditory opening 26
27. Distance from the nasal root to the angle of the lower jaw 28
28. Distance from the place where the hair begins to grow to the *incisura juglaris sterni* 18
29. Distance from the external occipital protuberance to the seventh cervical vertebra—the measurements 28 and 29 must be taken with the head in the same position, *i. e.*, the natural one 56
30. Distance from one auditory opening to the other 30
31. Distance of the uppermost points of attachment of the ear 31
32. Greatest distance between the zygomata, or zygomatic arches 32
33. Distance between the external corners of the eyes 33
34. Distance between the internal corners of the eyes 34
35. Distance between the points of attachment of the lobes of the ear 35
36. Breadth of the nose 36
37. Breadth of the mouth 37
38. Distance between the angles of the lower jaw-bone 38
39. Distance from the seventh vertebra of the neck to the semi-lunar notch of the sternum, *(incisura juglaris sterni)* 40
40. Transverse line from one middle line of the axilla, above the mammæ, to the other 43
41. Distance from the sternum to the vertebral column 44
42. Distance from one anterior superior spine to the ilium of the other 49
43. Distance from one trochanter major to the other 50

IV.—MEASUREMENTS WITH THE MEASURING TAPE.

44. Circumference of the head around the external occipital protuberance .. 29
45. Circumference of the neck 39
46. From the greater tuberosity of one humerus, in a horizontal line, to the other 41
47. Distance from one middle line of the axilla, above the mammæ, to the other 42
48. Circumference of the thorax at the same place 45
49. Distance from one nipple to the other 46
50. Circumference of the waist 47
51. From one anterior superior spine of the ilium to the other 48

	No. of the systematic series.
52. Distance from the trochanter major to the anterior superior spine of the ilium, (on same side)	66
53. Distance from the most prominent part of the sternal articulation of the clavicle to the anterior spine of the ilium	51
54. Distance from the most prominent part of the same to the umbilicus	52
55. Distance from the umbilicus to the upper ridge of the symphysis pubis	53
56. Distance from the fifth lumbar vertebra, along the crest of the ilium and the inguinal fossæ, to the symphysis pubis	54
57. Distance from the seventh vertebra to the terminal point of the os coccygis	57
58. Distance from one summum humeri, across the back, to the other	55
59. Distance from the summum humeri to the external condyle of the humerus	58
60. Distance from the external condyle of the humerus to the styloid process of the radius across extensor side	59
61. Distance from the styloid process of the radius, across the back of the hand to the articulation of the metacarpal bone of the middle finger.	60
62. Distance from the same articulation to top of middle finger	61
63. Breadth of the hand	62
64. Greatest circumference of upper arm around the biceps	63
65. Greatest circumference of the fore-arm	64
66. Smallest circumference of the fore-arm	65
67. Distance from the trochanter major to the external condyle of the femur	67
68. Distance from the external connyle of the femur to the external malleolus	68
69. Distance from the inferior margin of the symphysis pubis to internal condyle of femur	69
70. Distance from the internal condyle of the femur to internal malleolus	70
71. Greatest circumference of the thigh	71
72. Smallest circumference of the thigh	72
73. Circumference of the knee-joint	73
74. Greatest circumference of the calf	74
75. Smallest circumference of the lower part of the thigh above the malleoli	75
76. Length of foot	76
77. Circumference of foot around the instep	77
78. Circumference of the metatarsal joints	78

INTERNATIONAL ARCHÆOLOGICAL CONGRESS:

ORGANIZED BY THE ARCHÆOLOGICAL ACADEMY OF BELGIUM IN CONCERT WITH THE FRENCH SOCIETY OF ARCHÆOLOGY: ANTWERP, 1866.*

PROGRAMME.

ARCHÆOLOGY.—1. Do the discoveries of lacustrian habitations enable us to assign precise chronological limits to the custom which led to the erection of these constructions?

2. Do the dolmens of Ireland and Scotland modify the conclusions drawn by French archæologists from the study of monuments of the same nature existing on the coasts of Brittany and Normandy, in regard to the destination of these constructions?

3. Do the subterranean discoveries made within a few years in Denmark modify the results obtained by archæological science in the southern and central parts of Europe?

4. To what epoch may the tombs of lead be referred? By what characters can their succession be recognized?

5. Do the discoveries of antiquities made thus far in Germany enable us to determine, in a certain manner, the period during which the Romans occupied that part of Europe?

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* *Regulations.*—1. The Congress will open August 12, 1866, and will terminate the 21st of the same month. 2. It will be divided into two sections, which will transact business alternately, in general assembly. 3. The central bureau, composed of four members of the committee of organization, four of the committee of administration, and six foreign delegates, will nominate, each day, for the sessions of the next, the president, vice-presidents and members of the bureau of each section. Each day, the central bureau will prescribe, for the following day, the hours of session of the sections and determine for them the order of the day, which shall not be modified. 4. The secretaries of the sections will be nominated by the central bureau, which will proclaim the names at the opening session. 5. No one shall address the Congress during a session without permission from the president. 6. All discussion respecting religion and modern politics is expressly interdicted. 7. The reading of no memoir will be permitted. This interdiction does not apply to citations which the speakers may have occasion to make in their discourses. 8. Written memoirs will be referred to the central bureau, which will judge whether they merit insertion in the *compte-rendu* of the sessions. 9. The members shall have the right of presenting other questions than those of the programme, but these must have been previously laid, during session, upon the desk. They will be examined, the same evening, by the central bureau, which will judge whether they can be admitted. The result of the deliberation will be communicated next day to the sections which they concern. 10. Scientific excursions will be in order during and after the continuance of the Congress. 11. Those persons shall be members of the Congress who, having accepted the invitation extended to them, shall have deposited in the hands of the treasurer the sum of *ten francs* for the purpose of discharging in part the expense of printing and engraving the *compte-rendu* of the labors of the session. 12. Each member of the Congress shall be entitled to receive a copy of the *compte-rendu*, which will be published under the superintendence of the general secretaries. 13. Each scientific association which shall appear by representative shall receive gratuitously a copy of the *compte-rendu*. This does not dispense with the payment by the delegates of the contribution mentioned in article 11, and which gives them personally a right to the receipt of the volume. 14. Members prevented from attending the Congress may, like others, present memoirs on the questions of the programme. 15. Representatives of the public journals will, on application to the central bureau, have a special gallery assigned to them. 16. Any difficulty not foreseen in these arrangements will be submitted to the decision of the central bureau without appeal.

6. What influence has been exerted on the arts, and especially on architecture, in the Iberian peninsula by the occupation of the Moors?

7. What state has the study of runic inscriptions attained?

8. To determine the characters of Carlovingian architecture and its influence on the progress of the art of building. To explain the influence of the Normans on this art, after Charlemagne.

9. What classifications may be adopted for the discoveries made in Belgium and neighboring countries of objects anterior to the Carlovingian era? Are the divisions adopted by the Abbé Cochet admissible elsewhere than in Normandy?

10. What is the true symbolical interpretation of the box or coffer, the glass or *poculum*, and the cloth *mappa*, in the funeral representations of the Gallo-Roman epoch?

11. Did the architecture of Greek temples borrow its forms from constructions in wood? A discussion of the opinion of M. Violet-Leduc on this subject.

12. Is the ogival style to be considered as the natural and complete development of the Roman style? On the contrary hypothesis, might not this last be reinstated in practice and applied to the requirements of the present age?

13. There existed in France, in the twelfth century, several schools or systems of architecture, in determinate regions. Can the same fact be recognized in Germany and the Netherlands during the eleventh and twelfth centuries? If so, what was the geographic distribution of those schools, and in what did they differ from one another?

14. What were the mechanical means employed by the primitive races in moving the immense blocks serving for the construction of dolmens, peulvans, menhirs, and structures styled halls of the *giants*? An examination should be made of the opinions pronounced on this subject. See the memoir of his Majesty Frederic VII, king of Denmark, inserted in the publications of the Society of Antiquaries of the North, (1850, 1860.)

15. What is conclusively known respecting the different kinds of horseshoes found in Gallo-Roman mines, and the manner of using them?

16. What is the point to which the study of hieroglyphics, and other modes of writing adopted by the ancient Egyptians, has attained?

17. Are the ancient heaps or mounds which exist in southern Gaul itinerary landmarks, tombs, or monuments of some other kind?

18. To ascertain the origin of the ogival style, and explain how it was introduced into the Netherlands, Germany, and England.

19. To determine the special causes which have contributed to modify the ogival style in France, England, Germany, Italy, and the Netherlands.

20. To determine by texts and respectable authorities the honorary value of the ornaments which certain representations of Roman warriors show to have been in use.

21. What degree of certainty has been attained by studies directed to the deciphering of the cuneiforms?

22. The strongholds encompassed with stones or palisades of wood, represented on the column of Trajan, evidently afford the point of departure of our feudal castles of the tenth and eleventh centuries. What documents and texts exist showing the system used in the intermediate period, that is to say, under the Merovingians and Carlovingians, for abodes of opulent proprietors living in the country?

23. What rules should be adopted in the different countries of Europe: 1st, for constructing methodical archæological charts; 2d, for classifying systematically each principal epoch?

24. Should churches be made to front towards the east?

25. In churches, what is the best situation that can be given to organs and baptismal fonts?

26. Are there any modern requirements which the ogival style could not satisfy?

27. To present a view of the Anglo-Saxon antiquities brought to light by excavations made in England since the commencement of the present century. To institute a parallel between these antiquities and those of the Frankish epoch which are found in the soil of ancient Gaul.

28. To present a series of figures of feudal keeps or dungeons in western Europe at certain dates, going back to the twelfth, thirteenth, and fourteenth centuries.

29. Do there exist any certain means of distinguishing the vases of the Gauls, the Germans, the Batavians, the Britons, and the Roman or Gallo-Roman vases?

30. Are the ceramic classifications of M. Pottier, of Rouen, for the thirteenth, fourteenth, and fifteenth centuries (see the *compte-rendu* of the Scientific Congress of Rouen) accepted in England, Belgium, and Germany?

31. Where are to be found the most perfect models of Gothic civil architecture? Would it not be desirable that the monotony of modern houses should give place to ogival or Roman construction?

32. Are the white and light potteries covered externally with red lines characteristic of the thirteenth century?

33. Is the use of plaster, zinc, cast-iron, and similar materials admissible in artistic or monumental constructions?

34. What, in the different regions of Europe, has been the influence of local materials employed in ogival architecture during the middle ages?

35. A review of the art of cloth-making among the Romans as illustrated by the texts and ancient bas-reliefs which relate to this mode of industry.

36. What was the signification of the *horse* in the funeral bas-reliefs of Greece?

37. By what signs can the deceased person be recognized in ancient bas-reliefs representing death under the form of an *adieu*?

38. A compendious account of the art of the blacksmith in the second, third, and fourth centuries, with a citation of texts and the production of bas-reliefs.

39. Do the Greek temples present curves in their principal lines? A discussion of the theories of Penrose and Boetticher on this subject.

40. Is it practicable to ascertain, by facts, the epochs of the burning and burial of the dead in the Gauls?

41. Is it possible to determine the different phases of Roman civilization in the Gauls from the study of monuments and archæological discoveries?

42. What are the principles to be pursued in the restoration of ancient monuments constructed at successive epochs and in different styles?

43. What measures should be asked from the legislature to insure the preservation of historical monuments? A succinct account to be given of the legal provisions to this end which are in force in Belgium and adjacent countries.

44. To determine the age of objects in silex from their degree of elaboration.

45. To what age may we refer the instruments cut out of bone and silex in the caverns of Arriege of the provinces of Liege, Namur, &c.?

46. According to M. de Caumont the tumuli of a great part of France are not posterior to the Antonines; can this observation be generalized for the whole of ancient Gaul, or at least for the north of that country?

HISTORY.—1. An examination of the different opinions which have been recently advanced respecting the birth place of Rubens.

2. A review of the different opinions maintained respecting the true inventors of printing, with a statement of the conclusions arrived at.

3. Is it certain that America was discovered by the populations of the north of Europe before the expedition of Columbus?

4 An estimate of Charles the Bold as warrior and politician.

5. A determination of the Hellenic and the Etruscan influence on the development of the sciences and arts in primitive Rome.

6. A verification, by positive documents, of the place of birth of Godefroid de Bouillon.

7. What was the special character of the ancient Celtic, Batavian, and Gaulish divinities ?

8. What were the results of the Saxon immigrations in the eighth and ninth centuries into the ancient Low Countries and the north of France ? From what motives was Charlemagne led to favor them ?

9. Might not archæological and historical societies, by co-operating to this end, complete in France the work of Alexis Monteil and extend its application to other countries ?

10. What, in the tenth century, were the principal routes of communication in the Netherlands and the north of France ?

11. What was the place of birth, or at least the nationality, of Pierre l'Hermite ?

12. Is it necessary to abandon the inquiry into the birth-place of Charlemagne ?

13. A determinate geographical circumscription of the forest Charboniere ?

14. An estimate of the influence exerted by the Roman legions on the propagation of Christianity among barbarous tribes.

15. Were there personages of high rank in Roman society who as early as the first and second centuries had embraced the religion of Christ ?

16. What systems for the classification of historical archives have been adopted in the different countries of Europe ? What is the most rational method ?

17. Indicate the processes employed in the middle ages for the manufacture of stained glass windows, distinguishing the ancient workshops most renowned for the preparation of colored glass.

18. What has been the influence of Germany on the development of public law in France ?

19. What were the relations of the Flemish communes with Edward the third of England on the occasion of his wars with Philip de Valois ? An appreciation of Van Artevelde in reference to the Flemish policy regarding England and France.

20. What was the topography of ancient Menapia at the time of Julius Cæsar ?

21. An account of the external relations of the Society of Free Merchants of London, and the treaties of commerce which it negotiated.

22. What is the present name of the localities in which were situated the endowments conferred by Goibert and his son Guntbert, endowments verified by several diplomas of the ninth century of the cartulary of the abbey of St. Bertin ? (See *Cartulaire de l'Abbaye*, by M. Guerard : Paris, 1841.)

23. What are the means of determining the territorial circumscriptions of the Gaulish tribes mentioned in ancient authors ?

24. What was the influence of the navigation law, under Cromwell, on the development of the English marine ?

25. An account of the hydraulic labors connected with the ancient course of the Scheldt towards the sea, executed by the orders of King Otho. Did not these labors give rise to two distinct enterprises: the derivation of the Scheldt from Ghent to Boterswonde under Otho I, (941–946,) and the enlargement of the passage before Flessingen, under Otho II ?

26. What has been the influence of the different modes of artistic instruction employed from the thirteenth century to our own day, upon the progress of intelligence and the civilization of the popular classes ?

27. An estimate of the character of the ancient schools of painting of Liege and Tournai, and their influence on the development of the Flemish school.

28. What were the causes of the development of the fine arts in Flanders in the fourteenth and fifteenth centuries, and what was the influence of this artistic movement in France, and especially in Burgundy?

29. To what point did the religious dogmas of the Druids prepare the propagation of Christianity among the Gauls and Britons?

30. What was the origin of the Exchange (*Bourse*) in commerce?

31. Is it conclusively established that the brothers Van Eyck were the inventors of painting in oil?

32. What are the best means for awakening in the people the æsthetic sentiment? Do artistic exhibitions promise in this respect the desired effect?

33. Whence did the ancient tribes of Mexico, which, according to Aztec tradition, arrived in the twelfth century in the valley of Anahuac, derive their origin?

34. What date is to be assigned to the compilation of the first book of the Chronicles of Froissart? Distinguish in this first book what pertains properly to that historian, and indicate what he has borrowed, together with the interpolations or alterations which his work may have undergone.

35. Do the Assyrian monuments discovered up to this time, and the studies of which they have been the object, enables us to state the distinctive characters of Assyrian art, or to recognize any traces of that art in the Hellenic and Etruscan civilizations?

ON VITALITY.

BY THE REV. H. H. HIGGINS. M. A.

FROM THE PROCEEDINGS OF THE LITERARY AND PHILOSOPHICAL SOCIETY OF LIVERPOOL, ENGLAND, 1864.

THERE is at present among some very eminent physiologists a growing tendency to deny, or at all events to question, the existence of vitality as distinct from the action of known forces, such as heat, light, electricity, &c., or something analogous to these.

The views of the physiologists above referred to may thus be briefly stated:

(1.) Of the nature of vitality we know nothing; we are, therefore, not required either to admit or to deny its existence as a distinct thing.

(2.) The observed phenomena of life are consistent with, and, to a very great extent, derivable from, the operation of known laws; it is, therefore, not philosophical to introduce an entirely unknown agency to account for such residual phenomena as are not thus reducible.

The present paper will be devoted to the consideration of some questions bearing upon these two propositions.

It is a matter of comparatively little importance what term may be chosen to denote the object of our inquiry, whether it be "vitality," or "germ force," or the "vital principle," so that it be clearly understood to refer only to the ultimate element of life, and not to any even of the simplest functions of life. Seen under this aspect, vitality is simply the *sine quâ non* of the animate individual,* whose very existence, as such, stands or falls as vitality is, or is not, regarded as a distinct entity. Personality, which is a higher form of individuality, is equally dependent on the question whether vitality is, or is not, the result of forces such as we are accustomed to deal with in scientific investigations. It would be absurd to call a flame a person or an individual; yet it has active qualities, a distinct form, requires aliment, &c. A man is not a person because he has these properties, but because he has a something which a flame has not. Whether this something be designated soul, or spirit, or will, or intellect, or vitality, is, I apprehend, all the same in respect of its relation to physical science, which cannot recognize metaphysical distinctions. In fact, it is the question before us, whether on grounds of physical science we are competent to recognize vitality under any aspect as a distinct thing.

It must be evident that if the vital functions by which man is distinguished from a block of granite be the result of difference in the combination of the primary molecular forces of his living substance, he has no more right to be regarded as a person than has a thunder-storm; his being is a process, and in general terms he may be described as a segregation of certain forces, initiated by a similar combination, and passing away into equilibrium, or into the general stock from whence he was derived.

* In assigning this position to vitality the writer is aware of the difficulties which beset the subject, especially in connection with the development of plants, and in respect of the lower forms in the animal kingdom, compound animals, the alternation of generations, &c. If, however, vitality can be shown to lie beyond the range of scientific investigation, in all these cases the knot is cut; and while physical development remains in every instance a proper object of scientific inquiry, neither the relations subsisting between the vitality of a seed and that of the parent plant, nor any similar relations, can adequately be discussed as matters pertaining to natural science.

The issue, however, must be tried, not on its consequences, but on its scientific merits; on which grounds, as I apprehend, whatever may be demonstrated concerning the vitality of man holds equally good with reference to the life of a monad, or of a particle of red snow. Still, if it can be shown on purely physical principles that vitality is a something which is not analogous to the actions of known forces, then life is, to all intents and purposes, a miracle, by which I understand not the action of a power in opposition to or thwarting the physical laws of nature, but the manifestation of an agency extra-cosmical, working harmoniously with, and by means of, those laws.

It may seem to some hardly worth the while to contend for the possession by man of a distinct vitality, if by this term is meant only that which he must hold in common with an animalcule or a seed. But a moment's reflection will make it plain how vast a step is taken if we gain from physical science the admission that her kingdom is not universal. None will be more ready than the man of science to confess how little is that which is known when compared with that which remains to be known; nevertheless he is becoming more and more inclined to be convinced that all is *knowable*, and, if known, would be found conformable in all respects with the knowledge that he has already. Now, if it can be shown that vitality, even in a vegetable cell, is a thing which lies beyond the cope of phpsical investigation, the spell is broken, and a claim is established for the determination of what may or may not constitute the higher faculties of man on other grounds than those of physical science alone.

As a believer in something more than natural science, it is proper for me to state that I do not think a rational persuasion of the personality of man must rest upon evidence to be obtained from physical researches. Still I should expect to find in physical science some indication of its own limits, and of the commencement of that border land which separates the known from the unknown. More than this the very nature of the case forbids.

We may now proceed to the consideration of the two propositions given at the commencement of this paper as expressing the views of certain physiologists who decline to recognize in vitality anything beyond the operation of forces amenable to physical investigation.

It will be observed at the outset that much stress is laid upon the absence of all knowledge respecting the *nature* of vitality. This limitation is needful, because to say that nothing is known of its effects would simply be to anticipate the decision of the question at issue. But it may at once be admitted that we know nothing of the nature of vitality. How should we, if it has no analogy with any of the known forces? For, on this supposition, in what form could knowledge of vitality hold its place in our minds? Not in any of the old familiar forms, such as predicates respecting its quantity, intensity, polarity, and the like; for the thing thus known would have analogy with known forces, and would not be a thing *sui generis*, but would fall naturally into some recognized category. We have to give our reasons for concluding that certain observed facts imply the existence of an agency quite unlike any of the forces known to us. The reply that we know nothing of the nature of such an agency is certainly no disparagement to our hypothesis, unless it may be shown that nothing can exist of which we do not know the nature. If, then, we are at liberty to make the supposition that vitality is a thing *sui generis*, that we know nothing of its nature tells neither for nor against the probability of its existence.

One of the broadest generalizations deducible from the immense additions recently made to the ascertained truths of science is, that numberless things previously supposed to be distinct are now found to be so closely related that it is impossible to draw a line of separation between them. This has been the case alike with things organic and inorganic. The great kingdoms of animal and vegetable life pass quite imperceptibly the one into the other; and in both kingdoms the number of classes, orders, families, genera, and species that show the same tendency is so great that where an isolated group is found it is thought to be probable that the links of connection are missing only because they are extinct, or are not yet discovered.

It is important for our purpose to notice the manner in which one group of living things passes into another. It will generally be found that the transition is not effected by the shading off or blending of the extreme edges of the groups, but by a kind of interlocking of the one into the other. The junction often resembles that of the sea and the land on the coast of Norway, where in many places it is hard to say whether it is the sea that runs far into the land or the land that runs far into the sea. Thus, judged by one set of characters, an organic form may not only be a vegetable, but even hold a somewhat high place in the vegetable kingdom, while, judged by another set of characters, the same creature may not only be an animal, but have its place by no means on the confines of the animal kingdom.

Discoveries relating to the allotropic forms of matter and the correlation of the imponderable forces warrant the same conclusion as to the close affinity subsisting between inorganic things.

In fact, that "nature does not proceed by a leap" might seem to be a rule that lacks the criterion of an exception; yet there is an exception, and a striking one; limits have given way in all directions, yet there is one that stands out as sharply and as distinctly as ever—that, namely, which divides animated things from those which are inanimate. No form of matter that is not animate exhibits anything approaching to the phenomena of life. Of course, none but a superficial observer would suggest the process of crystallization, or the dendritic appearances of some metals, or the moss-like forms seen in agates, as exceptions. Neither can any instance of a degradation towards the character of inorganic matter be found in the very lowest forms of life, in the protozoa, or the sponges, or the nullipores, or the fungi, the algæ, the confervæ, or even in the single living vegetable cell.

Now, if the vital principle be analogous to any known form of matter or force, this solitary case of a great leap taken by nature is incongruous with all that we know of her other proceedings. If, indeed, the presence of life indicates the introduction of something entirely new, then we have a reason for the leap. But if not, the facts we have considered appear to be inconsistent with the hiatus between animate and inanimate things. If life be made up of forces similar to those which act in various ways both on organic and on inorganic matter, we might expect to find the transition from things inanimate to things animate the same in character with all other transitions in nature; the border land would be occupied with semi-animate materials and semi-mineral vegetables or animals, with instances of equivocal life and products of doubtful organization. Whereas, from the highest to the very lowest organism, even to the primordial utricle and cell, the phenomena of life are distinct and unquestionable. It is true that geologists have to deal with certain forms of doubtful organization found in the lowest rocks, but in these instances the difficulty arises from the changes through which the fossils have passed. There may be reason to doubt whether certain appearances truly indicate organic remains; but there is nothing to indicate that life, in the Silurian ages, was less distinct than it is now.

The above argument for the specialty of the vital principle, derived from the unparalled solution of continuity in nature just at the point where life commences, demands for its full illustration rather a volume than a mere notice in a brief paper. Its force, if it have any, is evidently acquired from facts revealed by the very researches which have led some to form an opposite conclusion.

Again, if vitality is analogous to, or correlative with, the known forces, we might expect to find at all events some resemblance existing between their respective properties and modes of action. Now the results of the action of heat, electricity, chemical affinity, &c., on various forms of matter have been much studied, and a good deal is known about them. Yet among all these results no single instance can be found resembling the commencement of the functions of vitality. It is, however, admitted that from the outset the vital principle works harmoniously with and by means of the known forces; so that much of that which was wont to be ascribed to vital force is now known to be a chemical process brought about by the agency of light and heat. But the question is not what light, heat, &c., and vitality can do conjointly, but what light, heat, &c., can do without the presence of vitality; and the reply must, I think, be unfavorable to the correlation which is supposed to exist between vitality and the known forces.

It may be replied that none of the imponderable forces has ever been alleged to be the force of life; yet if the latter force is correlative with any of the former, could it behave so differently? For example, the known forces act universally; there is no kind of matter that is not at all times affected by heat, light, electricity, and chemical affinity; on the other hand, the force of life stops suddenly and definitely at a limit which excludes a large proportion of existing matter.

We may now pass again to the alternative. If vitality be not *sui generis*, it must be something analogous to the agencies with which we are acquainted. Now of these agents collectively the *most certain thing* that we can say is, that they are strictly obedient to fixed laws; that under similar circumstances similar results invariably follow. So precise and constant is the operation of this rule that, under favorable conditions, we are able to infer the antecedent from the knowledge of the effect, with as much accuracy as we can predict the effect from the knowledge of the antecedent. We are quite certain that no portion of a result, however minute, can have been produced without a corresponding character having existed in the antecedent. It is not contended that we are able to trace otherwise than very imperfectly the remoter antecedents of the facts observed in inanimate nature, but we are convinced that everywhere the supremacy of law is absolute.

If, for example, we could trace the history of a drop of water which trickles down the window pane, we should find that its volume, its direction, its temperature, and all its other characters were derived from atmospheric and other agencies; and if the history of the drop could be submitted to a suitable kind of examination, it would yield up to us the exact qualitative and quantitative characters of all the forces which had collected it and sent it on its course. These forces would indicate their antecedents, till it might be seen that our drop was the inevitable and exact result of meteorological and chemical and mechanical changes and actions that had been going on continuously from the Silurian ages, and for we know not how long previously.

Nor would the whole history of the drop, however remote its starting point, present any very complicated or difficult facts. Some of its particles may have come from the antipodes, or have belonged to the polar snows; others, in a state of decomposition, may have assisted to lift a balloon or have been breathed by an icthyosaurus, yet they were never for an instant beyond the grasp of those known laws which in the end brought them together to be a drop; every step in the whole course being, with the utmost precision, dependent upon the previous one.

Those who do not admit the vital principle to be a thing *sui generis* must assign to it a place under the dominion of laws, the rigorous character of the operations of which I have endeavored to illustrate.* If the consequence of such an allocation can be shown to be difficulties unparalleled in magnitude, something will be done towards proving that the allocation is a wrong one, and that the vital principle is a thing not subject to the laws of ordinary forces.

* Be it remembered, once for all, that on this point there can be no playing fast or loose to suit the occasion. Vitality is, or is not, subject to the exact laws of nature.

Such difficulties arise in various directions when we begin to apply the laws which govern things inanimate to the phenomena of life.

It is well known that the common hydra may be reproduced by a small portion of its body severed from the rest, or even by a portion of one of its tentacles. On the supposition that known forces, or forces analogous to these, are alone concerned in this reproduction, it follows that in the severed portion must have existed physical peculiarities corresponding with *every part* of the entire animal; or, in other words, that the prototype of the perfect form to be evolved must have been in the particle capable of being thus developed; for known forces have no specific predilections of this kind, and except the bit contained that which could initiate the development in a particular direction, there is no conceivable reason why these forces should develop it into a hydra more than into an amœba or a rotifer.

On similar grounds the seed must contain the perfect initium of the future plant to its most minute organs. The color spot on the petal of a pelargonium could in no conceivable way have been produced through the agencies of chemical affinities, heat, electricity, and the like, except from the existence of some corresponding physical starting point in the seed. This is admitted by some who question the existence of a vital principle.

On the other hand, it may freely be admitted that if this wonderfully complicated structure did exist in the seed, it is by no means certain that we should be able to discover any traces of it. Our being unable to find it is no proof that it may not be there, unless we can show that in our investigation of the seed we have detected the ultimate forms of matter.

Here it is important to define what is meant by the existence, in the embryo prototype, of a structure corresponding in all respects with the future animal or plant to be developed. It is not contended that in the seed, for instance, the prototype must fix in every respect the form and dimensions of the future plant; for in the course of growth the plant is submitted to the variable influences of heat and moisture and light, and these will affect its ultimate condition, or it may suffer distortion from the attacks of insects or from mechanical pressure. Still, under all these possible influences, every portion, every fibre of the living plant will be what it would not have been but from some peculiar character in the seed which initiated its production.

There is not a leaf on an oak tree but must have had its own share in the acorn from which the tree sprang. For let us remember we are trying the consequences of the utter exclusion of a vital principle as a special thing; we have only to do with the results of the actions of known forces, which have no more tendency to produce a leaf than to produce an animal, except from the direction given to their energies by the original initium. In the action of these forces the result indicates the antecedent as much as the antecedent the result. It matters not how many steps backward we have to go. The leaf takes its character from the bud, not, let us remember, by the influence of a vital force, but by a process of development brought about by heat, electricity, light, moisture, carbonic acid, &c., acting on the bud. Then, I submit, the leaf in all its parts, down to its veins and stomata, must have been initiated by some special physical character in the bud. What have heat, electicity, &c., to do with forming veins and stomata? Given the proper nucleus, these forces are quite competent to develop any number of organs, but no particle of the leaf must fail of having had its type in the bud, else, instead of that particle, there would have been an hiatus, or something of a different kind.

The bud imposes the same conditions on the twig; but, in fact, the conditions multiply as we recede, for the twig has to furnish the starting points for the bud and all its leaves; the twig, in like manner, looks back to the bough, till, by the time we arrive at the acorn, it is useless to attempt to burden the mind with thinking of all its littl shell must contain. Yet we have only gone back a single generation. That acorn was itself borne on an oak, which sprang from an acorn; which must have held the type of the types in the second acorn, and of every leaf of the tree which the second acorn produced. In fact, if we conceive the oaks of the world to have sprung from a single acorn, on the hypothesis of those who deny the specialty of the vital principle, that acorn must have had in itself the types of all the oaks that have ever been produced; for agencies like those of electricity, heat, &c., can only work upon what they find; and if there should be a bud on the last oak that had not its representative in the first acorn, that bud would be a result without a corresponding antecedent.

Let us remark that the difficulty we here encounter arises from the peculiar manner in which life is transmitted from living things to their successors. No difficulties of this kind meet us in the history of the drop of water, though we saw that it had a history reaching back to the very limits of time. Its particles may have been many times round the world; some of its parts may have been ice, or steam, or free oxygen and hydrogen. The changes they may have passed through quite exceed our calculation, but they were either chemical or mechanical; and though the series is long, the terms are simple—there is no accumulation of difficulty. In the case of crystals the nucleus is the requisite initium, or rather the molecules of the mineral substance have characters sufficient to determine the form of the crystal. But the crystal is homogeneous, and has no special organs such as are possessed by plants and animals. The production of such an organ as the eye from the mere development of physical peculiarities pertaining to the germinal vesicle and spot can hardly be conceived to be possible.

In fact, the notion that every seed contained the perfect plant in miniature belonged to a former age, and has long been regarded as exploded; yet it seems to me to be an inevitable conclusion if nothing is concerned in the germination and subsequent growth of the seed but such as are of the nature of light, heat, electricity, &c. It is, however, certain that the seed possesses vitality; and without attempting to define what the vital principle may be, it appears reasonable to assign to it a certain special directive agency, by virtue of which it is enabled to apply the ever-present forces of heat, actinism, &c., in the construction of a plant of its own proper species. But it is manifest that a vitality thus endued cannot be correlative with any of the known imponderable forces.

Another point worthy of attention is the stability of form and structure observable through many generations of the same species. Whatever may be the wondrous exactness of the embryo prototype, the accidental influences of heat, moisture, light, &c., in the course of growth are so great that it is hardly possible to conceive that, with *only* the physical character of the embryo (itself produced under these varying agencies) to determine the succeeding form, similarity should be preserved through so many ages and in so many collateral lines of descent

The termination of life, on the supposition that the vital principle is analogous to known forces, ought to yield indications of some kind or other. Death is a common event, and one that lies completely open to observations of all kinds. In the animal world it is in our power to produce death under almost any conditions we may choose as most favorable for investigation; and it is an event so interesting that the discovery of its nature would be considered the greatest achievement of the intellect of man; yet the nature of death is absolutely unknown. The most delicate tests for indicating minute changes in electrical, thermal, and other conditions have been applied at the moment of death, and have shown no sign. Now it is certain of the forces of heat, light, motion, &c., that they are absolutely indestructible; they may be converted one into another, but they cannot cease to exist. If the vital principle was analogous to these agencies it might escape in any one of them; but of this no well-ascertained trace has been observed in any investigation of the phenomena of death.

The uniformity of the period through which life extends in any given species is a result that no one would anticipate if the vital principle be of the nature of the known forces. The physical elements of animals are said to undergo an entire renewal in the course of a few years. Surely, then, with renewed materials to work upon, and with something akin to the imponderables to do the work, animals should never grow old. Why should they—any more than oxygen should lose its vigor and grow tired of uniting with hydrogen?

We have now noticed some of the considerations which appear to favor the supposition that the vital principle is a thing *sui generis:*

1st. The unparalleled hiatus which exists between things animate and things inanimate.

2d. The great dissimilarity between the properties of the imponderables and those of vitality.

3d. The difficulty arising from the hypothesis that the embryo of a living thing is developed only by agencies analogous with known forces.

4th. The permanence of form and structure observable during many generations of the same species.

5th. The absence of any indications as to what becomes of the vital principle at death.

6th. The periodicity of life.

It is not contended that these considerations amount to a demonstrative proof that the vital principle is a thing *sui generis*, but the question naturally arises, how far, supposing our conclusion to be a right one, a demonstrative proof is possible.

An instance somewhat parallel may be found in the field of natural theology, in which is assumed, as a starting point, the existence of a first Supreme Cause, whose nature is beyond all comprehension; certain facts are adduced which agree with this assumption; also, its rejection is shown to involve many great difficulties; but all this is not proof; and in fact proof here seems to be out of the question, for the simple reason that whatever *that* might be of which the existence could be proved, it would not be the Infinite One. In logic, as in mechanics, action and reaction are equal, and the very *locus standi* required by a proof, in the thing to be proved, puts demonstration in this case out of the question. Similarly, if the existence of vitality as a distinct thing were capable of being demonstrated by direct proof, the vital principle must be of the nature of other agencies, which is contrary to the original hypothesis.

It would be very interesting to trace the course of those discoveries in physiology which have led some eminent observers to class vitality with other known forces. It cannot be denied that a very large number of facts connected with the phenomena of life, formerly supposed to be attributable only to the undefined agency of the vital principle, are now accounted for on principles which are purely scientific. For example: the constituents of some of the proximate elements of organic substances, such as starch, albumen, &c., were known long ago; but the power to combine these constituents so as to produce the proximate elements was regarded as being possessed by the vital principle alone, the working of which in the formation of the proximate elements could, it was thought, by no means be imitated in the laboratory. This is now known to be an error; the chemist by his science does that which before was considered to be the peculiar function of the vital principle. In these and many other instances it has been proved that the aid of the vital principle has been unnecessarily invoked to account for results explicable on scientific grounds.

In all this we have a parallel to that which has taken place in the more extensive field of the cosmos. Yet there are some who are conscious that if it were possible to trace the existing state of things by a regular series of scientific deductions to a nebular condition of the universe, no approach would be made thereby to the possibility of dispensing with a first Supreme Cause.

It is, however, abundantly evident that the great Originator of nature has chosen to accomplish His purpose by a wondrous succession of fixed laws, open to our investigation, and revealing depth beyond depth to a most remote and obscure profundity.

With this fact before us, on the hypothesis that the force of life, like its Author, possesses an unknown, perhaps an unknowable nature, we should not expect its interposition to be conspicuous; we should, I think, expect its agency to be executed only behind a succession of fixed laws, forming a vista, having its termination almost lost in the distance. The natural evidence in the two cases must of necessity, as we have seen, be of the same character, not direct nor demonstrative, but implied and inferential.

Whatever weight may be due to the considerations here urged in favor of the specialty of the vital principle, at all events they answer these expectations and conditions. Those arguments on which most stress has been laid arise at points where, if evidence be possible, any one who thinks at all on the subject would expect to find it. Something altogether unusual in nature marks the boundary of the province in which life-force prevails; and the phenomena of life do not submit to be accounted for under the rigid laws which govern the application of all forces that are known.

APPENDIX.

BY THE SECRETARY.

In the early study of mechanical and physiological phenomena, the energy which was exhibited by animals, or, in other words, their power to perform what is technically called work, that is to overcome the inertia and change the form of matter, was referred to the vital force. A more critical study of these phenomena has, however, shown that this energy results from the mechanical power stored away in the food and materials which the body consumes; that the body is a machine for applying and modifying power, precisely similar to those machines invented by man for a similar purpose. Indeed, it has been shown by accurate experiments that the amount of energy developed in animal exertion is just in proportion to the material consumed. To give a more definite idea of this, we may state the general fact that matter may be considered under two aspects, namely, matter in a condition of power, and matter in a state of entire inertness. For example, the weight of a clock or the spring of a watch when wound up is in a state of power, and in its running down gives out, tick by tick, an amount of power precisely equal to the muscular energy expended in winding it up. When the weight or the spring has run down, it is then in a condition of inertness, and will continue in this state, incapable of producing motion, unless it be again put in a condition of power by the application of an extraneous force. Again, coal and other combustible bodies consist of matter in a condition of power, and in their running down into carbonic acid and water, during their combustion, evolve the energy exhibited in the operations of the steam engine. The combustible material may be considered the food of the steam engine, and experiments have been made to ascertain the relative economy in the expenditure of a definite amount of food in the natural machine and the artificial engine. The former has been found to waste less of the motive power than the latter.

In pursuing this train of investigation the question is asked, "Whence does the coal or food derive its power?" The answer is, that these substances are derived from the air by the decomposing agency of the impulses from the sun, and that when burned in the engine or consumed in the body they are again resolved into air, giving out in this resolution an amount of energy equivalent to that received from the sun during the process of their growth. All the materials of the crust of the earth, with the exception of coal and organic matter, are in a state of inertness, and, like the burnt slag of the furnace, have expended their energy, and in this condition of inertness they would forever remain, were it not for extraneous influences, principally the sun.

From this point of view the phenomena we have been considering consist merely in the transfer of power from one body to another, and from a wide generalization from all the facts the conclusion has been arrived at that energy is neither lost nor gained in the transfer; and, pursuing the same train of reflection, we are finally led to the result that all power is derived from the primordial, unbalanced attraction and repulsion of the atoms of matter.

In the gradual development of the principles we have given there has been a tendency to extend the views we have presented too far, and to refer all the phenomena of life to the mechanical or chemical forces of nature. Although it has been, as we think, conclusively proved that from food, and food alone, come all the different kinds of physical force which are manifest in animal life, yet, as the author of the preceding paper has shown, there is something else necessary to life, and this something, though it cannot properly be called a force, may be denominated the vital principle. Without the influence of this principle the undirected physical powers produce mechanical arrangements and assume a state of permanent equilibrium by bringing matter into crystalline forms or into a condition of simple aggregation, while under its mysterious influence the particles of matter are built up into an unstable condition in the form of organic molecules. While, therefore, we may refer the changes which are here produced, or, in other words, the work performed, to the expenditure of the physical powers of heat, chemical action, &c., we must admit the necessity of something beyond these, which, from the analogy with mental phenomena, we may denominate the directing principle. Although we cannot perhaps positively say in the present state of science that this directing principle will not manifest itself when all the necessary conditions are present, yet in the ordinary phenomena of life which are everywhere exhibited around us, organization is derived from vitality and not vitality from organization. That the vital or directing principle is not a physical power which performs work, or that it cannot be classed with heat or chemical action, is evident from the fact that it may be indefinitely extended—from a single acorn a whole forest of oaks may result.

The principles of which we have here endeavored to give an exposition are strikingly illustrated in the transformation of the egg when subjected to a slightly elevated temperature. The egg of a bird, for example, as we know, consists of a congeries of organized molecules or vesicles, enclosed in a calcareous shell, thickly punctured with minute holes, through which the oxygen of the air can enter, and vapors and gases escape. Let us observe the difference of changes which take place in two newly-laid eggs, one of which is not possessed with vitality, and the other is endowed with this mysterious principle. Both of these eggs are in a condition of power, the carbon, hydrogen, nitrogen, sulphur, &c., of which their organized molecules are composed, are in a state of unstable equilibrium, and ready, when set in motion by a slight increase of temperature, to rush into the more stable compounds of carbonic acid, vapor of water, &c., by chemical attraction. While the eggs are in an unchanged condition they possess the same amount of what is called potential energy, which in both cases will be expended in the transformation of the materials; but how different will be the effects produced. In the case of the egg deprived of vitality, all the organized molecules will be converted into gases and vapors, with the development of heat and an elastic energy, in some cases sufficient to burst the shell, the power originally stored away in the egg being thus dissipated, in the production of chemical and mechanical changes. In the case of the egg possessed of vitality, a portion of the organized molecules will also run down into vapors and gases, which will gradually escape through the perforations of the shell, and will thus, as in the previous case, evolve an equivalent amount of power; but this, instead of being dissipated in mere mechanical or chemical effects, will be expended, under the directing principle of vitality, in elevating to a higher degree of organization the molecules of the remainder, and in transforming them into organs of sensation, perception, and locomotion; in short, in the production of a machine precisely similar to those constructed by the intellectual operations of man when guiding or directing the powers of nature. If we examine the transformation as it goes on from day to day, we shall see that it does not consist in a simple aggregation of particles in the production of the organs we have mentioned, but in preliminary arrangements, such as canals, and provisional, afterwards to be obliterated, and the adoption of means for a more remote end, the whole indicating an intention realized in the sentient, living, moving animal.

This vital principle, from strict analogy, cannot be considered as an essential property of matter, since it is only continued by transmission from one living being to another. It is true that it ceases to manifest itself when a slight derangement takes place in the organized material with which it is connected, and death ensues; but this is precisely analogous to the manifestation of the thinking, willing principle within us, the existence of which is revealed to us by our own consciousness as a primordial truth, beyond which nothing can be more certain.

INSTRUCTIONS FOR COLLECTING LAND AND FRESH-WATER SHELLS.

BY DR. JAMES LEWIS, OF MOHAWK, N. Y.

Explorations.—Before the collector can enter the field with much certainty as to the anticipated result of his labors, it may be necessary for him to satisfy himself that there are in the district about him shells enough to offer encouragement. It will be found, generally, that those sections of the country that have a dry sandy soil are unfavorable for the production of molluscs. Regions in which pines abound are proverbially of this character, and here the efforts of the collector are usually but indifferently rewarded. In the moist alluvial soils of limestone formations are found the most favorable conditions for the production of molluscs. This is more notably true with regard to land shells; aquatic species are also affected similarly, but less conspicuously, by the character of the soil. But it will almost invariably be observed that waters deficient in lime do not produce shells as perfect nor in as great numbers as waters charged with this earth.

Land shells.—With a few exceptions, relating to some of the smaller species, and also a few species of semi-aquatic habits, the land shells of this country are found most abundantly in the wooded alluvial regions, where during the day they are concealed under fragments of fallen trees, bits of bark, chips, &c., sometimes concealed under leaves or in the tufts of rank growths of moss. Some species will be occasionally found in the moist debris of shaly rocks in ravines. Species peculiar to the southern States are sometimes met with on shrubs and trees. But few species living in the northern or central portions of the country are often so found. Some species of semi-aquatic habits, though occasionally seen on the rank vegetation along rivers, (sometimes several feet from the ground,) are more frequently observed under bits of flood wood, leaves, &c., near the muddy slopes of streams or ponds, or in the vicinity of water, where they may find concealment either in grass or under the shade of aquatic plants.

Among our most minute species are those that delight in wet grass lands, or in localities that are usually moist during a very considerable portion of the year. They are sometimes found in such localities congregated in hundreds under stray fragments of boards, bits of wood, &c.

As different sections of the country offer constantly varying conditions affecting the habits of land shells, it may be expected that some species that usually are found in such stations as have just been indicated may in exceptional instances be found under circumstances where the collector might least expect to discover them. It accordingly becomes the collector to be at all times on the alert, and to inspect every variety of station. By doing so, he will often unexpectedly discover desirable species, and acquire information respecting their habits, of more value to him than any suggestions that might be conveyed to him by a volume of printed instructions.

As examples it may be stated that in and around dilapidated buildings, where fragments of bricks and mortar cover the ground, large numbers of the smaller species of *Helix*, *Pupa*, *Carychum*, &c., will be found. They adhere to the under surface of a porous brick in preference to a fragment of gneiss, limestone, or other rock. Also the cavity of a decayed tree or stump, when examined in the early days of spring, will reward the searcher abundantly. Rich harvest may also be frequently gathered by laying boards upon the grass or ground; wetting them previously, unless immediately after a rain. On taking them up after a night's exposure large numbers of shells will often be found attached to the under surface.

Fresh-water shells.—While searching for those species of land shells that are found usually near water the collector will often have his attention drawn to air-breathing molluscs that are properly designated aquatic mollusca. The habits of some species of this class are such that by one unacquainted with them they might be confounded with land shells. Many of these species have a habit of crawling out of the water, remaining on the moist mud without inconvenience. They will also sometimes be found on the stems and leaves of aquatic plants, or on other projecting substances several inches from the surface of the water. In their habits as a class they are adapted to a wide range of conditions, so that they will be found in lakes, ponds, rivers, canals, ditches, stagnant pools, swamps, and small rivulets, though some species seem to be adapted to a narrow range of conditions; the class, however, has its representatives over the whole continent. Though by far the greater number of species of molluscs belonging to this class prefer shallow water, feeding on the vegetation that abounds in such stations, there are a few exceptions in which species are found adapted to deep water, in which it is improbable they can reach the surface to respire the air. The collector will find many species accessible to him along the margins of water. Others will require the aid of a boat, especially such as are found feeding on the weeds in lakes and rivers. To discover some of the minute species found under such circumstances it may sometimes be advantageous to gather handsful of the weeds and gently lift them out of the water. If the operation be rudely performed, the molluscs may be disturbed so as to detach themselves. Many species will be found adhering to the grass-like plants that grow in streams. Others adhere to the stems of flag and bullrush, and may be discovered very readily by pulling up the plants by the roots, taking care to perform the operation gently and deliberately. Of analogous habits with some of the above are certain small species found concealed under stones just below low-water mark in rivers. They are sometimes also found adhering to larger shells. This class embraces only small cup-like shells—"fresh-water limpets."

Aside from the air-breathing aquatic molluscs, we have others whose respiration is aquatic. The necessities of the respiration of the water-breathing molluscs restricts them to a narrower range of circumstances than those to which the air-breathing molluscs can conform. Hence they are not usually found in stagnant waters—certainly not in waters of limited area, where impurities are generated by decomposing substances. The largest shells of this class are found in the swamps along the rivers of some of the southern States, and are objects of interest on account of their habits as well as on account of their value in the cabinet. In their season of active life they are found feeding on aquatic plants. Inhabiting localities subject to drying they burrow in the mud as the water diminishes. The collector will for convenience seek them when they are active. Another class, smaller than that just mentioned, but affording a greater number of species and varieties, is more widely distributed, being found not only in the waters of the various States, but also in Canada. They inhabit rivers, lakes, ponds, and canals, and when circumstances favor their habits they will be found most abundantly burrowing just beneath the surface of the soft mud near the shores, where undoubtedly they are attracted by more abundant supplies of food, and perhaps also by a more agreeable temperature. They will often be found in the muddy banks of rivers in great numbers, congregated at the margin of the water. In canals where conditions of food and temperature are very favorable, they attain a more luxuriant development than in neighboring rivers. Some localities are remarkable for affording varieties and monstrosities. Next to this class in size is one that embraces a large number of species included in several genera and sub-genera. The shells vary in form from a slender turret to a globular form, variously colored, and sometimes curiously ornamented with tubercles, ridges, and carinations. With a very few exceptions these interesting shells are found only in rivers and perennial streams. The different genera of this class seem to be adapted to certain modifications of conditions. Some of these molluscs prefer muddy, sloping river-banks, where they crawl in the comparatively still water on the surface of the mud. Others prefer the rapid current among the rocky portions of streams, where they are found adhering to the surfaces of the rocks. The habits of nearly all molluscs of this class are such as bring them to the shallower portions of the water they inhabit. They can often be reached from the shore with the hand.

By gradual transitions these genera, with their numerous species, are followed by other and smaller genera, some of which are of comparatively limited range; others are widely distributed over the whole country. Nearly all of them have habits in some respects similar to the last preceding class, and will be found on the muddy bottoms of shallow portions of rivers, lakes, &c., or feeding on aquatic plants. The small size of many of these shells renders them somewhat difficult to discover, unless the collector has expedients for securing them with ease and certainty.

Bivalve shells next claim attention, and for convenience they will be considered under two classes, though embracing several distinct genera and species. A class of shells (none of which ever attain dimensions much exceeding half an inch) inhabits nearly every perennial stream having a muddy bottom; found also in stagnant waters, lakes, ponds, canals, and, indeed, in every station fitted for mollusc life. Some species inhabit stations subject to drying during a portion of the year, and careless observers have been deceived on finding them alive in their dried *habitat*, and have inferred they were bivalve land shells! All the shells of this class burrow just beneath the surface of the mud, and are usually found in greatest abundance near the margin of the water, or where there is but little depth. This class embraces some species remarkable for their fragility, others equally remarkable for their minuteness. They are distributed over all the explored portions of the country.

Fresh-water muscles.—This class of shells embraces several genera, which on account of the great number of species contained in them will eventually be more minutely classified in sub-genera. No country in the world produces a greater variety of forms in this class of shells than the United States. In the northeastern portion of the United States the number of species found is comparatively small; but in the south and west the number of species becomes great, and the variety and beauty discoverable in the almost endless species make this class one of great interest to the collector. These molluscs inhabit lakes, rivers, and canals. Stagnant water is unfavorable to them. They afford abundant food for the muskrat and mink, who collect piles of shells on the shore where they bring the molluscs to feed upon them. The shells left by the muskrat sometimes serve as a resort for the collector who is not critical to have the best of specimens, while they should serve to point out to him that there are good specimens, alive, not far distant. Except in shallow portions of rivers it is sometimes difficult, however, to find muscles; but where the water is not so deep but that a person may with security wade in it, it is comparatively easy to discover them. They will usually be found partly buried in the mud or gravel, only enough of the shell projecting to enable the mollusc to extend the syphons of his breathing apparatus into the water above him. A little practice will enable the collector to detect the projecting shell. In lakes and ponds, where the water is not too deep, the collector may readily discover the objects of his search from a boat.

Having thus in general terms given such suggestions as will enable the collector to seek shells understandingly, it will now be proper to speak more particularly of

Collecting.—To obtain land shells the collector needs for the larger species a suitable tin canister, or a number of them, (the cover being perforated to admit air,) to serve as a receptacle for his gatherings. He will soon learn to distinguish immature shells, and unless they are to serve some special purpose it may be well not to secure many such, as, by clearing a station of all that may be found in it, future visits to the same place may not be remunerative. For small shells that will not admit of the same modes of treatment applied for the preservation of large shells, the collector should be provided with a small bottle of alcohol in which to put them. He will find his labor facilitated if he is provided with a pair of light, elastic pliers, the jaws of which are hollowed a little so as to embrace any small object taken between them. With such pliers very small and fragile shells may be picked up and transferred to the bottle with little risk of breaking them, and with much more rapidity than with the fingers alone. The pliers will be found very useful also for handling other small shells.

Such aquatic species as are found where it is most convenient to pick them one by one, may be handled with the pliers; others that are attached to the stems of aquatic plants, especially the class called fresh-water limpets, can best be managed with the blade of a knife, which, being slid under them, serves to convey them to the bottle.

Other aquatic species may be rapidly gathered by means of a perforated tin dipper attached to a wooden handle, which may be jointed like a fishing rod, so as to be available in a variety of stations. The dipper can be used to secure nearly all classes of aquatic shell animals when they happen to be within reach, especially those that burrow in mud, as well as those that crawl on its surface. It may be used among weeds and aquatic plants on which molluscs are feeding.

A bucket partly filled with water is a suitable receptacle for the shells taken with the dipper. In taking the shells that are found in mud, much care is sometimes requisite in sifting out the mud to avoid crushing fragile species. Perhaps a better mode in some instances is to be provided with a large culinary sieve through which to sift the mud, the tin dipper serving simply as a dredge. The sieve, when filled with mud and shells, is to be carefully manipulated in the water until all the mud and sand is washed away. The sifting process may be repeated upon fresh dredgings as often as is necessary, or until the collector has as many shells as he desires. The collector will find more eligible for his purpose a net made of wire gauze, of from twelve to sixteen wires to the inch. A convenient size and form will require an aperture for the mouth of the net about four by eight inches; the depth of the net eight inches. It should be attached to a jointed handle for drawing it along the surface of the mud to be dredged; the margin that enters the mud should have a sharp metallic edge, like a hoe. The net will require a sweep of from one to six feet to fill it with mud; it is then turned over, mouth upwards, and while yet in the water the mud is sifted out. The shells remaining are carefully turned into a bucket of water. With a net of this character the collector may stand in one position and dredge a large area in a very short time, often securing shells in great abundance and with comparatively little effort. The saving in time and labor can only be appreciated by trial.

For collecting fresh-water muscles, when they are found where they can without much inconvenience be picked up by the hand, the collector may dispense with all aids but a basket in which to place his treasures. As he will find species of varying degrees of fragility, all more or less easily broken if carelessly handled, he will observe to place his shells carefully in his basket, and not let them break each other by rude contact. In stations where it may be inconvenient or impossible to wade, a rake, provided with numerous teeth set close together and having a long handle, has sometimes been used with the most gratifying success. In lakes or ponds of moderate depth, where the bottom is readily seen, the dipper sometimes answers if the bottom is mud. In gravelly bottom a pair of tongs, the blades of which are like huge spoon-bowls, may be used with certainty. The tongs should be made so as to be attached to a sufficiently long rod or jointed pole for a handle, the moveable blade being operated by a string held in one hand, while the other hand guides the instrument to its work.

It is often the habit of the inexperienced collector to content himself with such specimens as from their attractiveness to his eye seem to be all that is worth preserving. Such specimens alone fail to satisfy the wants of the naturalist who extends his interest to every class of natural objects. For this and other equally important reasons the collector is earnestly recommended to secure specimens of every species that comes before him. If he cannot always obtain the best specimens he may serve the interests of science sometimes by preserving specimens that, in the presence of good living specimens, he would cast aside. Dead shells, though not always desirable in a collection of specimens, may serve to point out to the collector where he can find living ones. Dead shells may be preserved if no others can be found.

The collector often passes over species that are abundant at the moment, thinking he can obtain them at any time. This sometimes is a fallacious indulgence, as the habits of many species are affected by inappreciable influences, and often those species which have been abundant cease to be so, much to the disappointment of the collector when, perhaps too late, he has learned they have a value and importance of which he might have availed himself in his correspondence with others.

Preparation of specimens.—The larger land shells, the larger fluviatile gasteropods, and the large bivalves may have their soft parts readily removed after having been boiled in water a few minutes. After the soft parts have been removed by means of a small metallic hook, each specimen should be well washed, inside and out, as far as practicable. A small syringe serves readily to rinse the inside of univalve shells. A bit of stick with a tuft of cotton twisted on it is often an indispensable aid in completing the process A tooth-brush is useful in washing the exterior of shells. Operculated shells should have the opercle replaced within the shell to which it belonged, and if necessary it may be retained in its natural position by means of a little thick mucilage. Large bivalves may be tied with a bit of yarn to hold the valves together; or they may be wrapped in paper to secure the same object after the surface of the shell is dry and the hinge ligament is yet pliable. Care should be taken in the preparation of bivalves not to fracture the thin margins of the shell, nor to tear the epidermal fringe. Indeed, the same care should be taken to preserve *all* shells as perfectly as possible.

Small bivalves that cannot be submitted to the boiling treatment and afterwards closed, as recommended for muscles, and also any very small shells of any class, may be placed in alcohol for twenty-four hours, after which they may be dried in the shade on paper. They may be dried without the use of alcohol, but then the soft parts are longer in drying, and require a longer time to become divested of the unpleasant odor of decomposition. The appearance of the shells is more cleanly after alcoholic treatment, for the reason that the soft parts of the molluscs are contracted into the smallest dimensions thereby.

It sometimes becomes desirable to cleanse the surface of small fluviatile shells, bivalves, and univalves, together. With just enough water to cover them in the bucket in which they were collected add as much clean, sharp sand as will equal the bulk of the shells. Agitate the bucket for a few moments by a rotary and shaking motion. The friction of the shells and the sand on each other soon brightens the surface of the shells. The sand may subsequently be sifted away in water; the shells may then be rinsed, when they are ready for final treatment, with or without alcohol.

Some shells of considerable size may have the soft parts removed after having been in alcohol a few hours; but having been several days in alcohol (unless it is of low proof) the soft parts do not disengage so readily. Among the shells to which this mode of treatment best applies are land shells with an elevated spire, aquatic species with a short but elevated spire, and fresh-water limpets.

It is not recommended to retain shells in alcohol a great many days, unless it be for the preservation of the soft parts for anatomical investigation. If too long retained in alcohol the shells acquire a dark, smoky look that diminishes their beauty.

INCIDENTAL REMARKS.

With the result of his labor before him the collector desires such information respecting his shells as will enhance the pleasure and other advantages he expects to derive from them. He also desires that they should present, in the most obvious manner, all their intrinsic beauties. Perhaps he forgets that they are most beautiful as nature made them, and seeks to improve them by varnishing. Such shells as the collector designs to preserve in his own cabinet he may varnish if he pleases; but should he desire to acquire other specimens by exchanging his duplicates with a friend, let them be without varnish or any artificial treatment other than judicious cleaning.

In arranging a series of species in his cabinet the collector, if unacquainted with their names, may group his species according to those analogies and obvious resemblances that show their relations to each other. He should attach a label to each species, designating it by a number until he has opportunity to replace the number with the specific name. The label should have upon it the locality where found, and any other facts that might be of remarkable interest to the naturalist. The numbers he attaches to his species may serve as the means of ascertaining names by correspondence. Each species should be marked, so that no confusion may arise by displacement of labels.

Should the collector find it convenient to correspond with some other person similarly engaged, or should he desire to transmit a package of shells to some friend or to some public institution, a few suggestions relative to packing shells for transportation may be of use. Obtain a sufficiently strong box of suitable capacity; on the bottom should be laid a thin bed of soft hay, or some other suitable elastic material. The larger shells (if such are to form a part of the contents of the package) should form the lower tiers in packing. Each shell should be wrapped in paper, and the specimens should be so compactly stowed as not to shake about. The lighter shells should occupy the upper portion of the package, and those of a fragile nature should be protected from the possibility of fracture by being enveloped in cotton and enclosed in suitable small boxes. Small paper bags or cartridges may serve to contain minute species; but a more satisfactory mode is to wrap them in cotton and put them in small paper boxes, such as may be purchased at any drug store. Each species should be appropriately labelled, giving, in legible characters, the generic and specific name of the shell, if known, after which should be written the name of the author by whom the species was published; under this should follow the locality where found. Any vacant space at the top of the package may be filled with any light material that will serve to keep the contents of the package from shaking about after the box is closed. The top of the box should be carefully nailed secure, and plainly marked with the address of the person to whom it is to be forwarded, also designating the mode of conveyance for the guidance of parties to whom it is delivered for shipment.

ON THE COLLECTION OF SPECIMENS OF MYRIAPODS, PHALANGIDÆ, &c.

ACADEMY OF NATURAL SCIENCES,
Philadelphia, February 13, 1866.

DEAR SIR: My excuse for troubling you with the following communication must be my great desire to render more complete the already published monographs of the North American myriapods or centipeds, and also to obtain material for a similar memoir on the phalangidæ or "harvestmen" of the United States. As you are aware, the myriapods are divided into two great sub-orders, the more highly organized of which live on animal food, while others find their sustenance in decaying vegetable substances. These two sub-orders may be distinguished at a single glance by the most inexperienced eye; the first, the true centipeds, by their flat body, each segment of which has but a single pair of legs, and then a head, armed with a formidable pair of jaws; the second, the millepeds by their body being more or less terete or cylindrical, with two pair of legs to each segment, and the jaws of their head not being very apparent. The former may be found under stones, logs, beneath the bark of dead trees, about wood piles and out-houses, in fact, in almost every place which affords them shelter, and is not too wet. The larger species of this group are probably all known, but specimens, with the localities from which they have been obtained, are valuable, as they throw light on the problem of geographical distribution. Among the smaller species, at least the long, narrow-bodied species, (geophilidæ,) there are probably many species still unknown, and of these I would especially solicit collections.

The millepeds are to be looked for in decaying wood, beneath the bark of old logs, &c., as well as in similar situations with the last. The number of facts still unknown respecting these myriapods, and of species new to science, is probably very great, so that collections of them are even more valuable than those of the smaller centipeds. As in most other cases in natural history, the small specimens are the most valuable, affording the largest per cent. of unknown and rare species. When a myriapod is captured it should be thrown into alcohol, the only precaution necessary being to have the alcohol very strong in the case of millepeds, since they are very apt to fall to pieces if the preservative fluid is weak. It is not necessary to separate the species, but the general locality should always be marked both by a label pasted on the bottle, and also by one placed in the inside, the collector's name being distinctly written so as to facilitate acknowledgment.

The phalangidæ, "harvestmen," or "daddy long-legs," of the schoolboys, resemble the true spiders, from which they are distinguished by the great length of their slender legs, by their abdomen being sesile, *i. e.*, attached to the whole breadth of the anterior portion of the body, (the cephalothorax,) and unprovided with a spinning apparatus, as well as by their head, having a single pair of eyes, elevated on a common peduncle or process, and being armed with a pair of two-fingered mandibles. The female deposits her eggs in crevices, in walls and rocks, beneath stones, &c., in the autumn, and then dies. The following spring the eggs are hatched and produce minute individuals, which resemble the mature specimen. These grow through the summer, and reach their full size in the early autumn, when the sexes pair. They are preferably nocturnal in their habits, generally hiding under stones, on walls, &c., during the day, and during the night hunting their food, which consists of flies, gnats, and other small insects. They do, however, sometimes hunt during even the brightest part of the day. They should be collected during the latter part of the summer and the early autumn, because at that time they are mature. The collector should look for them on the walls, and about the eaves of barns and out-houses, along stone and other fences, beneath stones in dry or even moist gravelly places, under sticks, fence-rails, logs, and the like in dry, grassy groves. In catching them care should be taken not to break their legs, and they should be dropped into a bottle containing very strong whiskey or ordinary alcohol, or spirits of wine. When practicable each species should be preserved separately in homœopathic or other small phials, and an accurate account be kept of the localities in which they may be captured, and any observations on their habits will be especially valuable. After they have been put in spirits, no further care is necessary, except to see that the alcohol does not evaporate and leave them dry.

Hoping that you will be able to obtain collections, I remain yours, respectfully,

H. C. WOOD, Jr., *M. D.*

Professor Henry.

[The Institution will be pleased to receive from its correspondents the specimens asked for by Dr. Wood, to be placed in his hands for investigation.

J. H.]

A PLAN OF A RESEARCH UPON THE ATMOSPHERE.

BY PROF. C. M. WETHERILL.

Washington, D. C., *July* 24, 1865.

Dear Sir: I beg leave to submit to your consideration the accompanying sketch of a plan for securing the advantages of a research upon the atmosphere in our country, which would be not only new, but fruitful of important practical results. The work upon which I was engaged last winter, viz., the ventilation of the Capitol extension, has impressed me with the necessity of this research, and I hope that it will meet your approval.

I am, very respectfully, your obedient servant,

CHARLES M. WETHERILL.

Prof. Joseph Henry, *Secretary Smithsonian Institution.*

The need of a research upon the proportion of carbonic acid (and oxygen) in the atmosphere of the United States.

Plants require carbonic acid, animals oxygen, for their existence; hence an accurate determination of these constituents in the atmosphere is of vast importance, and the knowledge gained from such a research is of great value, both from a scientific and a practical point of view. The question has engaged the attention of scientists (in Europe) from the earliest period of modern chemistry, and its discussion has advanced step by step with the improvement of the methods of analysis. Through the labors of Gay Lussac, Humboldt, De Saussure, Dumas, Boussingault, Lewy, Schlagintweit, and others we are acquainted with the proportion of oxygen and carbonic acid in the atmosphere, and the influence, upon the quantity of these constituents, of day and night, winter and summer, rain and drought, of storms, and of elevation. These results have been obtained for Europe, with the exception of Lewy's experiments in South America, upon an atmosphere subject to disturbances from the carbonic acid emitted from volcanoes.

The natural tendency of a dense population and a sparse vegetation would be to increase the carbonic acid and to diminish the oxygen of the atmosphere; hence, the United States being in this respect different from Europe, it is of importance to know how our atmosphere differs, as to these gases, from the air of the old country. The research has never been made in America for obvious reasons.

Beside, the increased knowledge in respect to the nature of the atmosphere which such a research would yield, important practical results would flow from it with regard to ventilation, to the hygiene of cities and of military hospitals, to agriculture and the clearing away of forests, &c.

The examination of the ventilation of the Capitol extension, which has occupied my attention during the past six or seven months, has deeply impressed me with the great importance of experiments upon the atmospheric constituents, and practice in the kind of analyses required has led me to propose to co-operate with the Smithsonian Institution in the accomplishment of such a research.

In this co-operation I will undertake the analysis of specimens of air collected by myself where located, and of such as may be procured by the Institution from different points in the United States, the localities to be settled by the meteorological experience of the Smithsonian Institution. I will bear the expense of analysis myself, in respect to the determination of carbonic acid, and to a less extent of the oxygen. If, however, a large number of oxygen determinations are made, I would be glad if the Institution would furnish me with the apparatus of Regnault, or with some other improved one, in order to multiply the observations.

[The investigation proposed by Dr. Wetherill is one of much interest, and as full confidence is placed in his skill as an analytic chemist, and his consciencious accuracy in the statement of the results of his experiments, the assistance required will be furnished by the Institution as soon as the Dr. is prepared to undertake the research. It is probable, however, that he cannot commence the work immediately, since some time will be required to enable him to become familiar with the duties of his new position as professor of chemistry in the Lehigh University.

J. H.]

AN ACCOUNT OF THE CRYOLITE OF GREENLAND.

COMMUNICATED BY MESSRS. LEWIS & SONS.

[Messrs. Lewis & Sons, of Philadelphia, the proprietors of the mine of cryolite described in the following article, have kindly supplied the Institution with a quantity of the mineral for the distribution of specimens to such institutions as may desire to add it to their cabinets. The Institution is also indebted to the same gentlemen for the account here given and a large map which accompanied it.

The mineral cryolite derives its name (from *χρὺος*, ice) from its property of melting in the flame of a candle. Its composition is $Na. Fl. \frac{1}{3} Al.^2 Fl.^3 = \times$ Aluminum 13.0; sodium 32.8; fluorine 54.2.

It is valuable as an ore of aluminum, for when melted with sodium this metal is liberated from its combination with fluoıine, the result being aluminum and fluoride of sodium.

Since the high price of sodium restricts the useful applications of aluminum and its alloys, cryolite has lately become important as a source of soda, considerable quantities of which, with alumina as an incidental product, are manufactured in Pennsylvania from this mineral.]

COPENHAGEN, *November* 15, 1864.

GENTLEMEN: After staying a year at the cryolite mines, the nature of which I have thus had good opportunity of learning, I can give you the following information about them, as they were at the time of my departure in the autumn of 1864.

The cryolite mines are situated at Trigtut, (or Triktout,) on the south side of Arsut Fjord, in South Greenland, 61° 13′ north latitude and 48° 9′ west longitude. The surface of the vein of cryolite was originally covered with a layer of clay, gravel and earth, which loose material is now chiefly removed, there remaining only in the most southern part of the area of cryolite, a part not yet perfectly uncovered. The boundaries of the vein toward the surrounding mountain is everywhere very conspicuous, with the exception of a part of its northwest side.

The vein, the greatest length of which is about 600 feet, breadth about 150 feet, and extent of which can be reckoned at about 53,000 cubic feet, is of two parts, one close to the Fjord, the other to the east, and separated by a rock from 5 to 15 feet high and about 100 feet broad. The western part is washed by a little bay going in from the Fjord along its whole extent; this is now partly filled up, and will in time be perfectly separated from the sea, when the stone breakwater, now under construction, running in west-southwest and east-northeast direction, straight across the mouth of the bay, at a distance of about 250 feet from the cryolite mine, is finished, and when the whole of the bay inside the breakwater is filled up, which two works will in all probability be finished before the close of 1865. The rock bounding the vein is everywhere on the boundary line itself 10 to 15 feet high from west, and the south boundary rises tolerably quickly in towards land. The surrounding elevation is chiefly of granite, crossed in several parts by veins or layers running from northwest to south.

The principal mineral of the vein is, of course, the cryolite, but as constant accompaniments we find quartz, iron-stone, lead ore, copperas, sulphur, arsenic, tin-stone, (the last two very seldom.) These substances are not evenly distributed through the whole mass, but the cryolite is partly found quite clean and unmixed; while larger and smaller quantities are found containing more or less of the abovementioned minerals.

A strip of the vein only a few feet broad along the south and southwest boundary of the same is conspicuous for its richness in iron-stone and quartz, but especially for lead ore and copperas. The surrounding mountain is also, in certain places, viz., along the southwest boundary strongly impregnated with lead ore, copperas, and varieties of arsenic, tantalite, molybden ore and tin-stone. In the cryolite are found pieces of the surrounding mountain, both of granite and of trap; it is therefore quite certain that the cryolite has come from the interior of the earth by volcanic agency, and that it is newer than the granite and the trap. The melted mass of cryolite has, during its progress through the granite, torn off parts of the latter and enclosed them in itself. The cryolite vein is what is called a block with face towards south and southwest; in the south and southwest boundary of the vein the cryolite can plainly be seen going under the granite, in a very rapid dip, as much as 25° ; at one place, however, where the cryolite forms a kind of peninsula running west, the dip seems to be about 0° 12′; the cryolite appearing to continue under the mountain in a horizontal direction. As far as the quality of the produce is concerned, the cryolite vein consists of two different unequal parts, divided by a breakwater 60 feet high and 110 feet long, running northwest and southeast.

I. The part east of the breakwater, whose greatest length and breadth are about 235 and 100 feet, and whose total extent can be reckoned at about 16,000 cubic feet, consists of two parts—a northerly, 7,000 cubic feet in extent, which is totally of perfectly clean white cryolite, without any mixture whatsoever of unclean minerals, with exception of the mass or part on the surface of the angles or separation, which is colored red or yellow by a very thin layer of ochre or clayey iron; this part runs north and east right up to the granite. In the south and west it is surrounded by the second or southerly part, whose extent amounts to about 9,000 cubic feet, containing only impure cryolite, much mixed with quartz.

The north part is almost all mined to a depth of fourteen feet below high-water mark; the eastern part of the same only to a depth of 11 to 12 feet, and on the western corner a pump-shaft descends to a depth of little more than twenty-two feet below high-water mark. The southern part, on the boundary towards north, is worked to a depth of four feet below high-water mark, but it rises gradually towards the south until it is somewhat over high-water mark.

To keep the east mine free from water an engine is used which works four pumps, and has been more than sufficient to keep the working places free from water. The engine is calculated to raise the water from a depth of forty feet below high-water mark; until the whole mine exceeds this depth no greater power will therefore be required. The mine during the winter is of course, filled with snow and ice, and all blasting must, therefore, cease from the months of November to April. In the months from May to October the mine can be worked, except under unusually severe climatic conditions, and I think in this time, with a gang of fifty men, about 5,000 tons of cryolite can be had.

To convey the pieces broken out to the place where they are piled up, the two following means are used:

1. Toward the north on the granite rock before mentioned, which separates the eastern mine from the Fjord, is a horizontal road, running in a north and south direction on a level of one foot above the highest high-water mark, and on this road is laid a tramway running to a spot which has been filled up in the Fjord, in the middle of which is a turn-table and siding. The cryolite can be shipped from here with great ease, generally without the use of barges. At the southern

end of the road called the heading is a crane for hoisting the cryolite from the bottom of the mine to the tramway.

2. In the south side of the eastern mine is built an inclined iron road, leading from the southwest corner of the mine in a west-northwest direction up on to the breakwater, which forms the west boundary of the mine. Here the iron road is continued horizontally in the same direction, west-northwest, for about 100 feet, where, by means of a turn-table, it is connected with a cross line leading north, and with a stone basin built in the Fjord, where at low-water it has to be shipped in barges. At high-water it can be put at once on board the ships. Under the supposition that the dimensions of the mines remain unchanged in depth, the northern part will yield 630 tons of pure cryolite, and the southern part 610 tons of impure cryolite; estimating the weight of one cubic foot cryolite at 180 pounds, and a ton at 2,000 pounds.

II. The western part of the cryolite vein adjoins the breakwater towards the east which has been mentioned several times before; towards the west and south it extends up to the granite; towards the west the boundary is uncertain. The greatest length and breadth of this part, which on the drawing is marked "C," are respectively 200 and 120 feet; its whole extent can be reckoned at about 20,000 cubic feet, consisting of medium clean cryolite. About a third of this is blasted to a depth of four to five feet under high-water mark; the rest lies level with highest-water mark, with the exception of a small part extending from the southmost corner along the southeast boundary, where the cryolite rises to a height of five and a half to eight feet over high-water mark. In the front towards the west the cryolite is nine to ten feet over highest-water mark. In the extreme southeast corner of the west part a vertical shaft (with a profile twelve feet long and twelve feet wide) is blasted to a depth of about twenty-four feet under high-water mark; at this place the cryolite became better as we went deeper; it was whiter and less mixed with iron ore, sulphurous particles, and lead ore. By driving a network of horizontal galleries, crossing one another in the western part, from the shaft at the commencement, and supporting the passages with pillars, it will probably not be very difficult to procure, with a gang of about fifty men, 2,500 or 3,500 tons in the course of those months in which the open mine cannot be worked.

To keep these mines free from water, an engine with the necessary pumps is used, which, if the water streaming in does not in future increase in the twenty-four feet deep shaft, will be able to keep the mines clear of water to a depth of about forty feet under high-water mark.

Supposing that the length and breadth of this western part will remain unchanged in descending, each foot in depth will yield 2,700 tons of medium cryolite. The following remark should be made: towards the west the vein of cryolite forms a characteristic point or peninsula, (springing into the side stones,) which has been before mentioned. The space between this point and the stream, about 120 feet west, is occupied by a cupola-shaped hill, whose highest point is thirty-seven and a half feet above high-water. In the northeast part of this, several hollows are found, which have hitherto not been carefully examined, but in which traces of cryolite have been found. These holes have probably formerly been filled with cryolite, which has afterwards been removed or conveyed by running water or other means. From this and the nature of the cryolite in the projecting point, where the vein seems to go horizontally in, we have reason to suppose that cryolite can be obtained under the granite in the whole extent of "D." As it probably continues under the mountain with the same, or with slight variations, from the height with which it enters at the point, (or peninsula,) it will be easily worked by going in under the rocks with horizontal passages, in a level not under high-water (by which annoyance from water will be avoided) either from the sea-side or from the north or from the east.

Under the supposition of the doubtless volcanic origin of the cryolite, and, therefore, of the almost certain boundless depth of the vein, there can never be any fear of a lack of the mineral. The difficulties, chiefly with water, snow, and ice, will doubtlessly increase as the mines deepen, but with proper steam power and good engineering direction, these difficulties may be overcome. Supposing that the length and breadth of these different parts remain unchanged in descent, working the whole to a depth of forty feet under high-water mark, will give the following result:

A, whose medium level can be fixed at fourteen feet under high-water mark, will give 16,380 tons clean cryolite.. B, whose medium level is about two feet under high-water mark, will give 30,760 tons of dirty cryolite. C, whose medium level is about two feet over high-water mark, will give 113,400 tons medium cryolite. Working the whole to a depth of forty feet under high-water mark will, therefore, give about 160,000 tons cryolite of different qualities.

In regard to the relations and matters concerning the cryolite mines which are not mentioned here, I beg to refer to what I have formerly written from Trigtout.

Respectfully, PAUL QUALE.

EXTRACTS

FROM

THE METEOROLOGICAL CORRESPONDENCE OF THE INSTITUTION, WITH REMARKS BY THE SECRETARY, PROF. JOSEPH HENRY.

The object of the publication of the following correspondence and remarks, is to record isolated facts of interest, and to answer inquiries which are frequently made by meteorological observers.

From W. C. Dennis, Key West, Florida.

NOVEMBER 10, 1856.

I have been mindful of your remarks as to my opportunity to gather facts relative to surface evaporation. I have given a great deal of attention to the subject, but my principal object has been to learn the conditions which attend an increase of evaporation, for the purpose of applying them to practical utility in the manufacture of salt. There is a difficulty in the way of getting the yearly amount of evaporation from an ordinary surface, as I am convinced that evaporation goes on much slower from sea water as it approaches the state of saturation; consequently in order to get useful data to determine the yearly surface evaporation, the water employed should either be fresh, or kept at about the strength of sea water. So much difference is there in amount of evaporation from ordinary sea water and brine, nearly at saturation, that while the former evaporates at say 0.05 inches per day, the latter remains nearly stationary.

This fact I noticed frequently last summer, when the air was damp and heavy with dew at night. In the morning after a heavy dew, I often found brine that was very strong, weaker than it was the night before. This I suppose may be accounted for by the strong affinity water has for salt.

The hygrometrical condition of the air makes a great difference in the amount of surface evaporation. The past year has been very damp, but the rains have not been remarkably heavy, yet everything goes to show that there has not been much over half the evaporation that there was in 1851 and 1854, years in which there were heavy rains. Through this summer I did not observe the evaporation of quite 0.10 inch in any twenty-four hours. In 1854, in the month of May, I noted the evaporation in one day (twenty-four hours) of full 0.30 of an inch. From this it can be seen that in order to ascertain the average yearly evaporation from a given surface of the ocean, it will be necessary to make accurate *daily* observations for a number of years, with ordinary sea water kept at the same degree of density.

I have found that evaporation goes on faster, when the weather is clear and *calm*, from the surface of very shallow water than from deeper water; but when the wind blows sufficiently strong to form a ripple or wave, evaporation goes on faster from water that is six inches deep than from that which is not more than one or two inches. Some other of my observations lead me to think that very little heat is absorbed by perfectly *clear* water in the passage through it of the direct rays of the sun, and that the maximum heat to which it is possible for the sun's rays to bring water depends on the color of the bottom that reflects back those rays, as well as on the shallowness of the water.

REMARKS.—The difference between the evaporation from salt and fresh water is readily shown by covering the bulbs of two thermometers with linen cloth, and wetting one with salt and the other with fresh water. The reduction of temperature will be greater with the latter than with the former, provided the wetting be renewed from to time to time; but if the two linen envelopes be suffered to dry, the one to which the salt water is applied will continue wet the longer, and thus indicate towards the end of the experiment a lower temperature.

Although the difference of evaporation of salt and fresh water is apparently very obvious, yet it has been asserted by a distinguished meteorologist that the covered bulb of a thermometer sinks to the same degree whether wet with salt or fresh water, and the evaporation from the sea is the same as from fresh water lakes; but this, as we have seen, is not correct.

From the same.

JANUARY 8, 1857.

A most strange phenomenon occurred on the last days of last month. Large streaks and fields of fresh water were discovered in the bay to the north and northwest of this island. The water was of dark color, but otherwise clear, and so fresh that many drank of it. At the same time toadfish, eels, snakes, &c., which always inhabit salt water alone, came on shore in great numbers, dead and dying. For a time the existence of fresh water was not believed, the death of the fish being ascribed to the norther of the 22d and 23d of December; but four days ago fresh water was reported in the harbor of Key West. I examined, and found, at the last of the ebb-tide, water at 2½° Beaumér all along the wharves. At one place it was 3°, and when I got home I tried the water at my salt house, two miles distant, and found it 5°. The day before at salt house it was 4°. Evidently the water was fresher in the harbor the day before I tried it. I am continuing the examination, and will write you again. What is the cause of this? Westerly winds could not have deflected the waters of the Mississippi, Alabama, &c., here. Have not the waters of the Everglades, which have been very high for a year past, found cavernous outlets through the soft lime rock into the bay? This rock underlies the entire bottom of the Everglades.

From Rev. C. Dewey, Rochester, N. Y

MAY, 1857.

I think more attention ought to be paid to observations on the winds. It only needs a thought to convince us that the clouds alone can show the general current of the wind, and that the vane shows merely the under and local currents. The charts of some storms show a wonderfully strange direction of the wind in some parts of our State; the reason is that the surface winds are those which are exhibited.

The attention is directed more at this time to the upper winds, I know, and have no doubt that the direction of the wind in this State, and even New England, in Professor Coffin's admirable expositions of the winds in the Smithsonian Contributions, will be found not to correspond with the results obtained from observations for the next twenty years. There is too much reliance by observers now, I fear, on surface winds. I should rather give up the wind vane and direct their eyes above and below; the difference may not be great, but too much for the designed accuracy of results now and in the future.

REMARKS.—We fully agree with our much esteemed correspondent, whose remarks on any branch of meteorology are entitled to special attention, as to the importance of observations on the motion of the clouds, upper and lower, in determining the great currents of the atmosphere. The resultant winds of the valley of the Hudson, as determined by the surface wind, are north and south, while the resultant wind of the whole State has a westerly direction. Still we would not dispense with the use of the wind vane. There are many places where its indications are strictly correct, when it is placed at a proper elevation; and at almost any place of which the topography is well known an approximation to the true direction may be deduced from the directions indicated by the vane. Moreover, the vane serves to give the direction of the *changes* of the wind, an important element in the theory of the phenomena of storms; and even the indication which it affords of the lower surface winds is of value in perfecting our knowledge of the influence of moderate inequalities of the surface on the direction of currents of air and the distribution of temperature.

In the records of the motion of the clouds care must be taken to state whether they are the upper or lower.

From J. Baltzell, Tallahassee, Florida.

SEPTEMBER 16, 1857.

The regular wind on the Atlantic coast of Florida, from which is derived the health and comfort of the inhabitants in the summer, is a southeasterly one from the sea, filled with moisture, evidently having saline particles, and stimulating in its effects. The wind at this place, which produces a like effect in the summer, giving us life, health, animation, and comfort, is the southwest, which comes from the gulf of Mexico, about twenty miles distant; I think it is not as stimulating, or as highly charged with saline particles, as that on the Atlantic coast. With us the east wind is dry, parching, and uncomfortable, prevailing not longer than a week at a time, and, if for a month or more, sickness is sure to follow, with extreme lassitude, depression, and nervous uneasiness. On the Atlantic coast, as I was informed in a recent visit, the northeasterly winds produce these results. I think it will be found that the injurious effects of our east wind are not dependent upon the flat lands between this and Georgia. In general, the pine woods of this region are dry, with no water upon them, and they are generally regarded as very healthful; occasionally there are wet seasons, in which the low lands are for a short time covered with water.

I cannot give you information as to the prevailing direction of thunder storms, but will bear it in mind, and if I can obtain anything of value, will communicate it to you.

REMARKS.—The east wind, which reaches Tallahassee from the Atlantic, passes over the whole of that portion of Florida lying between the city and the ocean; a distance ten times greater than that traversed by the current from the gulf. This is sufficient to explain the greater heat and dryness of the air. The same amount of radiation from the sun raises the temperature of the land much more than it does that of the water; the specific heat of the latter being about six times greater than that of the former.

From L. F. Ward, Medina, Ohio.

OCTOBER 5, 1857.

I broke the tube of a standard barometer, and in trying to replace it, and preparing to do so, I wiped out the tube carefully with a wooden rod and cotton wool, and laid it by until I could prepare the mercury. Soon after laying it down it began to break up into sections, and some of those sections to split longitudinally. In two days it was broken into more than thirty pieces. The tube was ¾-inch diameter and ⅜-inch bore. Now, I am unable to give any reason for the breaking of this tube. Are you aware of its cause?

REMARKS.—This is so far from being an unusual phenomenon, that it has been observed by almost every one who has attempted to clean a barometer tube. It results from imperfect annealing, or, in other words, a too rapid cooling, which has left in a condition of opposing tension the outer and inner surface of the glass. The explanation usually given is as follows: When a tube of glass is heated to redness, and then exposed to air of a low temperature, the outer stratum is suddenly cooled, and its particles brought into a state of equilibrium, while the inner strata are still in an expanded condition; afterwards, when the latter shrink by gradually cooling, they tend to separate from the former, and thus produce a condition of unstable equilibrium. Hence the slightest scratch in the interior, of which the particles tend to separate, is sufficient to rupture the continuity, and cause the fracture of the glass.

That such a state of unequal density of the outer and inner strata does actually exist, is shown by the effect produced on a beam of polarized light transmitted through the tube at right angles to its axis; and this affords a ready means of detecting the existence in a piece of glass of defective annealing.

From Seth L. Andrews, Romeo, Michigan.

JANUARY 8, 1858.

The annual occurrence of shooting stars recalled to my mind a similar phenomenon witnessed many years ago, which, if it should not be already recorded, may be of some interest. Upon referring to my journal I found the following entry:

"DARTMOUTH COLLEGE, HANOVER, N. H., *August* 10, 1830.—Fair. The aurora borealis is very bright this evening; an arch is extended across the heavens about 5° above the horizon, with a dark cloud in the space below.

"Meteors (shooting stars) are darting across the heavens almost every minute; their apparent size varies from that of a spark to that of the planet Venus."

My recollection of the phenomenon is that they were more frequent than I have ever seen since. I have in mind the same phenomenon witnessed on the 9th and 10th of August, 1838; I was then at the Sandwich Islands. I am not able to find that I recorded it, and it is barely probable that it may have occurred a year or two later. On the first evening the meteors were most numerous and brilliant, some of them equalling Venus in the intensity of light.

REMARKS.—The dark segment under the arch is a general, though not an invariable accompaniment of the aurora. It may in part be due, as has been suggested, to contrast with the light of the aurora; but it cannot be entirely referred to this cause since there frequently remains after the light has ceased to appear, a dark cloud in the place previously occupied by the segment. The phenomenon is evidently connected with the precipitation of the vapor of the atmosphere at the time of the passage through it of the auroral beams. I have in some cases seen a perfectly clear sky almost instantly covered with a mistiness during a vivid display of the aurora. The same also sometimes occurs when no aurora is visible.

From James Balfour, Newport, Illinois.

SEPTEMBER, 1858.

The remarkably mild winter in the months of January and December has been sufficiently commented upon by the newspapers, but we cannot look to them for the cause. The west and southwest winds prevailed nearly all the time, especially in the upper atmosphere, very seldom changing to south or northwest. It is well known that changes in the weather are produced by the difference of temperature of the equatorial and polar currents. When two currents in different directions meet, clouds are invariably formed. When two currents unite in the same direction the sky becomes clear. On the 1st of February I had an opportunity of noticing three different currents in the air at the same time. In the lower atmosphere the wind came from the north; an east wind drove light cirri from Lake Michigan, and above them were transparent cirri coming from the southwest. Towards noon the two lower currents combined in one, having a northeast direction, which, gradually veering to northwest, ended in a snow-storm towards night, lasting till noon of next day.

On the 3d a west wind made a clear sky, with the thermometer at 10° Fahrenheit.

On the 10th of February we had a temperature of 12° Fahrenheit; on the 11th, 18° Fahrenheit, with a sharp west wind, whereby Lake Michigan showed the phenomena of rapid evaporation, the whole expanse of water appearing like a boiling caldron, light cirri over the lake indicating the transformation of vapors into clouds.

REMARKS.—It cannot be doubted that on the meeting of the contiguous surfaces of two streams of air moving in opposite directions, and having different temperatures, clouds must be formed, though I think it more probable that, in accordance with the theory of the late Mr. Espy, rains and violent winds are generally produced by an upward motion of the air, and the inblowing from opposite directions of currents of air to supply the ascending mass.

The phenomenon mentioned by our observer, of the fog on the lakes, is an interesting illustration of one of the ways in which fog is produced. While the atmosphere was at a temperature of 18°, very nearly, if not entirely, all of the vapor which it had contained was condensed, and it might be considered at that temperature as incapable of holding any moisture. At the same time, since the lake was not frozen, it had probably a temperature of about 40°, and would at this temperature give off a considerable quantity of vapor, when not resisted by the vapor already in the atmosphere. But as soon as this vapor was projected into the exceedingly cold stratum of air above the surface of the lake it was condensed into minute globules of water, constituting the elements of the cloud. Paradoxical as it may appear, it is probable that more water was evaporated from the surface of the lake during this extremely cold weather than was given off on warmer days, when the air was more nearly saturated with vapor. In the case under consideration had the air been of the same temperature as that of the water, or even greater, and fully saturated with moisture, no evaporation would have taken place; the vapor already in the atmosphere would by its reaction prevented any more from being formed.

From Geo. C. Huntington, Kelley's island, Lake Erie, Ohio.

MAY, 1859.

There is one feature in our climate which is peculiar. Dew during the heat of summer is the exception, not the rule. This I know from observation continued through twenty years' residence here.

Another peculiarity is an exemption from frosts in autumn. We almost invariably see notices of frosts in the southern States before we experience anything of the kind here.

REMARKS.—The want of dew on Kelley's island, in lake Erie, may perhaps be accounted for by the great amount of vapor which must exist in the atmosphere over the great lakes, and which after the sun begins to decline is slightly condensed into a thin haziness, which prevents the radiation from the earth. The exemption from early frost is perhaps partly due to the same cause and partly to the equalizing effect of the large body of water on the temperature of the air which is contiguous to it.

From Conrad Mallinikroot, Augusta, Missouri.

JUNE, 1859.

My twenty years' observations at this place seem to prove that the changes of the wind follow the motion of the sun, viz: from east to south; from south to west; from west to north; from north to east, and so on.

If it once springs back in the other direction it is of short duration, and soon the main succession is restored.

REMARKS.—This has been shown by Dové to be the normal motion of the points of the weathercock in the northern latitude, and has been called the normal rotation of the wind. In places south of the equator the rotation takes place in an opposite direction.

The explanation of the phenomenon is not difficult when considered in relation to the different easterly velocity of different parts of the earth's surface as it revolves on its axis. A cannon ball projected directly toward the north describes on the surface of the earth a line curving towards the east, on account of the eastward motion which it received from the earth at the point of starting being greater than that possessed by the several points of the earth's surface which is passed over in the flight of the ball to northward. The longer the motion of the ball continues the greater will be the curvature. For a similar reason a ball shot directly south will describe a line curving to the west. Now, suppose a wind produced by rarefaction of the air, or by any other cause, to commence near an observer, and to blow directly from the south in a broad stream. If it continues to blow, say for several hours, the particles of air which compose the stream must start from points more and more remote, as would be the case with the particles of water in a long trough of which the north end is suddenly removed. Each particle of air in the stream must curve to the east, and the greater the distance it has travelled the greater must be its easterly motion. The particles of air which first strike the weathercock must turn the point of the arrow very nearly south, while those which impinge against it after the wind has blown for some time, having a greater easterly motion, must direct the arrow to the west of south, the westerly direction increasing as long as the wind continues to blow from more southern parallels of latitude. For a similar reason a wind commencing to blow from the north must turn the point of the arrow more and more to the east the longer it continues to flow southward. A similar result, but differing in degree, must be produced from whatever intermediate point the wind commences to blow, providing the current continues to flow in a broad stream for a few hours.

Hence, as a consequence of the rotation of the earth on its axis, the wind, at a given place in its normal flow, must cause the point of the arrow to move in the direction of the apparent motion of the sun.

When the arrow is observed to turn in an opposite direction we may be sure that this result is produced by a storm or an abnormal disturbance of the air travelling in this latitude eastward, and drawing, as it were, the air towards it, and thus changing the direction of the arrow.

From Francis L. Capen, Chicago, Illinois.

AUGUST, 1859.

I have stumbled upon the discovery of the meteorological system of the globe, and will furnish a programme of the weather in advance for Washington.

REMARKS.—The discovery of a single scientific fact may be the result of a happy accident, but a scientific principle can never be stumbled upon. It must be the result of reflection and investigation, and is generally arrived at by the successive efforts of a number of minds, each making a step in advance, while in general the popular credit is given to the one who finally presents it to the world in its simplest form. Even important scientific facts are at the present day rarely discovered by accident, and only, when the accident happens, to one intimately acquainted with the latest developments of science, and who is well qualified to recognize the value of the facts thus presented. We are, therefore, not disposed to place much reliance on the meteorological system which has been stumbled on by our correspondent.

From A. Fendler, St. Louis, Missouri.

OCTOBER 2, 1860.

I have reduced my half-hourly barometrical observations made at Colonia Tovar, in 1857, to the freezing point, in order to find out whether the mean maximum and minimum of the whole differs much in point of time from the maximum and minimum of the *not* reduced observations. The result shows that there is a slight difference. The time of maximum as well as that of minimum in the reduced observations is found to be somewhat earlier, having the maximum at 10 a. m., instead of 10.30 a. m., and the minimum between 4 and 4½ p. m., instead of 4½. These reduced observations I have also sent along with the register for September.

From Spencer L. Hillier.

SEPTEMBER 5, 1861.

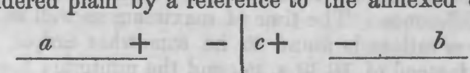
I am reminded of an incident that occurred in Red Wing; Minnesota, when I resided in that State, which may be of interest to you. A man was lathing in the second story of a new brick house, when a discharge of electricity took place, which, passing through the first and second floors, taking his body in its course, killed him instantly; and, strange as it may seem, it carried his cap up to the ceiling, there wedging it fast among the laths, where it remained till taken down the next day.

REMARKS.—This is by no means a very uncommon incident, since there are many cases on record of similar occurrences.

The late Dr. Carnahan, president of the college of New Jersey, a man of remarkable precision of observation and truthfulness of description, informed me of an accident by lightning which fell under his notice near Princeton. In this case, a negro man standing under a tree during a thunder-storm was struck with lightning and instantly killed; but the surprising part of the occurrence was the fact that his woolly hair was afterwards found adhering to the branches of the tree, several yards above his head.

There are also apparently well-authenticated cases in which an image of a body standing before a plastered wall has been transferred to the latter by a discharge of lightning passing from the one to the other.

These phenomena, though very surprising, are not inconsistent with well-established principles of electrical action. It has been fully shown by logical deduction from theoretical principles, and also abundantly proved by experiment, that the atoms of matter which exist along the path of an electrical discharge violently repel each other at the moment of the transit, the greatest intensity of the force being in the direction of the axis of the discharge. The effect may be illustrated by sending a discharge from an electrical battery through a narrow slip of tinfoil cut into parts, by passing the blade of a knife across it at intervals. The ends of each will be turned back on themselves, indicating that they have been subjected to a violent repulsive energy. It would appear that at the moment of an electrical discharge through a conductor, each atom becomes for an instant highly charged, and thus exhibits a repulsive force sufficient in some cases to separate the matter into its component parts. If a powerful charge be sent through a brass wire, the compound metal will be dissipated into powder of zinc and copper. But the transfer of matter in the line of the axis of the discharge is best shown by placing perpendicularly a plate of polished copper midway between two conductors, at about an inch and a half apart, the ends of the two which are turned towards the plate being covered, one with gold and the other with silver foil; after a powerful discharge from an electrical battery has been sent through the two conductors and the intervening plate, the gold foil on the one side will be found deposited on the plate, and the silver on the other. In this experiment the two metals at the same moment are carried in opposite directions, and, what is still more remarkable, upon minute inspection it will be found that particles of the copper plate have also been sent in opposite directions, and deposited on the opposite end of each conductor. The explanation may be rendered plain by a reference to the annexed diagram, in which



a and *b* represent the two conductors, and *c* the intervening copper plate, while the electrical condition of the several points of the system, at the instant the discharge is about to take place, is indicated by the signs + and —. From this diagram it will be evident that if the gold leaf be placed on the end of the conductor *a*, it will be repelled by this conductor, because both are + electrified, and will be attracted to the near side of the plate, this being — electrified, while a similar effect will be produced on the opposite side of the plate.

Although the repulsive and attractive action is the most intense in the line of the direction of the axis of the discharge, it takes place also in every direction. This is especially the case when a discharge is passed through air, and to this must be referred the surprising mechanical effects exhibited when a powerful discharge of lightning traverses a confined portion of air. In one instance, which came under my own observation, a stroke of lightning fell upon the chimney on the west end of a house; broke through the flue into the cock-loft or space immediately under the roof, and, passing through this a distance of about twenty feet, entered the flue of another chimney on the east end, in which was a stove-pipe, and descended to the earth. In its passage through the cock-loft, the particles of air near or in its path were rendered in an instant so mutually repulsive that they separated with such explosive energy as to lift up bodily the whole roof.

From Henry M. Bannister.

SMITHSONIAN INSTITUTION,
January, 1867.

In returning to New York from San Francisco in the present month, I observed a curious phenomenon in the formation of clouds over the Gulf Stream We found ourselves in the stream on the morning of the 19th, the temperature of the water being 70° Fahrenheit, that of the air at or below freezing point. The whole surface of the sea was covered with vapor, which was most dense at the level of the water, but did not much interrupt the view at the elevation of the vessel's deck. The vapor ascended in forms exactly like that of a water-spout, for which we at first mistook them. The long ropy column, swaying to and fro, was very distinct in many instances, several being seen at one time. When this was not developed we could still see a point stretching downward from the cloud above, and ragged masses of mist rising to it from below. We passed several of these vapor-spouts quite near enough to make sure of their real nature. They did not appear to be of any density, nor were there any signs of disturbance in the water or atmosphere about them. They were seen on all sides, several at a time. When we had crossed the Gulf Stream, and got into water of at most not more than 40°, we left the fog and had a clear sky over head, and on looking back the boundary of the stream was distinctly marked by the line of clouds hanging over it.

REMARKS.—The phenomenon of a cloud suspended over the Gulf Stream, marking its boundary, must have been frequently observed during winter, when there exists a great difference between the temperature of the water and that of the air, as in the case described by Mr. Bannister, while the interesting appearance of ascending columns, such as he has mentioned, must be of less frequent occurrence. It can probably happen only when the excess of the temperature of the water above that of the air is near a maximum, and this again can only occur after the air above the stream has been cooled by a northwesterly wind followed by, at least, a partial calm. In this condition the stratum of air immediately above the water would be heated, its specific gravity diminished, and the superincumbent mass brought into a state of unstable or tottering equilibrium. In the case of a small portion of air thus heated, the whole would rise in a single column on account of the pressure of the surrounding atmosphere; but in that of a large extent of heated surface, in which the temperature of the air above shades off gradually at the borders, the equilibrium will be restored by small ascending columns.

A similar phenomenon is sometimes observed on extended sandy plains in the form of ascending dust columns. They are interesting as serving to throw light upon the causes of the water-spout and the land tornado, which have generally been referred to electrical action, but which can be fully explained without the introduction of this principle other than as an effect and a modifying agent. In the case of the water-spout and the tornado, the unstable equilibrium may be produced by the gradual heating of the lower stratum, next to the water or ground, when saturated with vapor, by the rays of the sun, or perhaps in some cases by the passage of a heavier stratum over a lighter one next the earth. The intensity of the upward motion will generally be in proportion to the greater thickness of the heated stratum and the comparative density of the upper. In my meteorological essays published in the Patent Office Reports, I have endeavored to show that a thunder-storm, in accordance with the theory of Mr. Espy of the upward motion of the air in case of storms, is an upward moving column, in which the ascent is produced by the causes we have explained, and that the electrical effects exhibited are due to the disturbance of the equilibrium of the natural electricity of the ascending column of conducting vapor by the negative induction of the earth below and the positive influence of space above, or, in other words, that lightning is the result of conditions almost precisely analagous to those under which sparks are drawn from the lower end of a kite string under a clear sky.

From the same.

JANUARY, 1867.

During my residence at St. Michael's, Russian America, between Alaska and Behring's straits, while in the service of the Western Union Telegraph Company, I kept a meteorological register according to the instructions received from you. This is now in the possession of the company, but I understand that a copy is to be sent to the Smithsonian Institution.

The register extends over a period of eleven months, between October 1, 1865, and September 1, 1866. The barometer, thermometer, direction and force of the wind, and face of the sky, were noted three times a day, and the first two, on every seventh day during the winter months, hourly, betwen 7 a. m. and 9 p. m. I also noted casual phenomena, auroras, haloes, &c., and the times of the beginning and ending of storms of rain and snow. The amount of precipitation was not recorded.

The general movement of the air during the year appeared to be from a little east of north, though in midsummer the southerly winds were the most numerous and violent. In every instance, save one, during the winter, a high wind, no matter from what quarter, was followed by a rise in the thermometer. The one exception was a southerly wind from the direction of open water.

The maximum of the thermometer was $69\frac{1}{2}°$ Fahrenheit, in July, the minimum — 36° Fahrenheit in January. It probably descended still lower, but not at the hours of observation. The mean temperature for the three spring months was $28\frac{3}{4}°$ Fahrenheit; for the summer, $52\frac{3}{8}°$ Fahrenheit. That of the year was probably near 26° Fahrenheit—possibly a little lower.

We had one sharp thunder shower in July, the first that had occurred at St. Michael's for thirteen years, according to the statement of the residents, though thunder is not rare in the mountains of the interior.

The aurora was often observed, and was sometimes very brilliant, though not quite as frequent as I had expected. Ten was the greatest number observed in one month. One or two instances, I think, are worth mentioning here.

On the evening of August 23, after the corona had formed, streamers descended to all parts of the horizon; a cloud filled the whole southeastern portion of the heavens, but the streamers appeared even brighter against its dark surface than in other parts of the sky. It is my belief that they were between us and the cloud, which was quite dense. I observed a similar appearance in October, 1865, but I have not the record of it with me. Later in the evening the ordinary arch from northeast to northwest appeared.

An aurora, observed by Messrs. Pease and Ketchum, at Aloukuk, a place about forty miles inland from the head of Norton Sound, in November, 1865, was described as appearing against the side of a mountain a few miles distant, so that the top of the hill was seen above the display, and it was also accompanied with a hissing noise. The air was quite still, its temperature near — 30° Fahrenheit. While the display lasted it was light enough to read the finest print.

I have never myself heard these sounds with the aurora, though once, in the month of January, they were reported to me at St. Michael's by Russian residents, at a time when, on account of indisposition, I was unable to verify the statement by personal observation. I have no doubt, however, of their occasional occurrence.

In all cases the points of the compass are given according to the magnetic and not the true north. The variation in the vicinity of St. Michael's, according to Russian admiralty charts, is 30° 30′ east.

REMARKS.—Thunder storms are most frequent and violent in tropical regions; they seldom occur in high latitudes, particularly on the ocean. This is probably due to the coldness of the surface and the consequent small amount of vapor in the atmosphere, a thunder storm, being as we have said in the foregoing remarks on the Gulf Stream, a large ascending column of air rendered of less density than the stratum immediately above, by an excess of heat and moisture. Although the difference of density in this stratum may not be sufficient of itself to produce the upward motion at the level of the sea, yet the transfer of the same stratum up the side of a mountain would produce a precipitation of moisture, a liberation of latent heat, a vertical conducting column, and, in short, all the conditions necessary to a thunder storm. Hence the more frequent occurrence of this meteor in mountainous regions in the same latitude than on the shores of the ocean.

The statements of Mr. Bannister, as to the position and sound of the aurora, are in accordance with the testimony of some of the northern voyagers, who report having seen the beams projected on clouds and also against a hill at the distance of a few thousand yards. Indeed, it would appear, from an attentive consideration of all the facts relative to the aurora, that it consists of beams or discharges of electricity between the earth and the upper regions of the atmosphere, parallel to the dipping needle at the place of observation. These discharges, according to the theory, must, when a corona is formed, take place immediately around the observer, since in this case the corona is simply the vanishing perspective point of the parallel beams. We know that the appearance of the aurora at so small an altitude, and also the occasional emission of sound by the meteor, have been doubted, but we have never been able to infer from *a priori* considerations why the statement in regard to both of these phenomena may not be true. Late investigations leave no doubt as to the electrical nature of the aurora, and as the visibility of the discharges of this agent must depend upon the greater or less degree of the conductibility of the atmosphere, they may, in some cases, probably in dry weather, appear luminous down to the earth. The character of the sound is also similar to that which is produced by electricity.

In regard to the cold, it is a general observation that the thermometer sinks to the lowest degree during a perfect calm with clear sky. At this time the radiation from the earth is most intense, since all the local vapor is precipitated, and scarcely any obstruction is offered to the free passage of the rays of heat into celestial space. In this condition in a high latitude a wind which blows for some time, from any direction, is accompanied by a rise of temperature, since it must finally come, even across the pole, from a region of more moisture, though the water which it bears may be in a frozen condition, and thus produce a mistiness which would check radiation. Besides this, the wind from all points of more than one-half of the horizon, must come directly from a more southern quarter, and if it continues for a sufficient length of time, must needs have a higher temperature, from whatever direction it may come. Precisely at the poles all winds must be from the south.

The late Mr. Kennicott described to me a phenomenon which has also been mentioned by Sir George Simpson and other northern voyageurs; it consists of a rustling or crackling noise produced by the breath, when the temperature of the surrounding air is below -40°. He described it as producing a startling effect when first heard, the sound appearing to come from a point either to the right or left of the observer when in motion.

This effect is probably due to the sudden collapse of the vapor of the saturated air as it escapes from the lungs, and the consequent sudden reduction of the volume. That the condensation of the vapor may be sufficiently rapid in this case to produce a sound is scarcely surprising, when we consider the rapidity with which steam is condensed by water of the ordinary temperature in the operations of the steam engine.

HORARY VARIATIONS OF THE BAROMETER.

BY MARSHAL VAILLANT.

[Translated for the Smithsonian Institution, from the "Archives de la Commission Scientifique du Mexique," 1865.]

WHEN, in favorable weather, and especially under intertropical latitudes, we observe with attention the height at which the column of mercury stands in the barometer, it cannot long escape notice that this height undergoes, at different hours of the day, variations which pursue a quite regular course, and which are so well established that Humboldt (we believe) has said the elevation of the mercury in the barometric tube might serve as a timekeeper. This, in the domain of physics, is something analogous to what is presented to the botanist by the leaves or flowers of certain plants which open or close, droop or again become erect, at a particular period of the day, a fact which suggested to Linnæus the idea of his *horologium plantarum.*

The study of meteorological phenomena under the torrid zone presents a peculiar interest, and affords much more readily than in temperate zones a knowledge of the general laws which govern these phenomena. Between the tropics, in effect, they are disengaged from almost all the disturbing causes which, in our own climates, render their examination so difficult. It was under the equator that the horary variations were first recognized. Bouquer, La Condamine, and Godin observed them more than a century ago, (1736 and 1737,) when travelling together in South America. This phenomenon, to which is now given the more simple name of *diurnal period,* was described by Humboldt in the work which he published in 1807, and in which he has given the precise hours at which occur the maxima and minima of height of the mercurial column, as well as the extent of the oscillation. M. Boussingault has, by his own observations made at Guayra, (Colombia,) confirmed those for which science is indebted to Humboldt; and long calculations, embracing a large number of years, have resulted in mean quantities which place the existence of the diurnal period beyond doubt, even in latitudes as high as the north of France. "From facts ascertained by Bouvard, at the instance of Laplace, during the seventy-two months of the six years which elapsed from the 1st of January, 1817, to the 1st of January, 1823, it results that the mean diurnal variation at Paris is equal to $0^{mm}.801$; and also that the mean variation of each month is always positive."

I was not aware of the results at which Bouvard had arrived, when, availing myself of the tables furnished by the "*Annales de Physique et de Chimie,*" I verified, to my own satisfaction, that the oscillations of the mercury at Paris were so perceptible as to be wholly incontestable, especially during the mild season. The meteorological tables, comprising the year 1825, I discussed entirely, and those for 1824 in part. I shall give further on, and in detail, the results at which I arrived; for the moment, suffice it to say, that we are not to expect to realize, under the latitude of Paris, deviations like those which are manifested under the tropics, where their extent surpasses two millimetres.

The column of mercury attains the maximum of its elevation about nine or ten o'clock in the morning, from one of which hours to the other there exists, as it were, a solstice, or, in nautical phrase, a period of slackwater. After ten, the mercury begins to sink; at noon it is at a height which is very nearly the mean between the elevations of the day. From noon the column continues to sink, and reaches the minimum of height about three o'clock. From that hour till four of the afternoon we find a new interval of repose, the precursor of a change of sign in the movement of the mercury; and, finally, towards four or five o'clock, the column begins again to ascend.

About nine o'clock in the evening there is a second maximum in the height of the mercury, but the precise moment of this maximum is not yet well known. It would even seem that after an ascension, quite distinct and quite rapid, which takes place from five to nine or ten o'clock of the evening, the mercury does not absolutely cease to ascend, but that its ascensional movement, extremely slow and difficult to appreciate justly, still continues for an hour or two, or perhaps even three hours; after which succeeds a stationary interval of some length, and then a movement of subsidence, likewise very slow, and which may even continue during the first hour which follows the rising of the sun.

Such, in its principal circumstances, is the meteorological phenomenon to which has been given the name of *diurnal period.* Let us proceed to the explanation.

Let us suppose ourselves in the midst of a vast plain situated under the tropics. It is five o'clock in the morning, the air calm, the weather clear, the sun about to rise. It rises in effect, and its rays, at first tangents to the surface of the earth, glide over without warming it, and without communicating, so to speak, any heat to the air which is in contact with the ground. Presently, however, the luminous and calorific rays become inclined to the horizon, raise the temperature of the points upon which they strike, and consequently also the temperature of the air which touches the points warmed by the sun. These inferior strata of the atmosphere, from mere change of temperature, tend to dilate, to rise, and would in effect rise rapidly, were it not that in this ascensional progress they have to overcome the resistance which the superior strata oppose to their upward movement. It is necessary, therefore, that the air already heated should overcome the inertia of that which as yet is not heated—should displace and set it in motion. As long as the entire column, so to say, has not participated in this movement from below upward, the pressure is increased on the surface of the ground where the observer is supposed to be; it consequently increases also in the air which touches that surface, and hence upon the cistern of the barometer, which here fulfils not only the function of an instrument measuring the weight of the air, but also that of an instrument measuring the elasticity or resistance of a volume of air comprehended within certain limits of which the temperature continues to increase; in a word, our instrument is as well a manometer as barometer. And, let it be said in passing, it is because this double office of the instrument, which we owe to the genius of Pascal and Torricelli, has been too long overlooked that we have remained so much behindhand as regards the explanation of a great part of the phenomena of meteorology. But we return to the diurnal period.

In the struggle of which we have just now spoken, and which takes place between the atmospheric column and the part of the air which is in contact with the ground heated by the sun, now ascending more and more above the horizon, there is an instant when the resistance is a maximum, and when, consequently, the column of mercury attains its greatest height. Observation shows that this maximum occurs about nine o'clock in the morning.

There is nothing that need surprise us in this result of observation. On one hand, in warm countries, the greatest heat of the day prevails from ten o'clock in the morning or thereabouts; on the other hand, it is from eight to nine o'clock that the ascensional movement of the thermometer is most rapid. We have seen in Algeria, at the commencement of September, at the moment when we struck our tents, which were pitched in the plain of Abd el Nour, between Constantine and Sétif, the outside of these tents covered with crystals of ice, and the thermometer laid upon the canvas has descended by at least half a degree below zero. The sky was of the purest blue and the air of wonderful transparency. The sun arose in full radiance, and before eight o'clock the heat had become as oppressive as it was the evening previous on our arrival at the bivouac If I remember rightly the thermometer stood thus early at 27° centigrade, and it scarcely reached a higher point during the whole day.

From nine till ten o'clock of the morning, there is, as we have said, an equilibrium between the force which tends to communicate an ascensional movement to the column of air, and the resistance which that column opposes to the movement.

From ten o'clock onward, the ascensional force decidedly prevails; the inertia has been overcome, motion from below upward has been imparted to the whole column, and there results, as it were, a suction or partial vacuum over the surface of the earth, and consequently on the cistern of the barometer. The mercury sinks in the tube, not because there is less air above this cistern, but because that air ascending vertically has lost a portion of its gravity and of its elasticity.

The fall of the mercury, which commenced about ten o'clock, continues till three; at which hour, as observation proves, it is completed, and the barometer now indicates a minimum. There is nothing in this which does not correspond with what might easily have been foreseen. The heat begins or will soon begin to diminish; the solar wind or trade wind is about to lose its velocity—a velocity which in itself, also, has the effect of diminishing the pressure of the air on the barometer. Everything concurs therefore to produce the descending movement of the column of air, a movement which, it may be said, is rendered visible to all who observe what occurs with regard to smoke or the corpuscles suspended in the air, as soon as the sun begins to be inclined towards the horizon. Instead of mounting, as they do in the middle of the day, they remain on the surface of the ground. The perfume of flowers, instead of being dissipated in the atmosphere, is condensed and becomes stronger and more penetrating. Everything, in effect, manifests a movement of the air towards the earth; the clouds sink lower, the mists occupy the valleys, &c.

The surface of the earth becoming cooler towards four or five o'clock, a downward movement of the whole corresponding atmospheric column ensues; the air acquires a certain velocity, and this active force is represented to the sight by an ascension of the mercury in the barometer. From observation the metal is found to attain a new maximum of height about nine o'clock in the evening, twelve hours after the former maximum in the morning. But, as has been already said, there is some uncertainty about the precise instant of this second maximum, an uncertainty which may be explained by the slowness with which the temperature sinks during the night. The subsidence of the thermometer, from the time when the sun begins to descend towards the horizon to that of its setting, and even a little after, is quite sensible, but afterwards, although the earth's surface continues in reality to grow cooler, it is constantly brought into contact with successive volumes of the descending air which has not yet lost all the heat acquired during the day, and through the effect of this contact there is established, not indeed a complete equilibrium in the temperature, but, at least, a partial compensation for the effect of the radiation of the surface during the night.

The ascension of the mercury in the barometer, from four or five o'clock in the evening to about nine, would be certainly more rapid and extended if the condensation of the air in the strata next to the earth did not take place simultaneously with another very common phenomenon, which in hot countries acquires considerable proportions; I speak now of the evening vapors and the dew. The former begin to manifest themselves in intertropical regions as early as the fourth hour of the afternoon, and observant travellers even report that there are days on the Senegal when the phenomenon becomes perceptible within two hours after the sun's passage of the meridian. These vapors, which change their name and become dew in the morning, are so abundant under the tropics as to indicate a large conversion of watery vapor into liquid water. Now this water, when it was in a state of vapor, exerted on the barometrical cistern a pressure additional to the proper pressure of the air, and contributed, in a certain measure, to maintain the mercury at the height which it had attained; but if, afterwards, and when the air is about to descend because of its refrigeration by contact with the earth, the watery vapor is converted into cloud, (*serein*,) and no longer occupies more than a very small fraction of its primitive volume, this progressive diminution of the quantity of water which was in a state of vapor at the moment when the air began to assume a descending movement, cannot but operate as a lessening of the pressure which the descent of the column of air exerts on the barometer.

It would be difficult to estimate with any precision what part, as regards the ascension of the mercury in the morning, should be assigned to the evaporation of the dew, and equally so to estimate how much the maximum of the evening should fall below that of the morning from the effect of the production of the evening vapors. The difficulty is the greater because here we are to consider not only a weight to be added to the weight of the atmospheric column or to be deducted from it, but chiefly because when dew is in question, account is to be taken of the elasticity which the vapor of water acquires, as soon as it is formed, by its mixture with the air already warmed by contact with the surface of the earth. Nothing, however, forbids our making a supposition, and this I have done by inquiring how much a stratum of dew, represented by a height of two millimetres of water, (a supposition, I think, involving nothing excessive,) would cause the barometer to ascend, by the fact alone of its vaporization? The result has been found to be a height comprised between one and two-tenths of a millimetre. If we allow an effect of equal extent, but in an opposite direction, for the diminution of the mercurial column at the evening maximum of nine o'clock, there will result three-tenths of a millimetre, or very nearly so, as the quantity by which the maximum of the morning would surpass the maximum of nine o'clock in the evening; admitting, as has been just supposed, that the dew of the morning, like the damps of the evening, represents a stratum of liquid water two millimetres in height.

It was stated at the commencement of this article that, in the course of 1826, I took occasion to discuss the meteorological tables of the whole of the year 1825 and of a part of 1824, as they had been given in the *Annals of Physics and Chemistry*. I distributed the days first into categories indicated by the state of the sky, and next into categories determined by the direction of the wind which prevailed during these different days. Let us first observe to what result the former operation led:

	mm.
Eighty days of a sky entirely overcast. The mean height of the barometer, at the moment of the maximum of 9 o'clock in the morning, was	756.82
The mean height, at the moment of the maximum of the evening, was	756.20
Difference	0.62
One hundred and sixteen days of a cloudy sky. Maximum of 9 o'clock in the morning	758.28
Maximum of 9 o'clock in the evening	757.72
Difference	0.56
Forty-eight days of a sky perfectly clear, or presenting only small clouds, white and thin. Maximum of the morning	759.87
Maximum of the evening	758.53
Difference	1.34

From these differences it will be seen that, even for the overcast and cloudy days, which show the slightest disparities between the maxima, as might readily be foreseen from the explanation which we have given of the phenomenon of the diurnal period, these disparities still exceed by much the three-tenths of a millimetre to which we were led by our supposition of a stratum of two millimetres of water undergoing conversion into vapor, and conversely.

The number of days to which our calculations were extended was but 244; the rest of the year was left out of view, because the 121 days of which it consisted found no well-defined place in the categories we had adopted.

The researches which follow comprise the 365 days of the year, being distributed in the classification according to the direction of the wind:

Winds.			mm.
N. N.NW. N.NE.	75 days.	Mean height of the maximum of 9 o'clock of the morning	761.40
		Mean height of the maximum of 9 o'clock of the evening	761.36
		Difference	0.04

Wind.			
NE.	27 days.	Mean height at 9 in the morning	762.36
		Mean height at 9 in the evening	761.13
		Difference	1.23

Winds.			
E. E.NE. E.SE.	30 days.	Mean height at 9 in the morning	758.29
		Mean height at 9 in the evening	757.18
		Difference	1.11

Wind.			
SE.	12 days.	Mean height at 9 in the morning	753.59
		Mean height at 9 in the evening	752.48
		Difference	1.11

Winds.			
S. S.SE. S.SW.	61 days.	Mean height at 9 in the morning	755.41
		Mean height at 9 in the evening	753.27
		Difference	2.14

Wind.			
SW.	61 days.	Mean height at 9 in the morning	756.24
		Mean height at 9 in the evening	754.72
		Difference	1.52

Winds.			
W. W.SW W.NW	58 days.	Mean height at 9 in the morning	756.69
		Mean height at 9 in the evening	756.24
		Difference	0.45

Wind.			
NW.	41 days.	Mean height at 9 in the morning	759.10
		Mean height at 9 in the evening	759.02
		Difference	0.08

The preceding figures impart their instruction. We confine ourselves at present to the remark, already made in reference to the distribution of days according to the state of the sky, that the difference of the morning and evening maximum is always in the same direction, and that, if we except the two cases of winds whose course approximates to the north and northwest, the differences of the means generally much exceed the three-tenths of a millimetre yielded by the supposition of a stratum of water two millimetres in height. In observing with what regularity the phases of the phenomenon of the diurnal period present themselves even in the latitude of Paris, we can feel no surprise at the course, worthy of being styled chronometric, which the same phenomenon exhibits in tropical regions, and which led Humboldt to say that "no atmospheric circumstance—neither rain, nor fair weather, nor wind, nor tempest—affects the perfect regularity of these oscillations under the tropics; but that they subsist alike at all times and in all seasons."

Humboldt has given the law of these variations under the torrid zone; designating by Z the mean height of the mercury for the whole day, or, what is the same thing, the height at mid-day, we have:

The oscillation at 9 o'clock in the morning, equal to	Z + 1mm.1280
That of mid-day	Z
That of 4 o'clock in the evening	Z — 0 .9024
That of 11 o'clock in the evening	Z + 0 .2256
And, finally, that of 4 in the morning	Z — 0 .4512

For, as Humboldt tells us, he had observed that the ascensional movement. which is manifested from five o'clock in the evening, and which attains its apparent maximum towards nine, ten, or even eleven o'clock in the evening, ceases towards this last hour, and is replaced by a very slow fall of the mercury, a fall which is continued until about four o'clock in the morning, when it is at 0mm.4512 in relation to the mean height of the day.

The explanation which we have given of the diurnal period very readily accounts for this last phase of the phenomenon. There must, in effect, be a moment when the column of air, which is condensed on approaching the surface of the earth, has lost almost all its velocity of descent, and when, consequently it exerts on the barometric cistern no other action than that which results from its weight. There is no reason, on the other hand, why dew should cease from forming and from replacing the watery vapor which pressed upon the barometer, by liquid water, which exerts no pressure on that instrument. There may well, therefore, be a subsidence of the mercury, having its point of departure in the neighborhood of midnight, and continuing slowly until the moment when the morning sun begins to warm the surface of the earth and convert the dew into vapor.

In a very remarkable memoir, in which Ramond has treated of the question of the horary variations of the barometer, we find a table which gives the results of 2,065 observations made at Clermont, a table which we here present, still retaining the notation Z for the height of the barometer at noon, or the mean height of the day:

	Morning.	Noon.	Afternoon.	Evening.	Depression.	Ascension.
Spring....	Z + 0mm.39	Z	Z — 0mm.72	Z + 0mm.33	1mm.11	1mm.05
Summer...	Z + 0 .32	Z	Z — 0 .56	Z + 0 .33	0 .88	0 .89
Autumn...	Z + 0 .33	Z	Z — 0 .41	Z + 0 .47	0 .74	0 .88
Winter....	Z + 0 .37	Z	Z — 0 .36	Z + 0 .36	0 .73	0 .72

It would result from this table that, in our climates, the depression of the day is reduced to the half of that which is observed at the equator. Another consequence to be drawn from the same table would be this: that, with us, the ascension of the evening is very nearly equal to the depression which preceded it, while under the tropics these quantities differ from equal to double.

But here it is proper that an important remark should be made: the above table, as well as those which precede it, is the result of observations made in France *at the same hours of the day*, but at periods of the year wholly different.

It is readily understood that, under the tropics, where the duration of the days varies so little, the barometrical heights should be taken at moments always equally distant from mid-day, and therefore always equally distant from the rising and setting of the sun, or at least very nearly so. Such are the observations made by Humboldt, M. Boussingault, &c. But, in France, how are we authorized to collate and compare the effect of the warming of the earth's surface by the solar rays, *in summer*, at nine o'clock in the morning, when the sun is already five hours high, with that which has been produced at the same hour of the morning, *in winter*, when the sun is scarcely above the horizon and its rays reach the surface of the earth only through thick clouds or mists? There is evidently no comparison possible between observations made under conditions so different. It would seem that if the maximum of the morning corresponds, under the torrid zone, to eight or nine o'clock, as Humboldt has verified, this maximum will, with us, occur in summer, when our days are long, at about six o'clock. Further north it would be at five o'clock, while near the pole there would be no maximum either morning or evening. In the winter, on the contrary, the maximum would, in our latitude, occur at about ten or eleven in the morning and be necessarily not very obvious. In high latitudes, where the sun scarcely shows itself, all would be confounded, and the diurnal variation, if not wholly absent, escape the notice of observers by reason of its diminutive proportions. Continuous observations, directed to a verification of the suggestions thus thrown out, could not fail to be of much interest, especially if it were practicable to make them with a registering instrument and a barometer filled with a liquid much lighter than mercury.

The inquiry has sometimes occurred to me whether there might not be found in the experience of our every-day life some simple facts which would serve to illustrate and confirm the above theory on the diurnal period, and it has seemed to me that what takes place in our ordinary chimneys has much analogy with the oscillations of the barometric column. Let us suppose the fire to have been extinct for some time, and the air of the chamber, like that of the chimney, nearly motionless. Let fire be now supplied on the hearth; the lower air will become heated and have a tendency to rise, but it is necessary that it should make for itself a way in the column which occupies the body of the chimney, that, so to say, it should displace this whole column and set it in motion. The struggle is of more or less duration; sometimes the warm air, not being able to rise quickly enough, diffuses itself in the chamber; the chimney *smokes*. As long as the struggle continues there is a repression of the air, and consequently an augmentation of pressure in the body of the chimney. Here we have the first phase of the diurnal period; it corresponds to the warming of the surface of the earth by the newly risen sun. By and by the moment arrives when the pressure of the air in the body of the chimney attains its greatest intensity, corresponding to the maximum of eight or nine o'clock in the morning. After this, all the air being finally put in motion and the force of inertia overcome, the tension diminishes, and through the velocity acquired, this tension in the tunnel of the chimney must become even inferior to what it was before the kindling of the fire on the hearth, representing the period from nine or ten o'clock to midday and to even three o'clock in the afternoon.

Suppose now that after having kindled a brisk fire and established a strong draught, we cease to supply fuel; the draught will diminish, the air will ascend less rapidly, the pressure will augment in the body of the chimney, and if there were a barometer therein, the mercury would ascend in the tube. At length, when the fireplace has become altogether cooled, the entire column of air will acquire a movement from above downward, and will exert a pressure on the hearth; it is what often occurs in our dwellings during summer, when great heat prevails. The external air, which is warmer than that of the chamber, becomes cooled in penetrating from above, descends along the tunnel and throws down the chimney-screen if not well secured in its position. The odor of soot pervades the apartment, distinctly indicating the presence of a descending current of air in the flue of the chimney. Now, it is this descending movement of the air in the chimney which represents, in the diurnal period of the barometer, the phase commencing at three or four o'clock in the afternoon and continuing until after midnight.

Is it not a curious thing to find, in what occurs at the fireside of the most simple dwelling, so complete an analogy with all the circumstances of the phenomenon, apparently so complicated, which is called the *diurnal period* of the barometer? Need we hesitate once more to repeat that it is the presence or absence of the sun, the warming or cooling of the earth which thence results, which is the sole cause of all meteorological phenomena, from the ascent and passage of a soap-bubble to the most fearful tempests? Let us carefully observe, then, these effects of heat and cold, not by studying the phases of the moon and the tides which it may produce at the limits of our atmosphere, not by considering the stars, but by attending to what passes around us on the surface of the earth.

Biot has said, and M. Poey has recently repeated the remark, that it is necessary *to commence our study of meteorology from above;* we are not at all of that opinion. Almost all the phenomena of meteorology—rains, snow, hail—are accidents solely due to differences of temperature; they are produced at the surface of the earth, and it is they which occasion the disorderly movements of the lower strata of the atmosphere; a little above these there is only a blue or black sky, absolutely a stranger, let us believe, to all those phenomena which occupy so large a space in our own experience.

LETTER OF MARSHAL VAILLANT, ON THE FOREGOING THEORY, TO M. CHARLES SAINTE-CLAIRE DEVILLE, MEMBER OF THE INSTITUTE.*

I have attentively read your memoir, or, as you term it, monograph, *On the Barometric Phenomena of the Antilles and Neighboring Countries.* The following are some of the reflections which that examination has suggested to me.

The observations which you report confirm the truth of the theory which I have developed in my note on the horary variations, and I have no doubt that observations which will be made in the future, and for which, I trust, that theory will be accepted as a guide, will lead to the most satisfactory confirmation.

You are right in saying that *the different elements of the total oscillation undergo the constant influence of the solar heat.* Yes, it is to the influence of that heat that the ascensional movement of the mercury in the barometer is due, in consequence of the inertia which the air and watery vapor mingled together oppose to the ascension which the heat communicated tends to impress upon them; and it is also to the diminution of the calorific effect on the air and watery vapor, and to the inertia which retards their being put in motion, that is due the subsidence of the mercury in the barometric tube. Inertia, that force which is ever and everywhere encountered, has been, perhaps, rather too much neglected by the meteorologists.

The cause being known, or at least suspected, we can calculate its effects and proceed in advance of direct observation; this will come afterwards to the support of the theory. Thus, as you very properly say, *the phenomenon of the diurnal variation, at the level of the sea, considered in the establishment of the tropical hours or in the extent of the oscillation, is manifestly connected with the*

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* This letter forms a sequel to the previous article, *On the Horary Variations of the Barometer*, and was designed to furnish to the explorers and correspondents of the scientific commission of Mexico indications respecting the direction to be given to their barometrical observations.

causes which influence, as well the distribution of the annual mean temperatures as the distribution of the sum of the temperatures over the different hours of the day.

Let us go a step further, if you please, and state more precisely the effects due to this cause, inasmuch as we know it: the greater the heat, (the habitual heat due to the latitude,) the greater should be the extent of the oscillations; the warmer the season in comparison with other seasons of the same year, the greater will be the amplitude of the oscillations during that warm season; the more rapid the ascension of the thermometer during the day, as, for example, in certain countries of low latitude, where, in a few hours, there is a transition from a temperature below zero to a heat of twenty-five or thirty degrees above, the more considerable will be the oscillation; the clearer the sky and more transparent the air, the more rapidly will the earth be disposed to grow warm under the action of the solar rays, and the greater and more rapid will be the oscillation.

For the part of a continent having its general slope directed toward the rising sun, the ascent of the mercury in the morning would be more rapid, and it would attain a limit which it would not reach if that part of the continent were simply level or nearly so. For the same tract of country, thus strongly inclined to the east, the fall of the mercury in the afternoon would be also much greater than for a horizontal region, since what would be gained from the inclination of the surface in the morning would be lost in the evening.

All these effects are influenced also by the alternation of the breezes of the land or water, if the position is near the shore of an ocean, great lake, &c. And since these breezes, according as they arrive from the east or west, counteract or favor the calorific action of the sun on the surface, they would differently modify the oscillations of the mercury in the barometer. This your *monograph* conclusively shows. Nor would the direction of the trade-winds, on their part, fail to exert an influence on the amplitude of the phenomenon; not so much indeed as altogether to mask it, but sufficiently to extenuate certain details of time and extent, which theory alone could then supply and place in relief. While the theory here presented seems to account satisfactorily for the horary variations in an extensive and level plain, it would lead us to expect that, since the degree of calefaction through the presence of the sun is the cause of these variations, the latter will be proportionately less as the former also is less and the plain more elevated above the level of the sea; in other words, the greater the altitude of the country the smaller will be the oscillation of the mercury. This prevision of the theory is confirmed by the observations reported in the ninth page of your memoir.

The horary oscillation for a small island lost in the bosom of a vast ocean must be difficult to recognize and measure, the effects of the heat communicated to the soil being, as it were, absorbed and disappearing in the general movement of the air on that ocean. An analogous result, though even less distinct, would be produced in the case of a peak isolated in the midst of a great plain.

In recapitulation, I would say: if the phenomenon of the horary variations has its cause, as I am satisfied is the case, in the inertia of the air, that is to say, in the reluctance with which it enters into motion, whether upward or downward, when the surface on which it rests begins to grow warm or cold, observation ought to lead to the verification of the following facts:

The warmer a country in consequence of its latitude, the more decided will be the oscillations of the barometer, and the more feeble in proportion to the elevation of the country above the sea Their regularity, that is, their symmetry in relation to the meridian, will be modified through the aspect of the country as turned towards the east or west, by the manner in which it presents itself to the general current of the trade-winds, the breezes of the land or sea, &c.

Beyond the intertropical zone, the hours to be chosen for the observations should vary according to the seasons. The extreme hours should be more remote from midday in spring and summer, and should be nearer to it in autumn, but especially in winter.

The amplitude of the diurnal oscillations of the barometer will diminish in proportion as we advance towards the higher latitudes; it will very nearly vanish in countries where the sun no longer appears above the horizon, as in those also on which it constantly shines during six consecutive months.

The amplitude will diminish when the sky is overcast or cloudy, but the oscillations would not on that account cease altogether; for if fogs or clouds intercept the view of the sun from the earth, they still receive the rays of that luminary on their upper surface, which must consequently have its temperature raised, as would be that also of the air which touches it, though in a less degree than is the case when the rays of the sun strike the earth itself.

Beyond the zone for which the length and heat of the days are, so to speak, invariable, it should be in the warmest month of the year that the diurnal barometric oscillation is most considerable: hence we infer that there is a maximum for the year. But there is difficulty in admitting a monthly maximum or minimum. The existence of the latter, if observation should verify it in certain localities, must depend on special circumstances, from which it will be easy, we think, to disengage the phenomenon itself. As this phenomenon, however, according to what we have so often said, is in an especial manner connected with the temperature, there would be no positive ground for surprise if the months which comprise the spring and summer were found to have each a minimum at the commencement and a maximum at the end; while, for the months constituting the autumn and winter, there should be a maximum in the first days and a minimum in the last days of each of those months. But, in our high latitudes, and with all the causes which occur to derange the regularity of the course of our seasons and their temperatures, it will, perhaps, be very difficult to establish by direct observations what I am disposed to call the differentials of the phenomenon of barometric diurnal oscillations.

Permit me, in conclusion, to advert to a great variance which exists between us. In several passages of your work you speak of *variations in the weight of the atmosphere*, and you seem inclined to connect these variations of weight with the barometric diurnal oscillations; I might cite specially the last paragraph of page 32, in which you say: "We have inquired how it is that, in the same place, the weight of the atmosphere varies in the day according to the hours, and in the year according to the months." For a long time I have been opposed to this mode of interpreting the horary variations of the barometer; these, in my opinion, are wholly independent of the proper weight of the atmosphere, so that, on the supposition of the weight of the column of air remaining absolutely the same for several months, the diurnal variations would not the less be produced and within the same limits. They are the effect of the augmentation or diminution of the volume and elasticity of the air, in consequence of the heating or cooling of that air in contact with the surface of the earth; but this has nothing in common with the weight of the air simply as a gravitating body. I give but little credence to waves produced at the upper limit of our atmosphere; but had these waves several kilometres of altitude the phenomenon of horary variations would not be materially affected; they might slightly mask it, perhaps, but would neither deprive it of its form or its importance.

It is this unfortunate confusion of the weight and the action or elasticity of the air, as also of the weight and elasticity of the watery vapor diffused in the atmosphere, which renders the language of the barometer so critical a matter to interpret. This instrument, as regards the indications which it gives us and the consequences which we draw from them, is much more a *manometer* than a *baroscope*, as, at its origin, it was sometimes called. I hope to be able to recur to this subject hereafter, and in the mean time conclude by saying that, in the computation of heights by the barometer, there are circumstances in which it will be necessary to take account of the horary variations, at the risk of neglecting an element of exactness.

APPENDIX TO THE FOREGOING.

BY PROFESSOR HENRY.

Without wishing in the least degree to detract from the merits of the foregoing interesting exposition of a phenomenon which has long occupied the attention of meteorologists, I think it due to the memory of the late Professor Espy to state that the essential principle of the explanation was given by him in the first volume of the Journal of the Franklin Institute, of Philadelphia, in 1828. M. Vaillant has, however, without knowing of the previous explanation of Professor Espy, given a more full exposition of the phenomenon by the introduction of the effect of the variation in the quantity of vapor in the atmosphere. The following is an extract from the article published by Mr. Espy:

"The true cause of this phenomenon is the expansion, and, of course, rise of the atmosphere by heat, and the contraction and consequent fall of the atmosphere by cold. Suppose just before sunrise in the torrid zone, the temperature of the air neither increasing nor diminishing, and of course the air neither expanding nor contracting, the barometer stands at thirty inches; now when the sun rises at six o'clock the air will begin to be heated near the surface of the earth, and of course, by its expanding, will elevate the superincumbent atmosphere; and this by its inertia will react on the air below, and thus press harder on the mercury of the barometer than if it were at rest, and the more rapidly it is forced upwards the greater will be its reaction downwards, and of course the more will the barometer be affected. It is manifest that the most rapid increase of heat and rarifaction of the air will take place somewhere betwen sunrise and three o'clock, when the heat is the greatest, and this will evidently be near ten o'clock, at which time the barometer will stand highest. Though the heat is still increasing, and of course the air expanding, yet the rapidity of increase after this hour is not so great, and therefore the barometer will begin to fall; and at the moment of greatest heat, when the air is neither expanding nor contracting, the mercury will again stand at thirty inches high. But now the air begins to contract from cold, and the mercury will therefore continue to descend, and the rapidity of the descent will be in proportion to the rapidity of the contraction from cold. Perhaps this effect may be more clearly understood by imagining an extreme case. Suppose the lower strata of atmosphere suddenly annihilated, the mercury of the barometer would be relieved from all pressure for a moment, and fall down into the basin; and if annihilation removes all pressure from the mercury, a contraction of the lower strata by cold will remove some pressure; therefore the mercury will fall; at the moment, therefore, of a most rapid decrease of heat, which is probably near sunset, the mercury will stand lowest, and will be below the height of thirty inches, at which I have supposed it to stand when the air is neither expanding nor contracting. The mercury will now begin to rise, for the rapidity of contraction diminishes from this moment, and the upper parts of the air are permitted, more and more, to press upon the lower with their whole weight; and even when the contraction ceases below, the upper parts, having acquired a velocity downwards, are inclined to continue that motion, and thus by their momentum will press upon the lower parts with a force greater than their natural gravity, and thus the barometer will rise above thirty inches, at which height it was supposed to stand when the air was neither contracting nor expanding. This effect must take place some time in the night, and it seems probable, a priori, that it would be about ten or twelve o'clock. Now, as the mercury at this hour stands higher than it does by the natural weight of the air at rest, it is plain that it will begin to fall as soon as the force of the superior parts of the air begins to spend itself on the inferior, and when all motion downwards has ceased, the mercury will again be pressed by the natural weight of the atmosphere, and so stand at thirty inches. In this situation the sun will rise upon it, and the same fluctuations will be renewed.

"J. P. ESPY.

"PHILADELPHIA, *March* 1, 1827."

ON THE FORMATION OF ICE AT THE BOTTOM OF THE WATER.

BY M. ENGELHARDT, DIRECTOR OF THE FORGES OF NIEDERBRONN, (LOWER RHINE.)

Translated for the Smithsonian Institution from the "Annales de Chimie et de Physique," Paris, 1866.

THERE are not a few natural phenomena which are better known to the people than to the learned; and the reason of this is simple. The man of the people, always in quest of the means of bettering his condition, observes a phenomenon not with a view to investigate its cause, but to derive from it some practical benefit. The savant, on the contrary, withdrawn from nature in the meditations of the closet, usually recurs to practical experience only when he is led to do so by the speculations of theory, and often refuses to recognize the facts which do not square with his preconceived ideas.

Hence it has happened that sundry physicists, Nollet, Mairan, and others who have followed them, have denied a phenomenon well known to all the watermen and frequenters of the great rivers of the centre and north of Europe: I mean the ice which, in severe frosts, forms at the bottom of rivers, and which the Germans call *grundeis*, ice of the bottom.

The Rhine and the Danube, rivers with a rapid current, do not freeze like the Seine by being covered with a plane and uniform stratum; they bear along large blocks of ice which cross and impinge upon one another, and becoming thus heaped together, finally barricade the river. It is a grand spectacle, when the Rhine is thus charged, to see these countless drifts adjust themselves in their relative position, where they unite by congelation, and convey the idea of the fall of some mountain which has covered the plain with rocks of every dimension. But it is not this accumulation of ice drifts in the Rhine which is of itself the cause of danger; it is, on the contrary, the debâcle or breaking up which is often productive of calamitous consequences. When this debâcle commences in the upper part of the river, above the point where the latter is completely frozen, the masses of ice, drifting with the current and unable to pass, are hurled upon those already soldered together; thus an enormous barrier is formed which the water, arrested in its course, cannot pass over, and hence overflows to the right and left, breaking the dikes, inundating the plains, and spreading devastation and suffering far and near.

The disasters caused by the debâcles of the Rhine have taught the riparian inhabitants to observe attentively the facts which may serve them as a prognostic, and put them on their guard against the irruption of the ice. It is thus that they have been led to observe the *grundeis*—that is to say, the ice formed at the bottom of rivers, for it is this ice which, in becoming detached from the bottom and rising towards the surface, unites itself to the under surface of the masses already in place, and by further embarrassing the discharge, exposes the country to inundation.

Being intrusted with the direction of important works, I felt it the more incumbent on me to observe with care the daily phenomena which might have an influence on our labors, and it is from having followed them with great attention that I have found, as I believe, the true cause of the presence of ice at the bottom of rivers.

Let us first consider the phenomenon historically as we find it related by

Horner in the *Dictionnaire de Physique* of Gehlen, 111, 127; by Arago, (*Annuaire du Bureau des Longitudes pour* 1833,) and by L. F. Kœmtz, (*Dictionnaire Encyclopédique des Sciences et des Arts,* published by Ersch and Gruber.)

The physicist Plot appears to have been the first to make mention of the ice formed at the bottom of rivers, in his "Natural History of Oxfordshire," 1705. Hales, in his "Vegetable Statistics," London, 1731, mentions the existence of ice at the bottom of water as having been ascertained by bargemen, and reports his own observations. He had seen ice of spongy consistence form at the bottom of the river, and remarked that "the water must be in motion in order to have sunk to zero throughout its mass," and he pointed out the influence of asperities and projecting bodies on the formation of this ice.

Nollet, in 1743, thought the phenomenon incompatible with theory, and completely denied it. Mairan and several other physicists of the epoch shared his opinion.

Nevertheless, Desmarest, in the Memoirs of the Academy for 1776 and the *Journal de Physique* for 1783, t. iii, verified anew the formation of ice at the bottom of rivers; it was afterwards confirmed by Braun, *Hanvôrisches Magazin,* 1783, and *Journal de Physique,* 1788; also by Leslie and by Garnet. In 1806 observations were made by Knight on the Teine. Stencke, chief of the pilots of Pillau, (Prussia, on the Baltic,) reports in Gilbert's *Annales de Physique,* xx, 332, that, on the 9th of February, 1806, at a temperature of — 32½ degrees of Fahrenheit = — 35.11 degrees centigrade, chains 12 feet in length surrounded with ice were brought to the surface of the water from a depth of 12 to 15 feet, as well as a large cable 30 toises long, which had been immersed for a considerable time.

On the 11th of February, 1816, the abbé Branthoma, then professor of chemistry in the Faculty of Sciences of Strasbourg, again verified the formation of ice at the bottom of the bed of the Rhine, in presence of the engineers of bridges and roads. The temperature of the air was at 12°; the Rhine, at the place where the ice formed, was about 2 metres in depth. The ice was seen to form very perceptibly at the bottom of the water, not only at this point, but at many others. The thermometer was at zero at the surface of the water, while another thermometer introduced into the ice was equally at zero. When taken from the water the ice had the same temperature, was very porous, and formed of interlacing crystals. Towards 10 o'clock the ice, having become more compact, was detached from the bottom, and rising floated on the surface. (*Bibliotheque Universelle,* xvii, p. 304.)

In 1827, Merian observed, at Bâle, ice at the bottom of the water in the canal of Saint Alban. In the same year Hugi made a great number of observations on the spongy ice formed at the bottom of the Aar, near the bridge of Soleure. The very cold winter of 1829 led to the first observations which I made at Zinsweiler, (Lower Rhine,) and of which I gave notice, the same winter, to the Society of Natural History of Strasbourg, then recently instituted.*

M. Fargeaud, professor of physics, published, in 1830, his thesis on caloric, in which he communicated his observations on ice thus formed, in the following terms: "On Sunday, January 25, 1830, I happened to be present with several of my pupils on the bank of the Rhine, opposite to Kehl. A thermometer, suspended to a tree, indicated 11° Reaumur; while another, placed in the snow at an inch from the soil, stood at nearly 6° Reaumur. Notwithstanding this great degree of cold, the water which flowed in the moat of the citadel was frozen only on its borders. That part of the bed of the Rhine which by an embankment of sand was sheltered from the wind, was also destitute of ice. At the same time the thermometer resting on the surface of the water rose rapidly to

* This society was founded in December, 1828, by certain members of the Society of Sciences, Agriculture and Arts of the Lower Rhine, viz., MM. Bœckel, Duvernoy, Ehrmann, Engelhardt, Fargeaud, Lauth, Nestler, Silbermann, and Voltz.

zero, and when plunged one or two feet deeper ascended to 3½ above zero. Proceeding to the end of the embankment, at a short distance from a very rapid current, and in a sort of inlet where the water had at first but little depth, I saw all the pebbles covered with a kind of transparent moss from an inch to an inch and a half in thickness, composed of crystals of ice combined in every manner; a true crystallization in effect, promoted by the presence of the pebbles, and in all respects similar to that which is furnished by certain salts in our laboratories. The thermometer indicated zero at this point, as well towards the border as at a depth of several feet, in the most rapid part of the current. We were presently able to distinguish in this latter part, at a depth of about five feet, large masses of this spongy ice into which an oar could be thrust with the greatest facility, and portions which were thus detached were found to be exactly similar to innumerable fragments of ice which were then drifting in the river."

Hence M. Fargeaud concludes: "1st, that in rivers having a greater or less depth and no current the water will long maintain a temperature above zero, whether from the warmth of the ground or the small conductive capacity of the water, or, in fine, from the very fact of the species of equilibrium which results from the maximum of density. When such a river freezes, the ice must of necessity form at the surface; 2dly, that if, on the contrary, the current be very rapid, the whole mass must be brought to zero; 3dly, that the water thus brought to zero may preserve for some time its liquid state, especially on a bottom of ooze or sand, but that in general it will be disposed to crystallize, and will in fact crystallize wherever any cause shall contribute to produce this change of condition."

M. Fargeaud then adds, in allusion to the facts which I had stated in his presence in the Strasbonrg Society of Natural History, "that he had learned that a superintendent of the forges of the Vosges, in order to prevent the ice from forming at the bottom of the stream which supplies his works, had caused the stones and other extraneous bodies found therein to be removed every year." My experiments were afterwards cited by Arago and other physicists with simple reference to a "master of the forges of the Vosges."

Berzelius, in his *Compte Rendu* for 1829, also speaks of the ice at the bottom of rivers; he had himself seen the phenomenon but once. He reports the observations of Hugi and a letter of M. Reaucourt, published in the *Globe* of the 17th February, 1830.

Arago, in the *Annuaire du Bureau des Longitudes* for 1833, relates all that was then known respecting ice at the bottom of the water, and gives the following explanation of it: "Who does not know that in order to hasten the formation of crystals in a saline solution, it is sufficient to introduce into it a body either pointed or of uneven surface; that it is especially around the asperities of bodies that crystals take their origin, and receive rapid accretions? The same, we may be assured, is the case with the crystals of ice.

"But what we have just said," he continues, "is not precisely the process in the congelation of rivers; this will scarcely be suspected, I think, if we remember that the congelation never occurs on the bed itself except where rocks, pebbles, pieces of wood, plants, &c., exist. Another circumstance which seems capable also of playing a certain part in this phenomenon is the movement of the water. At the surface this movement is very rapid and sudden; it must, therefore, tend to obstruct the symmetrical grouping of the needles in that polarized arrangement, without which crystals of whatever nature acquire neither regularity of form nor solidity; it must often break the crystallized nucleus, even in its rudimentary state.

"Movement, that great obstacle to crystallization, if it exist at the bottom as at the surface of the water, is there at least greatly diminished. It may be supposed, therefore, that its action will only conflict with the formation of a regular or compact ice, but will not prevent in the end a multitude of small filaments from uniting with one another confusedly, in such a manner as to generate that kind of spongy ice through which M. Hugi so readily thrust the oars of his bateau.

"Having reached this point, the reader will, perhaps, ask why I do not offer the preceding as the complete explanation of the formation of the *grundeis* of the Germans, the bottom ice of our watermen. The following is my answer:

"We are still in want of observations to prove that nowhere does this sort of ice show itself until the whole mass of the liquid has descended to zero. It is not certain that the small needles of ice floating on the liquid, of which M. Knight makes mention, and which may have acquired, at least at their surface, a temperature much below zero, do not play an important part in the phenomenon, and one which I have wholly neglected; that, for instance, of imparting coldness to the pebbles which cover the bed of the river when the current carries them down to it. Might it not be possible even that these floating filaments were the principal elements of the future spongy ice?"

In 1836 Gay Lussac (*Annales de Chimie et de Physique*, t. lxiii) recurs to this theory of the ice at the bottom of the water. He regards with doubt the first explanation of Arago, and adheres rather to the hypothesis of the floating filaments of which Arago speaks in the last place: "The spongy ice," he says, "which is observed on the bed of certain rivers of rapid current has its origin in the small and innumerable flakes of ice which drift on top of such rivers in very cold weather, and whose surface in contact with the air is somewhat below zero. The submersion of these flakes takes place by means of the current which sweeps them along in its movements. Their adhesion, whether to the extraneous bodies which cover the bed of the river or to one another, is occasioned by the congelation of the film of water at the points of contact, owing to the greater coldness of the flakes, while their permanence at the bottom of the water, without other enlargement than through accumulation of numbers, is explained by the constancy of the temperature at zero, which itself could not exist except by the effect of a current sufficiently rapid."

In 1847 Dr. Plieninger, of Stuttgard, published in the annual report of the Wurtemberg Society of Natural Sciences a note, accompanied by an account of the experiments made on the Danube by M. Lenke, of Ulm. Three troughs of deal plank were immersed in that river, one of which was made of smoothly planed boards, the second of rough unplaned boards, while the third had been rendered still more uneven by hewing. It was found that in the first no ice was formed; in the second there was a deposition of minute crystals; and in the third, considerable groups and accumulations of ice. The results of these experiments seem to me unfavorable to the views taken by Gay Lussac; their explanation will be hereafter adverted to.

The following are the experiments which I made in 1829, and which I have since, at different times, repeated. For this purpose I employed three kettles used for smelting, of about one metre in diameter, which I filled with water. In order to judge of the influence of extraneous bodies, I placed at the bottom of one of these kettles bits of wood and cast iron; in another a little water had been allowed to congeal; in the third there was nothing, These extraneous bodies exerted no sensible influence on the congelation. At the moment of commencing the experiment the circumambient air was at — 2 degrees; it became colder during the night; the water was at 0°, and was immediately covered with films of ice which crossed one another at 30, 60, and 120 degrees, forming very soon an entire crust of ice over the surface. The following day I broke this crust, which was from 35 to 40 millimetres in thickness; I decanted the water from the kettles, and found the bottom and sides lined with a continuous stratum of ice having a thickness of 20 to 25 millimetres. The surface was smooth, offering but here and there slight rugosities; to these rugosities tufts of crystals of ice were found to have attached themselves.

In January, 1860, I made new experiments. I took three smelting kettles of 550 and 670 millimetres diameter, and a wooden tub of 640 millimetres, and filled them with water from the river, which was at + 2 degrees; the temperature of the atmosphere was — 2 degrees in the daytime; it sunk to — 5 degrees in the night. The vessels were placed on supports 200 millimetres in height, with a view to having them surrounded with an equal temperature on all sides. The next day, the four vessels were found to be covered with a layer of ice 120 to 140 millimetres thick. On the sides the kettles were covered with a layer of ice of 20 millimetres, and from 15 to 20 on the bottom; this stratum of ice was smooth, and without rugosities. The wooden tub had a stratum of only some 3 millimetres on the sides, and a few tufts of crystals. On the bottom appeared some isolated films of ice 100 to 110 millimetres in length, 5 to 6 millimetres in breadth, and 1 to 2 millimetres in thickness, furnished on the edges with small projecting points, planted vertically on the larger film, like the teeth of a saw; these teeth or lateral crystals were 5 to 6 millimetres long, with a breadth of 1 to 2 millimetres.

These experiments, repeated on different occasions, at a temperature of from — 6 to — 7 degrees, always yielded the same result. *After being covered with a stratum of ice on the surface, the vessels were also lined with a stratum of ice on the sides and at the bottom, as might have been foreseen; this ice on the walls of the vessel is of different thickness, according to the conductibility and radiation of the walls.* Thus the wooden tub had a thinner layer of ice on its sides than the smelting kettles, and ordinarily only some needles of ice on its bottom.

I observed that in the kettles these layers of ice were proportionably thicker as the external cold was more intense; that they were always somewhat thicker on the side-walls than on the bottom, and that when once formed these layers, themselves a bad conductor of caloric, operated as isolating walls, and scarcely received any augmentation in point of thickness towards the interior. I have also observed that the surface of the walls, as well as the interior surface of the stratum of ice which covered the liquid, was sometimes striated by minute lines, crossing one another at angles of 30, 60, and 120 degrees. Small pools of water of little depth commonly freeze in films which cross one another under the same angles.

In order to observe well the formation of ice *at the bottom of water* I have taken smelting vessels, which I have placed in a refrigerating mixture of snow and marine salt yielding a cold of — 16 degrees. The circumambient air was at + 13 and + 15 degrees; the water at 0°. Naturally, under these circumstances, ice was not produced on the surface of the water, but was produced at the bottom of the vessels. The congelation, however, was not always the same; at one time needles would be formed, which might be seen to increase sensibly until the ascensional force produced by their slight specific gravity had overcome the feeble adhesion of their minute base, when they became detached, and rising, floated on the surface; at another time the bottom and the walls were very rapidly covered with a thin stratum of smooth ice, marked with the minute lines already mentioned in speaking of the ice formed on the walls of the kettles, or else with a stratum of spongy ice composed of crystals crossing one another, like the ice at the bottom of rivers.

It results from these experiments, as might have been foreseen, that *whenever a mass of water is cooled below 0°, and the walls which enclose it are also at a temperature of less than 0°, this water will freeze at the bottom as well as at the surface, and that if water usually freezes only at the surface, it is because the bottom of the water and the walls are for the most part at a temperature above zero.*

In effect, the earth, always at a temperature above 0°, loses its caloric only at the surface by radiation or by contact with colder bodies. Not only is the earth, which forms the bottom and the banks of rivers and great reservoirs of water, a very bad conductor of caloric, but the water and ice are still worse conductors. Ice, being specifically lighter than water, always rises and floats on the surface when the ascensional force produced by its less weight has overcome its adhesion to the bottom; it sometimes even draws up with it from the bottom of the water bodies still more ponderous than itself. The maximum density of water not being at 0°, but at 4°.44, causes all great masses of water which are more or less tranquil, and even such as have a continuous but not whirling movement, to maintain, at the bottom, a temperature above zero, even when the water is at zero, or frozen on the surface. In winter, at a temperature of — 11 and — 15 degrees, the reservoir near the forge of Niederbronn, which is but a metre in depth, is covered with a stratum of ice 25 centimetres thick, and yet the water which flows from it has a temperature of + 3 degrees.

Owing to this admirable concurrence of circumstances, great masses of water never freeze to the bottom; and even when ice does form at the bottom of water, it eventually becomes detached and floats on the surface. If it were otherwise, all our seas, lakes, and great rivers would be but masses of ice which would never be thoroughly melted. Yet we see also that whenever water becomes cooled to 0°, and finds a bottom also cooled to 0°, it congeals at the bottom as well as at the surface. It is necessary, then, in order that ice may be produced at the bottom of water that the latter should be in motion, so that its inferior strata shall be cooled to 0°; that this cold water shall descend to the bottom of the stream; that it shall chill the walls, and finally encounter, in the course of the movement, some point of repose where its force of adhesion, its capacity of crystallization, may be exerted.

To explain this apparent contradiction: A foreign body, an obstacle placed in the midst of a current of water, produces there two different effects; on one hand, it changes the direction of the liquid which strikes it, and gives to that liquid a movement of rotation sufficiently strong sometimes to form vortices; on the other hand, the molecules of the liquid which happen to be immediately behind the obstacle pass into a state of repose, and there are points where they become stationary and motionless. The conditions here are suitable for the formation of ice at the bottom of rivers. The whirling movement produced by the obstacles brings the water cooled to 0° to the bottom of the bed of the river, and thus chills the walls. Hence the molecules of water, nearly motionless behind the obstacle, can there exercise their force of adhesion and crystallization. But, in order to produce these effects, there must be cold both intense and of some duration.

The influence exerted by these obstacles is evident in the different experiments which I have related; we recognize it in the slight asperities of the unplaned and the roughened boards of Lenke, in the pebbles of the Rhine as observed by Fargeaud, and the piers of the bridge of the Aar described by Hugi.

With Arago, then, I attribute the formation of ice at the bottom of water principally to the obstacles which occur in the current; but, in my view, these obstacles are not solely resting points for the crystals, but they serve, on the one hand, to augment the movement of rotation, the vortiginous movement by which the water at a temperature of 0° is made to descend to the bottom of the river; and, on the other hand, they create stationary points in the midst of the movement, where the crystallizing force can exert itself.

I have satisfactorily established the influence of these foreign bodies in the canal which supplies the works at Zinsweiler. During the winter of 1829 ice was formed beneath the water wherever there were large stones, roots, or branches of trees which were immersed in the canal. I caused the formation of this ice to cease almost entirely by having those extraneous bodies removed. Hence the propriety of removing, at least during great frosts, and as far as possible, all grates and bars of iron from the neighborhood of canal locks and flood-gates; all bodies, in short, which tend to communicate an eddying motion to the water.

Upon the whole, then, it may be said: I. That water which is exposed to a temperature below 0° in a vessel or reservoir whose walls may be easily chilled, is always covered first on its surface by a thin stratum of ice, and that afterwards the walls and bottom of the vessel also become covered with a stratum of ice. II. The thickness of this stratum is in direct ratio to the intensity of the cold, and in inverse ratio to the conductibility of the walls; the stratum of ice itself operates as a wall, which is a bad conductor. III. There remains, almost always, at the centre a certain quantity of water which, surrounded on every part by ice, with difficulty loses its latent heat, and does not freeze. Very often, also, a bubble of air, which has been disengaged from the water during its congelation, is comprised in the mass. But it sometimes happens, through intense and continued cold, that the upper ice cracks, and the caloric of the central mass escaping by the fissures, the whole congeals, and the surface assumes a convex and protuberant form, from the expansion of the ice formed at the centre. IV. The plashes or small puddles of water which occur in the roads or fields are promptly covered with a thin crust of ice, which assumes the form of films crossing each other at 30, 60, and 120 degrees, and leaving void spaces between them; the water which filled them is absorbed by the porosity and capillarity of the surrounding earth, and, in turn, becomes congealed. V. It is quite otherwise with water in large masses, for water at 4°.44, being specifically heavier than that at 0°, descends to the bottom, while the water of the surface continues to grow colder, and finally is frozen. VI. In order, then, that ice may be formed at the bottom of great masses of water, it is necessary, first, that the water should be impressed with a movement sufficiently rapid to overcome the superposition by strata according to the differences of specific gravity, and to bring to the bottom the cold strata, so that the water may be chilled to 0° quite to the bottom and the inferior walls, growing progressively colder, shall also be brought to 0°; secondly, that there shall exist in the midst of the current an obstacle against which the water impinges.

In effect, whenever there occurs in a current a body forming an obstacle, the collision with that obstacle augments the movement of rotation of the current, and may even occasion a vortex or eddy in the water; it must be recollected at the same time that behind this obstacle there is a space where the water is in a state of perfect repose—so much so that when the body which forms the obstacle is of considerable volume, a deposit of sand and even pebbles takes place at that point and forms a sort of delta. It is here that the ice of the bottom, the *grundeis*, is formed, the adhesion of which gradually augments the volume of the obstacle and the effect produced, until the moment when, by virtue of its smaller specific gravity, the ice is detached, and is borne to the surface of the water.

At a session of the Society of Natural History of Strasbourg, May 3, 1864, Professor Bertin presented a memoir on the polarization of light by ice, from which it appears that the ice formed at the bottom of water polarizes light in the same manner as the ice formed at the surface.

There is a valuable article on ice, by L. F. Kœmtz, in the *Encyclopædie* of Ersch and Gruber. The specific gravity of ice is there given as follows: According to Kraft, 0.905; Irwin, 0.937; Scoresby, 0.9146, 0.9166, 0.9253; Royer and Dumas, 0.950; Osonn, 0.9268. Thomson, in his *Chemistry*, gives 0.2900, which is nearly the mean of the above numbers.

As regards the form of the crystal of ice, Haüy thought that it might be deduced from the octahedron. Brewster (*Poggendorff's Annalen*, t. vii, 509) recognizes hexahedrons terminated by three planes. Hericart de Thury (*Ann. de Chimie et Physique*, xxi, 156) adopts the hexahedral prism. Clarke, (*Trans. of the Philosophical Society of Cambridge*, 1213,) in a seemingly very exact article, pronounces for rhombohedrons, with angles of 120 and 60 degrees. This is confirmed by Marx and Brewster, (*Ann. de Poggendorff*, xxxii, 399,) and seems corroborated by our own observation of lines crossed at 30, 60, and 120 degrees, as already noticed. See also Scoresby: *Cristillisation de la neige*, in the *Ann. de la Chimie et de Physique*, t. xviii.

THE EARTHQUAKE IN EASTERN MEXICO OF THE SECOND OF JANUARY, 1866.

BY DR. CHARLES SARTORIUS, OF MIRADOR, NEAR HUATASCO, DISTRICT OF CORDOVA, STATE OF VERA CRUZ, MEXICO.

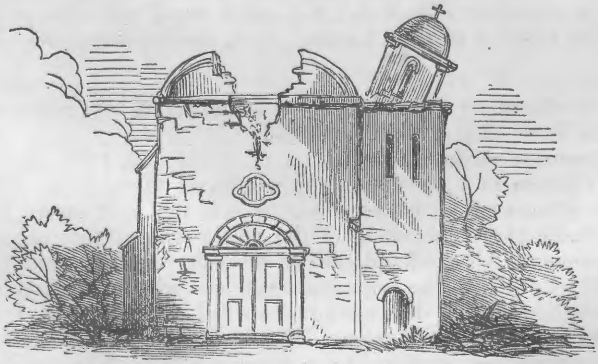
[From a letter addressed to the Smithsonian Institution.]

On the 2d of January, 1866, at six hours ten minutes in the evening, the earth shook without detonations; the movement seemed to be vertical, and lasted some ten seconds; then followed a strong shock from west to east; after eight seconds another equally strong, succeeded by strong oscillations which endured for about twenty seconds, and finally subsided in a tremulous motion. The duration of the whole phenomenon I estimated here at one minute. The beams of houses creaked and were perceptibly moved; doors opened and closed; utensils were thrown down from west to east; mirrors and pictures on the wall shook to and fro; and a pendulum, which I had suspended by a long string, exhibited vibrations of one yard for ten minutes after the whole was over. The column of the barometer sank and rose—the magnetic needle vibrated. These were the phenomena here, but no walls were fractured, and the high chimneys of the steam and sugar factories remained uninjured. The hills consist throughout of conglomerates, which may be observed in ravines to the depth of 500 feet.

Huatasco lies about ten miles (English) south from this place, surrounded by volcanic hills, (trachite and lava,) in a southwardly direction from which project three mountain craters, composed of crystalline limestone. Here the effects were much stronger. In the cathedral a part of the dome fell in; the walls of many houses were ruptured; hundreds of earthen and porcelain vessels were dashed to pieces. In two neighboring villages the churches were destroyed, and large masses of rock were detached in the adjacent limestone mountains and rolled into the valley.

The focus and centre of the whole concussion was the volcano of Orizava, which rises ten miles southwestwardly from Huatasco. At its foot, on the east side, lies the city of Coscomatepec, of 4,000 inhabitants. Here the concussion was so violent that the new and strongly-built parish church, with cupola and trebly vaulted dome, was reduced to ruins; many private houses were rendered uninhabitable; many walls overthrown, but no human life was lost. On the west side of the great mountain, the city of St. Andres Chalchicomula suffered more than any other; several churches and numerous houses were destroyed, and many of the inhabitants buried under the ruins.

The history of the country informs us that the volcano was in activity from 1559 to 1569, but has since been at rest. Yet, although eruptions may have ceased, certain it is that the flame in its depths has not been quenched. This is manifested by the columns of smoke which ascend from time to time, the *fumarolles* on the sides, glowing hot rocks on the western declivity, hot sulphurous springs at the eastern foot, &c. On the morning after the earthquake it was observed that the snow-covered cone (3,000 feet, the absolute height of the mountain being 17,800 feet) was almost bare of snow on the south side, while the southwest border of the crater had fallen and a vertical cleft of the highest peak had taken place. This cleft traversed the entire summit, and from its deepest part gushed great volumes of water. The length of the cleft is estimated at 1,000 metres; its breadth at one metre. The volcano is surrounded by limestone mountains, and stretches southwardly in a calcareous formation. Wherever limestone forms the chief mass of the chain, the effect of the concussion was greatest. In the village of Chocaman, on the southeast side of the volcano, between high calcareous ranges, under which extends a stratum of clay slate, the violence of the concussion is most conspicuous. The church built in the sixteenth century fell, but of the three stories of which the tower consisted, the middle one was precipitated outward, so that the highest fell upon the lowest, and remained standing.



In the city of Cordova the church of the convent of San Antonio, two hundred years old, sustained the loss of its cupola and the dome of its nave, while the parish church—a handsome building with three vaulted naves—is so injured as to be unfit for divine worship; the large town hall, with its arcades, the hospital, and many private dwellings were likewise so much damaged as to threaten to fall.

Orizava, eight miles west from Cordova, though surrounded by high calcareous ridges, suffered less; the reason of which may be that the town stands on the site of an ancient lake, which, after a bed of from six to eight feet of dam-earth, contains a thick stratum of chalk, and from its porosity propagated the shock with less force; yet damage was not wholly averted. A church once belonging to the Jesuits was thrown down, as was also the upper part of the high tower of the cathedral. This part had been but newly built, in order to place a clock thereon, when the earthquake of October, 1864, hurled it thirty yards distant towards the market-house; the reconstruction was resumed, and on the day when the clock was placed in position the earthquake again demolished the dome of the tower, but without injury to the clock.

From Orizava a valley stretches to the southwest, bordered by steep rocks, through which passes the highway leading to Mexico. In this valley, which is twelve miles long, the concussions were peculiarly violent. Several churches and many private houses were destroyed, and a number of persons lost their lives. Further to the south the towns of Tehuacan and Oajaca were visited most severely; in the latter five churches were wholly or in part demolished. From the report of a traveller the force of the shocks, in the long distance from Orizava to Oajaca, was peculiar, the effects being rather extended in length than diffused in breadth. The neighborhood of the calcareous mountains, which run parallel to the great chain of the Andes, would seem to have determined the conditions of the concussions.

Northward from the peak of Orizava, it was only quite near that point that the movement was violent; here a church was destroyed. In Jalapa, though very perceptible, the phenomenon was attended with no damage. The same was the case to the east, as, for instance, at Vera Cruz. A strong norther was blowing at the time, on which account the effects were not perceived on board the ships in the harbor.

To the westward the oscillations were slighter according to the greater distance from the peak of Orizava. In Puebla the concussions were strong, but not so much so that large buildings were injured. In the city of Mexico the effects were mitigated. According to a communication from a point seventy-two miles west of the latter city, an earthquake, but of no considerable force, was distinguishable.

All opinions concur in assigning the peak of Orizava as the focus of the phenomenon, as it was also in October, 1864, and it would not be surprising if that mountain, after a repose of three hundred years, should again commence its eruptions.

NOTE.—The following supplementary communication was received from Dr. Sartorius, in a letter of the 27th May, 1866:

"We had another earthquake at 9.15 a. m. of May 10, 1866, extending from the peak of Orizava in a direction from southwest to northeast. There were three shocks, at intervals of ten seconds, the last the most severe, followed by continued tremblings; the duration of the whole was seventy seconds, during which the barometer fell one-hundredth of an inch, returning immediately after to its original condition. The magnetic needle was without declination, but with a strong inclination north. The earthquake was local, only feeble traces having been observed in the city of Mexico. The shock was so severe in Orizava that several houses were destroyed and some persons killed. The oscillations were from north to south; the time the same.

"On the 27th of April a violent shock was noticed southwest of the city of Mexico, proceeding from the volcano of Jorullo, of which no traces were observed here."

STATISTICS RELATIVE TO NORWEGIAN MOUNTAINS, LAKES, AND THE SNOW-LINE.

DEPARTMENT OF STATE,
Washington, November 22, 1865.

SIR: I transmit herewith, for your information, and the use of the Institution, a collection of tables, maps, &c., showing the height of the Norwegian mountains, with other valuable papers transmitted to this department by the consul of the United States at Bergen as enclosures in his despatch No. 52, dated September 29, ultimo.

I will thank you to advise this department of the receipt of the same.

I am, sir, your obedient servant,

F. W. SEWARD,
Assistant Secretary.

JOSEPH HENRY, Esq.,
Secretary of the Smithsonian Institution.

STATISTICS OF NORWEGIAN MOUNTAINS EXCEEDING THREE THOUSAND FEET IN HEIGHT.

In the geographical publications relative to Norway published in foreign countries, the heights of few of its mountains have been given, and even of those few, the statements with regard to many are erroneous. I have therefore deemed it expedient to compile a table of those Norwegian mountains which exceed in height 3,000 feet above the level of the sea. The measurements in all cases have been reduced to English feet, the Norwegian foot being equal to 1.029357 English; all fractions under half a foot have been stricken out, and all over half counted as one.

The measurements made by theodolite, levelling, or other geometrical methods are specified; all others were obtained by the barometer. I have given all the different measurements and results made by different persons, and it will be observed that many of these measurements, made at different times, differ considerably. Such discrepancies are due to the state of the atmosphere and forbid perfect confidence in the results, but great heights cannot well be measured by levelling or other geometrical methods without involving much time and great expense. It is, however, thought that when measurements are made on different days, the mean result will be nearly correct.

The length and breadth of the larger lakes of Norway stated herein are derived from Mr. Schöth's Geography, in which the dimensions are given in Norwegian miles, all fractions below one-sixteenth of a mile being rejected. The dimensions are reduced to English geographical miles, the proportion adopted being one Norwegian mile to six English geographical miles. There are, in addition to the lakes whose sizes are here given, many smaller, but, as it was only my intention to furnish a correct enumeration of the larger ones, as of the highest mountains, all less than one Norwegian mile in length have been left unnoticed, as being of inferior interest.

I have also given the height of the snow line indicated by different observers, from which it will be seen that the gradual descent of this line toward the north, as stated by Professor Leopold Von Buch in his travels through Sweden and Norway, cannot be fully relied upon. By comparing the various statements, we must conclude that the height does not only depend upon the latitude, but also to some extent on the longitude as well as physical conditions of the different localities.

As many of the names of the Norwegian mountains allude to their form or other peculiarities, I have deemed it expedient to give, in alphabetical order, explanations of such, derived wholly from Major Vibe's measurements of heights.

I have also enumerated, in alphabetical order, the authors to whom we are indebted for the measurements, with references to the publications in which they have been given.

I am especially indebted to Mr. Carl Stoud Platou, mayor of this city, Mr. J. Geelmûyden, A. M. and rector of the Latin school, both authors of geographical publications, for much valuable information; to Dr. Johan Kohren, director of the public museum of Bergen, for his kindness in facilitating for me the use of the manuscripts and general library of the museum; and to Mr. J. J. Aastrand, director of the observatory of Bergen, for valuable assistance in the arrangement of this compilation.

O. E. DREUTZER.

CONSULATE OF THE U. S. OF AMERICA,
Bergen, September 28, 1865.

Heights of mountains over 3,000 *feet above the ocean.*

THE AMT OF HEDEMARKEN.

Blekkefjald, Tryssil, in the district of Osterdalen	3,602	feet above the sea, measured by Keilhau, in the year 1838.
Bratfjeld, east of Fæmundsjö, (lake,) near the Swedish boundary	3,045;	Hörby; 1854.
Drevfjeld, at Tryssil, Osterdalen, point upon the boundary line between Sweden and Norway, mark No. 131	3,222;	Hertzberg, by theodolite; 1849.
Highest summit of the mountain, mark No. 132	3,442;	Hertzberg, by theodolite; 1849.
Eldaahögda, between Elvedalen and Ringebu	4,148;	Brock; 1827.
Elgepiggen, at Rendalen, between the river Glommen and Lake Fæmund	5,306;	Brock; 1827.
Elgaahaagn, east of Fæmundsjö	4,882;	Hörby; 1854.
Faxefjeld, point on the Swedish boundary line, at Tryssil, Osterdal, the southwest horn	3,088.	The north horn, upon the Swedish side of the boundary, southeast of the boundary mark No. 126.
	2,918;	Keilhau; 1838.
Flerstöten su Faxefjeld.		
Fulufjeld, at Tryssil, upon boundary line, mark No. 127	3,225;	theodolite; Hertzberg; 1849.
Do ... do ... No. 128	3,340:	theodolite; Hertzberg; 1849.
Do ... do ... No. 129	3,538;	theodolite; Hertzberg; 1849.
Gröttingbratten, west of the river Glommen, in the district of Rendalen	3,820;	Brock.
Gröthaagn, east of Lake Fæmund, district of Osterdal	4,701;	Hörby; 1854.
Herjehagna, point upon the Swedish boundary, in the district of Osterdalen	3,881;	Keilhau; 1838.
	3,720;	Hörby; 1854.
	4,069;	theodolite; Hertzberg; 1849.
Humelfjeld, or Graahögda, highest point, district of Osterdalen	5,148;	Brock.
	5,093;	Hörby; 1847.
Nehatten, Osterdalen	4,323;	amt map.

Salfjeld, or Salsfjeld, east of Lake Fæmund, at the Swedish boundary, Osterdal	4,153;	Hisinger; the north horn.
	3,946;	Hörby; 1854.
Sörkvola, east of Lake Fæmund, Osterdalen	3,088;	Hörby; 1854.
Svukufjeld, on the Swedish boundary, Osterdalen	4,702;	Hisinger.
	4,692;	amts map.
Svukustöt, east of Lake Fæmund, Osterdalen	4,624;	Hörby; 1854.
Sölen, or Solentinden, southwest of Lake Fæmund, Osterdalen	6,176;	Kraft.
	5,835;	mean of two measurements; Hörby; 1848.
Sölen, (Storsölen,) at the Little Elvedal, Osterdalen	6,176;	Brock.
Sölenkletten, lower peak, separated from Storsölen in Osterdalen	4,644;	Brock.
Tronfjeld, at Tönset, Osterdalen	5,749;	Esmark.
	5,632;	Hisinger.
	5,519;	Brock.
	5,916;	Hörby; 1854.
Tryssilfjeld in about the centre of the parish of Tryssil, Osterdalen	3,738;	Keilhau; 1838.
Vonsjogusten, upon the Swedish boundary, in the north of the district of Osterdalen	3,420;	Hörby; 1854.
	3,483;	theodolite; Hertzberg.

THE AMT OF CHRISTIANS.

Aakerkampen, at Gudbrandsdal, in the district of Quiœkne	4,632;	Brock; 1827.
Armehornet, at Vang, district of Valders	4,966;	Wergeland; 1843.
Aursjöhöe, at Lom, Gudbrandsdal	4,152;	Wergeland; 1841.
Bitihorn, in the north of the district of Valders	5,324;	Keilhau and Brock
Bounaaset, upon Filefjeld, northwest, in the district of Valders	4,404;	Wergeland; 1843.
Bæverhöiden, at Lom, Gudbrandsdal	3,635;	Wergeland; 1844.
Brurskardknatten, at Valders	5,017;	Wergeland; 1843.
Digervarde, between Lom and Læssö, Gudbransdalen	5,661;	Brock; 1827.
Dovre range of mountains:		
Blaahat, (at the snow line)	5,353;	Naumann.
Liltvaratind	5,353;	Naumann.
Fogstuehie, or Graahöe, about the highest of the mountain back, south of the river Foldal, between Hjærkin and Fogstuen	5,692;	Fearnley; 1841.
	5,685;	Brock.
Knulshoe, east of Kongsvold	5,456;	Brock; 1826.
Sletfjeldet, upon Dovre	5,457;	mean height; Naumann
Storekuven, east side of Dovredal	4,885;	Brock.
Dryllenaaset, the outer horn between Voldal and Bygdensö, (lake,) district of Valders	4,868;	Wergeland; 1843.
Dyringshöe, at Lom, Gudbrandsdal	4,264;	this point is of even height with Opnaaset, and about 205 feet lower than the highest top of the mountain of same name west of Söverhöe; Wergeland; 1841.
Elje Vashöe, the lowest point of the ridge, between the Great and Little Vashöe (Elje Vashöe)	3,649;	Fearnley; 1841.
Fagersetnaaset, north of Ionskardsœter, Valders	5,475;	Wergeland; 1843.
Filefjeld, upon the boundary between the Stifts of Christiania and Bergen:		
Stugunaaset	4,827;	Wergeland; 1843.
Sörfjeld, by Nystuen	3,163;	Sommerfelt.
Formekampen, at Vaage, in Gudbrandsdal	4,836;	Brock.
Fuglhöe, isolated horn between Vee and Grönflyen in the high mountain south of Ottavandet, (lake,) Gudbrandsdal	4,894;	Brock.
Fæförkampen, at Sodorp, Gudbrandsdal	3,945;	Brock.
Gaakilskardet, in the north of Gudbrandsdal	4,952;	Brock; 1827.
Galdebergknausen, foot of, in the district of Valders	5,059;	Brock and Keilhau.

Galden, at Tyrevand, (lake,) in the district of Valders	4,676;	Wergeland; 1843.
Galdhöpiggen, great, or Ymesfjeld, the highest mountain in the north of Europe, at Lom, Gudbrandsdal	500	feet higher than Glittertinden; also about 8,544 feet; Wergeland; 1841; 8,338, or 7,850, about 416 feet below the summit, measured by theodolite; Hansten and Andersen; 1844; mean of three measurements.
Glellerjhöe, at Lom, Gudbrandsdal	6,845;	this point is of even height with Skauthöe, and about 300 feet higher than Laurhöe, and 21 feet lower than Rundhöe; Wergeland; 1841.
Glittertinden, at Lom, Gudbrandsdal; part of Iotunfjelden	8,040;	about 50 feet below the highest snow horn; Wergeland; 1841.
Graahöe, on the range of mountains between the districts of Valders and Gudbrandsdal	5,134;	Brock.
Graahögda, the highest crag of the group of mountains called Klövklön, between Vinebygden and Södrop, Gudbrandsdal	4,837;	no mountain situated further south reaches this height.
Grindadden, in the district of Valders	5,605;	mean of three measures; Holmboe; 1829.
Grötaafjeld, at Lom, Gudbrandsdal	6,382;	in about two miles west of this is a high tind or horn of even height with Sundedalspiggen and Haslbrahöe: Wergeland; 1841.
	6,921;	Vergeland; 1845.
Grönflyen, by the Otavandet, (lake,) Gudbrandsdal	5,043;	Brock.
Grönsendknippen, at Slidre, district of Valders	4,370;	Brock and Keilhau.
Heggerbothövde, or Heggerbollensæter, at Lom, Gudbrandsdal	4,505;	Wergeland: 1841.
	4,193;	Wergeland; 1842.
Heilstuguhöe, (see Nautgardstinden,) in the north of Gudbrandsdal	7,677;	Brock.
	7,537;	Wergeland; 1841.
Heindalshöe, north of Hvindalsvand, (lake,) Hvindal, in the Gudbrandsdal	5,970;	Brock; 1827.
Höggjöemen, between the rivers Tora and Töisa, at Lom, Gudbrandsdal	5,707;	Wergeland; 1842.
Höigien, at Roekne, in the Gudbrandsdal	3,966;	birch timber is found upon the summit: Brock.
Höitinden, at Gudbrandsdal	3,933;	Brock; 1827.
Itdmandshöeden, between Ætnedal and Sæl, Gudbrandsdal	5,611;	the highest crag is about 100 feet higher.
Jetta, upon the ridge between Otavand (lake) and Dovre	5,433;	Brock.
Jukuleggen, or Högeloftseggen, between Valders, Hemsedal, and the inner sogn	6,291;	Wergeland; 1845.
Kalvaahigda, or Hugnafjeld, in the district of Valders	7,155:	Brock.
	7,139;	Brock and Keilhau.
Kraakhoved, between Birid and Terpen, Gudbrandsdal	3,203;	Suhrland; 1841.
Kringla, (see Styrfjeld.)		
Kroshoe, at Lom, Gudbrandsdal	5,712;	Wergeland, 1842.
Kvammenaaset, at Vang, District of Valders	3,901;	Wergeland; 1843.
Kvitingskjolen, at Lom, Gudbrandsdal	6,333;	Wergeland; 1841.
	6,587;	Brock; 1827.
Kvilljernklubben, at Vang. Valders	5,170;	Wergeland; 1843.
Lerhöe, at Lom, Gudbrandsdal, (see Nautgardstenden)	7,617;	Wergeland; 1841; mean of several measurements.
	7,677;	Brock.
	7,537;	Wergeland; 1841.

Lomseggen, at Lom, Gudbrandsdal; the northeast peak.4,982; Naumann.
5,087; Wergeland; 1841.
The peak further to the westward ....5,977; Naumann.
The southwestern peak............................6,653; Naumann.
6,926; mean of several measures; Wergeland; 1841.
6,653; Brock; 1827.
Lomshorungen, at Lom, Gudbrandsdal ....5,661; Brock; 1827.
6,654; Wergeland; 1842; the latter is considered too large.
Mosebakfjeld, at Lom, Gudbrandsdal..................3,596; Wergeland; 1841.
Muen, or Muven, at Ringebu, Gudbrandsdal..........4,756; the last mountain south of Rundane and Sölenklellen, being cone-formed; Brock.
Mugnafjeld, (see Kalvaahögda.)
Mukampen, between Hedalen and Krikne, in Gudbrandsdal..5,898; Broch.
Nautgardstinden, in the north of Gudbrandsdal ....7,677; Broch.
7,537; Wergeland: 1841.
Norekampen, between Fröen and Ringebu, Gudbrandsdal...3,620; Broch; 1827.
Nysoterkampen at Ringebu, Gudbrandsdal............3,602; Broch; 1827.
Pighætten, upon Dovre, the last horn upon the, from northwest towards Hjærkin, stretching mountain back, 5,105; Brock.
Pekhætta, according to Professor Naumann, a high point south of Vaataasö, (lake,) upon Dovre ....4.632; Naumann.
Præstkampen, in the north of Gausdal, Gudbrandsdal..4,094; Broch; 1827.
Rundane, also called Runderne, the western horn of a collection of alps, between Ætnedal and Sæl, Gudbrandsdal..6,392; Broch.
The north horn, Högrund ....6,691, and over. The Lake Rond (Rondsöen) divides Rondene or Rundkumpene into two separate groups of mountains.
The three southern horns...........................6,928; Broch.
The furthest off horn, Degerrund...................6,270; Fearnley; 1841.
Rulen at Gausdal, Gudbrandsdal......................4,998; Keilhau; 1843.
4,936; Brock; 1827.
Sigurdshöpiggen, at Vaage, Gudbrandsdal............5,661; Broch; 1827.
Skaget, between the districts of Gudbrandsdal and Valders..5,548; Broch.
Skarhöe, on the boundary between Vaage and Lom, Gudbrandsdal.....................................5,355; Broch; 1827.
Skalstenden, or Skardstinden, upon the boundary between Læssö and Lom, Gudbrandsdal ....6,176; Broch; 1827.
Skarvdalseggen, at Lom, Gudbrandsdal ....6,315; Wergeland; 1841.
6,326; Wergeland; 1842.
6,485; Broch; 1827.
Skotten, at Vang, district of Valders.................5,073; Wergeland; 1843.
Stagsfjeld, at Ringebu, Gudbrandsdal..................3,960; Broch; 1827.
Sletflykampen, at Lom, Gudbrandsdal..................4,514; Vergeland; 1841.
Snehetten, or Snehætten, upon Dovre, Gudbrandsdal..8,120; Esmark.
7,509; Skreahög is of about the same height; Naumann.
7,566; Hisinger.
7,662; Everest.
7,588; Shultz.
Stebersberget, between Fröen and Rengebu, Gudbrandsdal...3,602; Broch; 1827.
Steenflybræpiggen, at Lom. (See Nautgardstinden.)
Storhögda, at Læssö, Gudbrandsdal....................6,691; Broch.
Storhöpiggen, at Læssö, Gudbrandsdal ....4,726; Broch.
Stugunaaset. (See Filefjeld.)
Stygfjeld, upon the boundary of Osterdal and Gudbrandsdal: the western horn...........................5,892; Fearnley; 1841.
The eastern horn ....6,176; Fearnley; 1841.
The lowest of the mountain back, stretching from Stygfjeld to the Great Kringla, being the dividing ridge between the rivers Gremsaa and Deraaen, in Gudbrandsdal.................................3,435; Fearnley; 1841.

Svartdalspiggen, at Lom, Gudbrandsdal	7,128;	this horn is about fifty feet lower than the one in the east from this rising horn of the same name; Wergeland; 1841. (See Nautgardstinden.)
Srartkampen, at Sæl, Gudbrandsdal	4,518;	Brock; 1827.
Synstakerken, at Lom, Gudbrandsnal	4,364;	Wergeland; 1842.
Sörfjeld. (See Filefjeld.)		
Tjernhuttinden, at the north of Gudbrandsdal	7,823;	Wergeland; 1841.
	7,679;	Wergeland: 1843.
Troldhovd, at Vang, in the district of Valders	3,208;	Wergeland; 1843.
Tværaadalskerken, or Stændalskirken, in about the centre of the snow plain between Lom, in Gudbrandsdal, and Sogn, in amt of North Bergenhus	6,886;	Wergeland; 1842.
Tværfjeld, at Lom, in the district of Gudbrandsdal	6,136;	Wergeland; 1841.
Tykningsuen, at Lom, Gudbrandsdal	7,772;	Wergeland; 1841.
Veslefjeldtinden, the outer horn, between Lurdal and Bævervand, (lake,) Gudsbrandsdal	6,912;	Brock.
	7,224;	mean of two measures; Wergeland; 1841.
Vulueggen, at Lom, Gudbrandsdal, highest horn	5,672;	about 300 feet higher than the highest Veslefjeld in Otadalen.
Ymesfjeld. (See Guldhöpiggen.)		

THE AMT OF BUSKERUD.

Augendshoug, at Krydsherred, west of Noufjeld	3,990;	Wergeland; 1840.
Björdalsnuten, at Skurdal, in the district of Numedal	4,527;	Bassöe; 1846.
Bolhövd, at Aal, district of Hallingdal	3,960;	Wergeland; 1845.
Brakanuten, upon the boundary between Numedal and Telemarken, and near the boundary of Hardanger	4,454;	Bassöe; 1846.
	4,863;	Næser; 1852.
Dyna, the highest point of the range of mountains between Næs and Aal, at Hallingdal	4,359;	Wergeland; 1845.
Eidsfjeld, at Rollag, highest peak	4,387;	Wergeland; 1845.
	4,506;	Næser; 1846.
	4,351;	Bassöe; 1846.
Next highest peak	4,347;	Naumann.
Tjeldsenden, the eastern horn upon the mountain, backs north of the great (store) Strandefjord, at Aal, in the district of Hallingdal	5,935;	Wergeland; 1845.
Hagelbunatten, the southern and highest top of the Renfjelds, at Sigdal	4,219;	Wergeland: 1842.
	4,148;	Wergeland; 1845.
Haldalshögda, south of Haldal, on boundary between Hallingdal, Borgund, and Urland	5,740;	Wergeland; 1845.
Hallandsfjeld, in the north of Numedal	3,835;	Wergeland; 1845.
Hallingnallen, horn upon Rensjofjeld at Næs, district of Hallingdal	4,391;	Wergeland; 1842.
	4,303;	Wergeland; 1845.
Hallingskarven, between the districts of Hallingdal and Hardanger	6,073;	Keilhau; 1849.
Folaskardskarven, horn upon Hallingskarven, at Hallingdal, near the boundary line of the stift of Bergen	6,432;	Wergeland; 1845.
Præstholtskarven, high horn upon Hallingskarven	6,331;	Vibe
Halnekollen, between Numedal and Hardanger	4,539;	Bassöe; 1846.
Hestejuvnatten, at Næs, Hallingdal	3,494;	Wergeland; 1840.
Högvarden, the highest mountain top between Eggedal, in Sigdal, and Flaa, in Næs, about three miles east of the church of Hovland, (Eggedal)	4,793;	Wergeland; 1845.
Jorunfjeld, west of the Tunhovdfjord, Numedal	4,237;	Wergeland; 1842. 4,406; mean of two measures upon different days; Wergeland; 1845.
Jukuleggen, or Högeloftseggen, between Hamsedal and Valders, at Tndre Sogn	6,281;	Wergeland; 1845.
Jöranfisen, west of Sperilensjö, (lake)	3,443;	Keilhau; 1838.
Korpenatten, upon Renfjeld, Hallingdal	4,453;	Wergeland; 1843.

Krækjehua, between Numedal, Hallingdal, and Hardanger	4,357 ;	Bassöe ; 1846.
Laugaasen, two miles east of Rollaûg, (church,) district of Numedal	3,013 ;	mean of two measures ; Næser ; 1846.
Norifjeld, at Rollaûg, Numedal	4,941 ;	Keilhau ; 1838.
	4,962 ;	Wergeland ; 1842.
Nybunaaset, at Aal, Hallingdal	5,631 ;	Wergeland ; 1845.
Oddenakken, at Aal, Hallingdal	4,012 ;	Wergeland : 1845.
Præslholtskarven, horn upon Hallingskarven, east of Buckehalliden	6,331 ;	see Præsleholtskarven, under Hallingskarven.
Raubergskarven, at Aal, in the district of Hallingdal	5,945 ;	Wergeland ; 1845.
Rauhellernuten, upon the mountain plateau of Hardanger, in western Numedal	4,696 ;	Bassöe ; 1846.
Reinsfjeld, at Aal, Hallingdal	5,872 ;	Wergeland ; 1845.
Röislandsfjeld, at Rollaûg, Numedal	4,263 ;	Wergeland ; 1845.
Saabölsfjeld, at Flaa, Hallingdal	4,211 ;	Wergeland ; 1842.
Saatenatten, upon the boundary between Numedal and Næs, Hallingdal	3,737 ;	Wergeland ; 1845.
Sangerfjeld, at Aal, Hallingdal	3,813 ;	Wergeland ; 1842.
	3,940 ;	Wergeland ; 1845.
Sigvidnuten, at Opdal, Numedal	4,098 ;	Bassöe ; 1846.
Skarseggen, Opdal, Rollaûg, Numedal	4,336 ;	Bassöe ; 1846.
Skaupsjönuten, between Numedal and Hardanger	4,665 ;	Bassöe ; 1846.
Skogshorn, at Hemsedal, Hallingdal	5,661 ;	and 2,500 above the base; Keilhau.
Skræken, upon the mountain plateau of Hardanger, west at Opdal, Rollaûg, Numedal	4,697 ;	Bassöe ; 1846.
Slaabefjeld, about one mile west of the church of Rollaûg, Numedal	3,460 ;	Wergeland ; 1845.
Synhovde, at Rollaûg, in the district of Numedal	3,461 ;	Næser ; 1846.
	3,719 ;	Naumann.
	3,893 ;	Næser ; 1846.
Synhovd, at Opdal, Rollaûg, Numedal, about twenty miles northwest of the aforenamed Synhovde, with which it should not be exchanged	4,651 ;	Bassöe ; 1846.
Syningen, horn, about four miles northwest of Terpe, Hallingdal	3,478 ;	Keilhau & Suhrland ; 1842.
Söllandsfjeld, on the west side of Eggedal	3,475 :	Wergeland ; 1842.
Tjonaasnaasen, or Tjærnaasnaasen, at Rollaûg, Numedal	3,813 ;	mean of two measures upon different days ; Bassöe ; 1846.
Tunhovdaas, at Rollaûg, Numedal	3,449 ;	Wergeland ; 1845.
Usletind, at Aal, Hallingdal	4,633 ;	Bassöe ; 1846.

THE AMTS OF JARLSBERG AND LAURVIG.

There are no mountains in these amts over 3,000 feet.

THE AMT OF BRATSBERG.

Aardalssaata, at Hviteseid, district of Ovre (upper) Thelemarken	3,695 ;	Bassöe ; 1847.
Baasnuten, at Aamotsdal, Thelemarken	4,747 ;	mean of two measures upon different days; Bassöe ; 1846.
	4,732 ;	Bassöe ; 1847.
Blaanuten, the highest summit of Haakenæsfjeld, at Tind, Telemark	3,997 ;	Haakenfjeld is the base of Gausta ; Smith.
Blefjeld, upon the boundary between the districts of Numedal and the Thelemarken	4,364 ;	Maochmann.
West Blefjeld	4,463 ;	Næser ; 1846.
East Blefjeld	3,919 ;	Næser ; 1846.
Bosnuten, by the lake Mjosero in the Thelemarken	4,691 ;	Næser ; 1853.
Brakanuten, between Numedal and the Thelemarken, near the line of Hardanger	4,463 ;	Bassöe ; 1846.
	4,863 ;	Næser ; 1852.
Brokefjeld, at Hviteseid, in the Thelemarken	3,488 ;	Suhrland and Ellesen.
	3,599 ;	Næser ; 1847.

Djupsjöhövde, at Tind, in the Thelemarken	4,893;	Bassöe; 1847.
	4,854;	Næser; 1853.
Eirfjeld, at Höidalsmo, Laurdal, Thelemarken	3,398;	Bassöe; 1847.
Eistenfjeld, at Tyresdal, Moland, in the Thelemarken	3,912;	Bassöe; 1847.
Erthammer, in the south of Hjertdal, Thelemarken	3,533;	Bassöe; 1847.
Fliseggen, at Vinje, in the Thelemarken	5,341;	Bassöe; 1847.
	5,312;	Næser; 1853.
Gluggvarde, at Nissedàl, in the Thelemarken	3,035;	Næser; 1847.
Glugsvardeggen, west of Vinje, in the Thelemark, near the boundary of the stift of Bergen	4,311;	Bassöe; 1847.
Grasnuten, at the upper Thelemark, near the boundary of the stift of Bergen	4,961;	Bassöe; 1847.
Goustafjeld, in the district of Thelemark	6,268;	Esmark.
	6,259;	Schouw and Smith.
	6,182;	Smith.
	6,134;	Mashmann.
	6,169;	Holmbœ; 1829.
	6,159;	Bassöe; 1846.
	6,231;	Næser; 1853.
Hegefjeld, west of Nesservandet, (lake,) at Hviteseid, Thelemarken	3,454;	Næser; 1847.
Hellekionnuten, at the east of Tind, Thelemark	4,007;	Næser; 1846.
Himingen, at Hjærtdal, in the Thelemark	3,444;	Bassöe; 1847.
Hætte, upon the mountain plateau of Hardanger, at Tind, Thelemark, 12 miles north of Mjösolvandet (lake)	4,653;	Bassöe; 1846.
Juvigfjeld, upon the east side of the north of Lake Mjösen, Tind, Thelemarken	5,017;	Smith.
	4,951;	Bassöe; 1846.
	5,176;	(this latter point of the mountain is not the one measured by Bassöe;) Næser; 1853.
Kjelsnuten, or Kilsnuten, at Vinje, Thelemark	4,308;	Bassöe; 1847.
Kollingerne, south of Hviteseid, upon the boundary between Vraadal and Torrisdal, Thelemarken	3,111;	Næser; 1847.
Krekledyrflottet, upon Haukelidsfjeldet, at Vang, Thelemarken	3,657;	Holmbœ; 1829.
Kvamsfjeld, at Tind, in the district of Thelemarken	4,789;	Smith.
Maarsnaas, at Tind, Thelemarken	4,853;	Bassöe; 1846.
	4,666;	Bassöe; 1847.
Medlfjeld store, (great,) at Vinje, in the district of Thelemarken	4,684;	Bassöe; 1847.
Mælfjeld, or Melfjeld, at Siljord, Thelemarken	4,696;	mean of two measures made upon different days; Bassöe; 1847.
	4,672;	Næser; 1853.
Oklednuten, at Tind, on the Thelemarken	4,195;	the highest point north of the extent of mountains between the Thelemarken and Remsdal; Næser; 1846.
	4,099;	Næser; 1853.
Præstetinden, at Tind, Thelemarken	4,153;	Bassöe; 1846.
Raulandsfjeld, west of Lake Mjösen, at Vinje, Thelemarken	5,157;	Bassöe; 1847.
	5,098;	Næser; 1853.
Rautefjeld, at Mo, on the Thelemarken	4,487;	Bassöe; 1847.
Rohottsfjeld, upon the boundary between Vraadal and Hviteseid, Thelemarken	3,348;	Næser; 1847.
Rusfjeld, upon the boundary between Nissedal and Moland, Thelemarken	3,345;	Næser; 1847.
	3,117;	Bassöe; 1848.
Ræksjöhovde, at Tind, Thelemarken	4,977;	Bassöe; 1847.
Selsnuten, between Vinje, on the Thelemarken, and Suledal, Ryfylke	4,572;	Bassöe; 1847.
	4,551;	Næser; 1853
Skarfjeld, at Tind, Thelemarken	4,923;	Bassöe; 1846.
Skorvefjeld, at Siljord, Thelemarken, in northwest from the church	3,942;	above the Paronaye at Siljord, Carpelan.
	4,448;	Bassöe; 1847.
Storefjeld, in the western part of the Thelemarken	4,887;	Bassöe; 1847.

Tangefjeld, at Vinje, in the Thelemarken	4,677;	Mashmann; 1832.
Tesungtind, in the north of Tind, Thelemarken	4,636;	mean of two measures upon different days; Bassöe; 1846.
Tindenfjeld, at Tind, in the Thelemarken	3,842;	Smith; 1812.
Tjönnefjeld, north of Tyrisvand, (lake,) at Moland, Thelemarken	3,399;	Bassöe; 1847.
Urdnaasfjeld, upon the boundary between the parishes of Mo and the annex of Skaffe, Thelemarken	4,865;	Bassöe; 1847.
Vasdalseggen, between Vinje, in the Thelemarken, and Suledal, in Ryfylke	5,433;	Næser; 1853.
Vatndalsnuten, between Tind and Vinje, Thelemarken	4,932;	Mashmann; 1832.
Vehuskjæringa, at Laurdal, Thelemerken	4,508;	Bassöe; 1847.
Venaasfjeld, at the south end of Tindsö, (lake,) Hjertdal, Thelemarken	3,480;	mean height of two measures; Næser; 1846.
Vindeggen, in the west of Hjertdal, in the west of Thelemarken	4,895;	Bassöe; 1846.
	4,927;	Bassöe; 1847.
Ævaberg, in the north of Tind, Thelemarken	4,629;	Bassöe; 1846.

THE AMTS OF NEDENÆS AND ROBYGDELAGET.

Aureskorefjeld, at Bykle, in Valle, amt of Robygdelaget	4,240;	Falkenberg; 1852.
Gruvletind, at Hyllestad Valle	3,767;	Næser; 1849.
Holmevasnulen upon Hekfjeld, between the amts of Lister and Mandal and Robygdelaget	3,277;	Bassöe; 1850.
Kislefjeld, at Hyllestad, Valle Robygdelaget	3,393;	Næser; 1849.
Kjondalsnuten, upon Hekfjeld, Robygdelaget	3,531;	Bassöe; 1850.
Ljomsnet, at Valle, Robygdelaget	4,155;	mean of two measurements; Næser; 1849.
Rusfjeldhei, at Opslat, Bygland, Robygdelaget	3,510;	mean of two measurements; Næser; 1849.
Strandefjeld, at Bykle, Valle Robygdelaget	4,599;	Næser; 1849.
Svarvarnet, Valle Robygdelaget	4,499;	Næser; 1849.
Urœdalsknuden, at Valle, Robygdelaget	4,636;	Keilhau; 1839.
	4,739;	Falkenberg; 1832.
Vatndalsfjeld, at Bykle, Nalle, Robygdelaget	3,739;	Naumann.

THE AMTS OF LISTER AND MANDAL.

Bergcheia, at Siredal, amt of Lister and Mandal	3,496;	Bassöe; 1850.
Grubbaafjeld, at Lister	3,602;	Keilhau; 1839.
Hilleknuden, at Lunde, Tonstad, Lister	3,957;	mean of two measures by Falkenberg; 1852.
Holmevasnulen, upon Hekfjeld, between the amts of Robygdelaget and Lister and Mandal	3,274;	Bassöe; 1850.
Silfarrinden, upon Hekfjeld, at Lister	3,226;	Bassöe; 1850.
Skaaret, upon Hekfjeld, Lister	3,385;	Bassöe; 1850.

THE AMT OF STAVANGER.

Brandeknuden, at Aardal, in the district of Ryfylke	3,774;	Falkenberg; 1853.
Bakkedalsskardet, at Lunde annex, Ryfylke	3,359;	Falkenberg; 1852.
Faalasprangknuden, at Höle, Ryfylke	4,028;	Falkenberg; 1852.
Grodalsheia, at Höle, Ryfylke	3,719;	Falkenberg; 1852.
Hustveitsaata, at Suledal, Ryfylke	3,941;	Falkenberg; 1853.
Lysekammen, at Höle, Ryfylke	4,344;	Falkenberg; 1852.
Napenfjeld, at Suledal, Ryfylke	4,427;	Falkenberg; 1853.
Skaapknuden, at Höle, in the district of Ryfylke	3,185;	Falkenberg; 1852.
	3,153;	Falkenberg; 1852.
Natlandsnuden, at Suledal, Ryfylke	3,138;	Falkenberg; 1853.
Skaulen, at Suledal, Ryfylke	5,172;	Næser; 1853.
Snenuten, at Suledal, Ryfylke	5,266;	Næser; 1853.
Vasdalseggen, between Suledal and Vinje, Ryfylke	5,433;	Næser; 1853.
Venjokula, at Birkreno, Yœderen	3,028;	Falkenberg; 1852.
Vingenuten, at Suledal, Ryfylke	3,587;	Næser; 1853.

THE AMT OF SOUTH BERGENHUS.

Fjeldsjöberg, at Graven, in the district of Hardanger, one mile south of the boundary of the stift of Bergen	4,756;	Bassöe; 1846.
Folgefonden, snow-covered mountain at Hardanger:		
Njuen, upon the northeast corner	4,933;	theodolite; Sexe; 1859.
	5,034;	Sexe; 1859.
The lower edge of the snow upon Valaberg, (glacier,) half mile southwest of Njuen	3,628;	Sexe; 1859.
Buersdal, the lower edge of the glacier, (Isbræ)	3,656;	Sexe; by theodolite; 1859.
The lower edge of the snowbræ at Odde	3,536;	Sexe; 1859.
Blaavand, (lake,) at the lowest edge of the glacier	3,391;	Sexe; 1850.
The lowest edge of the glacier six miles west of the estate of Brække, at Odde	3,542;	Sexe; 1859.
Kjarengbotten, six miles from the estate Ekemo, at Aakrefjord, the lower edge of the glacier	2,669;	Sexe; 1859.
Upon the summit of the snowbra, three miles southwest from Melderskin	4,386;	Sexe; 1859.
Urabotten, the lower edge of the snowbra east of Ore, at Mauranger	3,400;	Sexe; 1859.
The lower edge of the snowbra northeast of Ore, Mauranger, at the northwest corner of Folgefond, at a lake called Iuklavandet	3,261;	Sexe; 1859.
The mountain plateau under the north edge of the Folgefond	4,567;	Falkenberg; 1855.
The greatest height of Folgefond, Hundsöere	5,456;	Hertzberg.
	5,322;	Smith.
	5,322;	Næser; 1852.
Regnenuten, which, according to the observations of Professor Naumann, is the highest summit of Folgefond	5,396;	Naumann.
	5,343;	Smith.
Thorsnuten, upon the north of the Folgefond	5,250;	mean of two measures; Næser; 1852.
	5,169;	mean of two measures; Næser; 1852.
	4,941;	Sexe; 1859.
Upper plateau above Tokheim, six miles from Söræfjord	5,456;	Hertzberg.
Saxaklep, one of the highest points north upon Folgefond	4,632;	Naumann.
Solnuten, opposite Ullensvang	4,786;	Hertzberg.
	4,834;	Sexe; by levelling.
The highest point of the highway between Rusæter and Iondal, close by Saxaklep	4,519;	Naumann.
Gjonekvittingen, (Smith,) Gronekvitingin, (Sommerfeldt,) (Ionakvitingen,) between Haalandsdal and Strandebarm, in the district of Hardanger	3,899;	Smith.
	3,843;	by Kraft.
Gulfjœld, at Samnanger, in the district of Söndhordland	3,239;	mean of three measures; Næser; 1852.
	3,088;	Naumann.
Haafjeld, at Elm, Söndhordland	3,159;	Falkenberg; 1853.
Hallincæjökelen, the highest mountain in Hardanger	5,713;	at a point upon Hallingjökelen, 206 feet below the highest summit.
	5,352;	Suhrland; 1842.
Hallinskarven, between Hallingdal and Hardanger	6,073;	Keilhau; 1849. This mountain does not appear to be much over 5,000 feet, but is one of the highest points between Hallingdal and Hardanger. Naumann, Smith, Forsell, Olsen, and Heissinger, are all giving precise heights, but in their time this mountain was not yet measured. (Vibe.)

Tolaskardskarven, the most easterly and highest horn of Hallingskarven, about the middle of this mountain, at Aal, in the district of Hallingdal	6,431.	In the immediate vicinity of Tolaskardskarven are several yet higher horns, but those are within the boundaries of the stift of Bergen; those are Iökulen, which is calculated to be 6,536; Vosseskavlen, 6,742; Införebraen and Blaaskavlen, east of Urland and north of Vosseskavlen, 6,176; Wergeland; 1845.
Præstholtskarven, horn upon Hallingskarven	6,330.	Further to the south are several higher mountain backs, of about 100 feet higher than this.
Halnekollen, between Hardanger and Numedal	4,539;	Bassöe; 1846.
Hardangerfjeld, dividing ridge of the east and west mountains, or mountain division:		
Haarteigen, at Kinservig, Hardanger	5,567;	Smith and Hertzberg.
	5,549;	Keilhau and Suhrland; 1842.
	5,540;	Næser; 1852.
Hondalsnuten, at Vang, in the district of Voss	4,782;	Falkenberg; 1856.
Hundsöira, see Folgefond.		
Högeheia, at Eldfjord Hardanger, near the line dividing the districts of Hardanger, Numedal, and upper (övre) Thelemarken	4,672;	Bassöe; 1846.
Ildebrudfjeld, at Vangen, in the district of Voss	3,633:	Falkenberg; 1856.
Ildhusfjeld, at Hosanger, in the district of Nordhordland	3,923;	Falkenberg; 1856.
Införebraen, see Hallingskarven.		
Iengledsnulen, at Eldfjord, Hardanger	4,541;	Bassöe; 1846.
Iökelen, see Hallengskarven.		
Kallenuten, upon the boundary between the districts of Voss and Hardanger	4,471;	Næser; 1852.
Kvetenaase, at Vangen, Voss	4,751;	Falkenberg; 1856.
Kvetingen, at Strandebarm, in Hardanger	4,250;	mean of two measures; Næser; 1852.
	4,123;	Falkenberg; 1856.
Lörhæörje, at Vangen, Voss	5,135;	Falkenberg; 1856.
Melderskin, see Folgefond.		
Nupseggen, at Röldal, Hardanger	5,753;	mean of three measures; Næser; 1852.
	5,667;	Næser; 1853.
Præstholtskarven, see Hallingskarven.		
Raveldseggen, at Ullensvang, Hardanger, near the church	4,497;	Von Buch. This range of mountain rises some hundreds of feet higher, but is of about even height for thirty-five or forty miles in distance.
Skupsjonuten, near Eidsfjord, Hardanger	4,665;	Bassöe; 1846.
Solbjorgnuten, at Standebarm, Hardanger	4,290;	Falkenberg; 1856.
Steinbuhein, at Eidsfjord, Giaven, Hardanger	4,319;	Wergeland; 1845.
	4,602;	Bassöe; 1846.
Storrust, upon the boundary between Voss and Vigör, Hardanger	3,977;	Næser; 1852.
Salhefonden, at Ullensvang, snow-covered mountain, Hardanger	4,778;	Smith.
	4,729;	Topographical and Statistical Collections, page 30; notes of Forsell 4,850 Swedish feet, but it is observed by Major Vibe that he adopted the measurement made by Smith, without himself having measured it.
Trondevardnuten, at Eidsfjord, in the district of Hardanger	4,644;	Bassöe; 1846.

Vasfjæren, at Ulvig, Hardanger	5,353;	Næser; 1852.
	5,361;	Falkenberg; 1856.
Vesholda, at Strandebarm, in the district of Söndhordland	3,520;	Falkenberg; 1865.

THE AMT OF NORTH BERGENHUS.

Dyrhougen, east of Skagastölen, at Fortûn, in the district of Sogn	4,009;	mean of three measures; Holmboe; 1829.
Dyrhougstinden, near Skagastölstinderm, Sogn	6,538;	Bohr.
Haldalshögda, upon the south side of Hildal, between Borgund, Urland, and Hallingdal	5,749;	Wergeland; 1845.
Iökuleggen, the highest point, also called Högeloftseggen, between Hemsedal and Valders	6,291;	Wergeland; 1845.
Kamphammerne, at Lom, in the district of Gudbrandsdal	4,272;	Langberg.
Koldetind, at Koldedal, in the district of Inner Sogn	7,247;	Bouk and Keilhau.
Kvanshift, (store,) north of Dalsfjord, at Söndfjord	3,705;	Naumann.
Lodalskaupe, at Nordfjord:		
The south horn, probably the Little Lodalskaupe	6,292;	Bohr.
The eastern or highest horn	6,596;	(namely, 304 feet higher than the south horn;) by levelling; Bohr.
The base of the store Lodalskaupe	3,993;	Naumann.
Snekuppe, between the two Lodalskaupes	6,496;	Naumann.
Ringstinderne, at Aûrdal, in the Inner Sogn, two horns of the same height	6,176;	Holmboe.
Skagastolslinderne, several mountain horns in the inner Sogn:		
See Nautgardstinden	7,566;	Keilhau and Boeck.
	7,185;	Naumann.
	7,180;	(by geometrical methods;) Bohr.
The northern horn	7,180;	by levelling; Bohr.
The horn furthest to the west, which is probably the same as above, is called the north	7,170;	Holmboe.
The middle horn	7,678;	Holmboe.
The eastern, (Horungen)	8,086;	Holmboe.
Sulutind, upon Filefjeld, at Serdal, Inner Sogn	5,887;	Von Buch.
	5,729;	Naumann
Sundalshammaren, between the east and west, running mountain stream, upon Langefjeld, between Sundal, at Opstryn, Norfjord, and Randal Lom	4,137;	Naumann.
Surnesælnaasen, at Krogen, in the Inner Sogn	4,028;	Ravenaasen and Melkekollen, on the opposite side of the fjord, of about the same height; Holmboe.
Tværaadalskirken, or Slændalskirken, in the centre of the snow-plain, between Lom and Gudbrandsdal, in the stift of North Bergenhus	6,885;	Wergeland; 1842.
Vangsnen, at Iûstedal, Inner Sogn	5,864;	Holmboe; 1829.

THE AMT OF ROMSDAL.

Hjelmen, northwest of Bærufjord, district of Nordmr.	3,201;	Suhrland; 1841.
Romsdalshorn, at Gryten, in Romsdal	4,117;	Naumann.

THE AMT OF SOUTH DRONTHUM.

Dovre range of mountains:		
Knutshoe, east of Kongevold, upon the boundary between Gudbrandsdal and Opdal	5,456;	Brock; 1826.
Kolla, west of Driva river, four miles in northwest from Hjærkin	5,548;	Brock.
Nunsfjeld, Stenkolla, Skrunskolle	6,794;	1,030 feet higher than the mountain pass between Læssö and Opdal; Naumann.
Rottesjohoe, four miles northeast from Hjorkin	5,412;	Brock.
Rödalshoiden, north of Foldal	4,800;	Kraft.
Graasia, at Tydal, Srinden, and Sælbü	3,398;	Hörby.
Grythatten, at Opdal, in the district of Guldal	4,526;	Suhrland; 1841.

Haftorslot, on the Swedish boundary, at the parish of Rörass	3,762;	Hörby; 1854.
Hammerklep, (the little,) at Trysil, near the Swedish boundary	3,257;	Hörby; 1854.
Langsövola, at Tysil, near the Swedish boundary	3,047;	Hissinger.
Reisefjeld, at Meldal, in the district of Örkedal and Guldal	3,773;	Heiberg; 1859
Rulefjeld, upon the Swedish boundary, east of Lake Aursuen	3,639;	Hissinger.
Skarsfjeldene, upon the boundary between Tydal, in Islbo, and Herjedal, in Sweden; the highest horns, Grönstenstop	4,964;	mean of two measures; Hörby; 1854.
Granittop	4,993;	Hörby.
North Skarsfjeld, upon the Swedish boundary	4,705;	mean of two measures; Hörby; 1854.
Skibrofjeld, upon the Swedish boundary, in the parish of Rörass; boundary mark No. 146	3,603;	by theodolite; Hertzberg; 1849.
Sylfjeld, upon the Swedish boundary, between Sælbu and Yemteland; the highest horn, Syltoppen	5,869;	Hissinger.
Vegelfjeld, upon the Swedish boundary, between Lake Ferugen and Herjedal, in Sweden	4,618;	Other points of this range of monntains rises some hundred feet higher. Hissinger.
Vegelskaflet, a lower top of the Veögelfjelds, in the parish of Rörass, boundary mark No. 147	3,965;	Hertzberg; by theodolite; 1849.
Oefjeld, at Tydal; Sælbu, west of Esand	4,250;	Hörby; 1854.

THE AMT ON NORTH TRONDHJERN.

Bergsluen, upon the mountain called Gluken, near the Swedish boundary, at Meraœger, in the district of Stördal	3,270;	Hörby.
Fosdalsfjeld, the highest horn in the district of Inderöen	3,602;	Keilhau; 1831.
Gusledpiggen, in the district of Inderöen	3,304;	Keilhau; 1831.
Hermansnaasen, in the upper part of the district of Vardal	3,773;	(geometrical method;)Schult.
Jomafjeld, near the Swedish boundary	3,767;	Keilhau; 1831.
Jærsofjeldene, in the district of Inderöen	Greatest height 4,323;	Keilhau; 1831.
Kjölhaugen, near the Swedish boundary, east of Stordal	4,190;	Hissinger.
Sibmeken-Soupts, upon the Swedish boundary, in the north of Namedal, the lower edge of the glacier	3,443;	Keilhau; 1831.
Sibmekfjeld, at the Swedish boundary, district of Namdalen	About 4,735;	Keilhau.
Skjækerhatten, the horn farthest to the north of Skjækerfjeldene, in the east of Sparbuœ, in the district of Inderöen	4,801;	Schult.

THE AMT OF NORDLAND.

Buextinderne. (See Höitinderne.)		
Brurskanken, at Vefsen, Helgeland	4,338;	Suhrland; 1843.
Börsvatstinderne (See Höitinderne.)		
Habris-Vaggie. (See Höitinderne.)		
Hatten or Hatfjeld, at Helgeland	3,781;	Sohrland; 1843.
Himmeltinderne. (See Bensjortinden, in the Finmark.)		
Höitinden, between Glomsfjord and Byerfjord, at Saeten	4,476.	There is another horn further to the westward, perhaps a little higher. Upon the west side of Buerfjord rises Stormyrtinden, Habris-Vaggie, and Romel Zhrok to a height of at least 3,600 feet. Further to the northward is a group of mountain horns called Buertinderne or Smaatinderne, Sandvats, and Börsvatstinderne, which all are over 3,000 feet high. Keilhau.

Jadnemsriset—point of the Swedish boundary; also between the amts of Dronthum and Nordland	3,933.	Other points upon this range of mountains are higher.—Keilhau; 1831.
Rigs-grandsen, (the boundary between Sweden and Norway;) middle height in Vessen, between boundary marks Nos. 207 and 208, near Harevand, (lake,) Helgeland	3,278;	Suhrland.
Ronsdalsfjeld, at the Opot. (See Bensjordtinden.)		
Sandhornet, at Gildeskaal, district of Salten	3,268;	Everest.
	3,184;	Essing.
Strandofjeld or Strandaafjeld, at Folden, Satten	3,202;	(by geometrical method;) Wahlenberg.
Sulitelma, near the Swedish boundary, at Salten:		
The base of the horn	5,902;	Wahlenberg.
The horn Almajalus	5,542;	Wahlenberg.
The North Saulo	5,658;	Wahlenberg.
Lomme-Javre	2,265;	Wahlenberg.
Troldtinderne. (See Strandefjeld.)		
Vefsen, at Helgeland, Brurskanken	4,338.	In the vicinity of this is situated Gedetind, about 200 feet higher. Suhrland.

THE AMT OF FINMARK.

Arbust-tinden or Aarbust-tinden. (See Bensjordtinden.)		
Akka-Oalgek, in the district of Talvig	3,395;	Von Buch.
Arnö, in the district of Senjerö and Tromsö	about 3,000;	Von Buch.
Bensjordtinden, between Balsfjord and Malangen	4,013;	Everest.
Tromsdalsfjeldet, opposite Tromsö, the group Gerragas Zhjokko, or the glacier south of the junction of the rivers Bardu and Mols, Faxetinden, east of Andergö, Arbusttinden upon Andorgö, and Nonsdalsfjeldet, at Ofoten, all of these reach between 3,000 and 4,000 feet; also Vandtinden, upon Vandö, Tussen upon Grytö, Mosadlen upon Hendö, Vaagekallen upon east Vaage, and Himmeltinderne upon west Vaage, reach a height between 3,000 and 4,000 feet; Keilhau.		
Golze-Varre, (Kavringtinden,) two miles south of Lyngsudet	4,195;	Keilhau. The horn Gaatzagaesa, and several others, reach over 4,000 feet; Otterfjeld, Olmai Varre, at the upper end of Lyngenfjord, reach to about 4,000 feet; Keilhau.
Iedke, the highest mountain upon the island of Sieland, south of Hammerfest	3,556;	mean of five measures; Lundh; 1845. Iedke is covered by eternal snow.
	3,556;	mean of five measures made upon different days; Kloumann; 1847.
	3,495;	zenith distances, by Kloumann, from two points; 1846.
Kaaven, west of Langfjord	3,121;	Kloumann; 1847.
	3,105;	by zenith distances, from two points; Kloumann; 1847.
Kjærringfjordfjeld, (great,) upon the island of Stjunö, at Rogersund	3,136;	mean of three measures; Lundt; 1845.
Nonskarfjeld, upon the island of Stjernö, in the district of Talvig	3,551;	Von Buch.
Seiland, island in the Finmark	3,540;	Keilhau.
Storvandsfjeld, south of Storvand, (lake,) at Talvik...		
Vandtinden. (See Bonsjordlenden.)		
Nordkap, upon Magerö, in the Finmark	1,009;	Von Buch

LAKES.

The length and breadth of the principal inland lakes of Norway, measuring over one Norwegian mile in length.

Name and location of lakes.	Length in English miles.	Breadth in English miles.	Height above the sea in English feet.
Mjosen, situated in about the centre of the stift of Christiania, in the amt of Agershus; the largest lake in Norway.	54.08	1.52	425 feet, mean of five measures, four in July and the fifth on the 5th of August, by Hissinger; 426, mean of several measures in June, 1837, Keilhau; 415, mean of two measures, July and August, 1841, Keilhau and Suhrland; 444, mean of two measures in the latter part of May, 1841; 422, mean of three measures made on the 6th, 21st, and 26th of July, 1843, Keilhau; 408, Hörby, June 30, 1848; 384, Hansten, by theodolite, August 15, 1847.
Rosvandet, at Vessen, Helgland, in amt of Nordland.	12.00	12.00	1,378, mean of three measures, by Suhrland, 1843.
Randsfjord, at Land, in the district of Hadeland, amt of Christians.	38.04	2.28	309, Von Buch; 491, Esmark; 341, Hansten; 432, mean of four measures upon three different days, Keilhau, 1838; 319, Carpelan; 508, measured at Rodnas, the upper end of the lake, Keilhau, 1845; 473, at Jonvold, Keilhau, 1845.
Famundsö, at Osterdal, in the amt of Hedemarken.	33.48	0.56	2,250, Hissinger; 2,188, mean of six measures, Hörby, 1848; 2,194, mean of twelve measures, Hörby, 1854.
Tyrifjord, at Ringerige, in the amt of Buskerud.	15.02	6.84	193, Keilhau, 1838; 242, Mashmann; 226, Keilhau and Suhrland, 1842; 228, at Holsfjord, Næser, 1852.
The chain of lakes at Fredrikshald, in the amt of Smaalenene; height measured at Femiöen.	36.52	1.52	226, Boeck, 1834.
Store (great) Leesö, near the Swedish boundary, in the amt of Smaalehnene.	30.44	2.00	340, Keilhau and Boeck, 1834; 422, mean of two measures by theodolite, Hertzberg, 1849.
Tindsö, at Tind, district of Thelmark, amt of Bratsberg.	22.84	3.80	570, Esmark; 630, Hansten; 609, Mashmar; 550, Smith; 659, mean of several measures upon two days, Keilhau and Suhrland; 667, mean of two measures, Næser, 1846, at high water; 581, Næser, 1853.
Otavand, or Vaagevand, at Gudbrandsdal, in the amt of Christians.	22.84	0.76	1,163, Naumann; 1,126, Broch; 1,214, theodolite, by Hansten, mean of thirty-six measures, in July and August, 1844; 1,208, mean of twenty-six measures in July and August, 1844, Andersen.
Niservand, at Nisedal, Telemark, in the amt of Bratsberg.	21.28	1.52	787, mean of four measures, Keilhau and Suhrland, 1840; 873, mean of three measures upon different days, Næser, 1847; 758, by levelling, Sandborg.
Kröderen, in the amt of Buskerud, district of Buskerud.	21.28	0.76	471, Keilhau, 1838; 441, mean of two measures, Keilhau and Suhrland, 1842; 417, Wergeland, 1840.
Mjösvandet, at Tind, Thelemark, in the amt of Buskerud.	18.24	3.04	2,844, Smith; 2,899, Bassoe, 1846; 3,012, mean of three measures, Næser, 1853.

Length and breadth of the principal lakes in Norway—Continued.

Name and location of lakes.	Length in English miles.	Breadth in English miles.	Height above the sea in English feet.
Snaasen-Vand, district of Inderöen, in the amt of North Drontheim.	18.24	2.28	66, Keilhau, 1831.
Nordsjö, in the lower Telemark, amt of Bratsberg.	18.24	1.52	77. This lake was formerly higher, but was lowered to the present stand by an earthquake. Carpelan; from 62 to 72, Keilhau; 128, Næser, 1844.
Byglandsfjord, at Raabygdelag, amt of Nedenes.	18.24	1.12	661, mean of two measures, Næser, 1849.
Storsjön, at Rendalen, in the amt of Hedemarken.	18.24	.76	843, mean of four measures, upon two days, Horby, 1848.
Sælbusjö, in the district of Strind and Sælbu, amt of Seuth Drontheim.	16.72	2.28	516, Naumann.
Fyrrisvand, at Moland, Telemark, amt of Bratsberg.	15.02	3.04	853, Sandborg, by levelling.
Öieren, at Lower Romerike, amt of Smaalenene; the junction of the river Nld and Glommen.	15.02	3.04	358, Boeck, 1834; 348, Keilhau, 1834. These measures were made at somewhat high-water stand; the high-water mark is from 24 to 28 feet (Norw.) above low water. In the year 1789, this lake rose 34 feet above high-water mark.
Toke-vand, district of Bamle, amt of Bratsberg.	15.02	1.52	120, Næser, 1847.
Vinstervandene, at Gudbrandsdal, Valders, amt of Hedemark.	15.02	1.52	3,276, Broch. This measure was made on the ease side of the middle lake, at a fishing hut there.
Lemingen, at the Swedish boundary, in the district of Namdal, amt of North Drontheim.	13.68	3.04	1,332, Keilhau, 1831.
Siredalsvan, at Lister, in the amt of Lister and Mandal.	15.02	.76	124, mean of two measures, Keilhau, 1839; at common stage of water.
Osen, at Trysil, in the amt of Hedemark.	14.44	1.52	1,473, Keilhau, 1838.
Nams-vandene, at Namdal, in the amt of North Drontheim.	13.68	3.04	1,343, Keilhau, 1843; measured at the west lake.
Bygdin, in the district of Valders, amt of Christians.	13.68	1.52	3,623, Keilhau and Boeck; 3,623, Broch; 3,456, mean of several measures, Wergeland, 1843.
Herningsdal-vand, at Nordfjord, in the amt of North Bergenhus.	13.68	1.52	159, by levelling, Kragh.
Sulidals-vand, or Soledals-vand, at Ryfylke, in the amt of Stavanger.	13.68	1.52	143, Naumann; 267, Falkenberg, 1853; 269, mean of two measures, (under very unfavorable circumstances,) Naumann, 1853.
Totak-vand, at Vinje, Telemark, amt of Bratsberg.	12.	3.80	2,234 Maschmann.
Farres-vand, in the district of Laurvig, amt of Jarlsberg and Laurvig.	12.	1.52	Height not measured, or cannot be ascertained.
Lundevand, district of Lister, amt of Lister and Mandal.	12.	1.12	Height not measured.
Bandags-vand, at Laurdal, district of Thelemarken, amt of Bratsberg.	12.	1.12	219, mean of several measures, Maschmann; 232, Bassoe, 1847.
Spirilensö, at Aadalen, district of Ringerige, amt of Buskerud	12.	.76	535, Keilhau.

Length and breadth of the principal lakes in Norway—Continued.

Name and location of lakes.	Length in English miles.	Breadth in English miles.	Height above the sea in English feet.
Stor-sjön, at Odalen, in the amt of Hedemarken.	11.40	5.32	926, Esmark; 843, mean of four measures made upon two days; Hörby, 1848.
Slidrefjord, at Valders, in the amt of Kristian.	11.40	0.76	1,255, Naumann; 1,194, Keilhau, 1845.
Norefjord and Tlovand, at Numedal, amt of Buskerud.	10.64	1.52	904, Naumann, measured at Norefjord.
Engersö, at Trysil, in the amt of Hedemarken.	10.64	0.76	1,575, mean of three measures; Keilhau, 1838.
Tunhovdfjord, at Numedal, amt of Buskerud.	9.88	1.52	2,316, Wergeland, 1842.
Læssö vand, at Gudbrandsdal, amt of Kristian.	9.88	0.76	1,714, Naumann.
Istersö, at Odalen, amt of Hedemarken.	9.12	2.28	Height not ascertained.
Ekernsö, at Fiskum, amt of Buskerud.	9.12	1.52	65, Næser, 1852.
Hurdalsö, at the upper Romerige, amt of Acekershœs.	9.12	1.52	564, Keilhau, 1837; 594, Keilhau and Suhrland, 1841.
Vegaardvandet, at Gjerrestad, amt of Nedenæs.	9.12	1.52	541, mean of three measures, Suhrland and Ellesen, 1840; 624, Næser, 1849.
Hitterdals-vand, at Thelemarken, amt of Bratsberg.	9.12	1.12	118, Næser, 1846.
Gjendin, at Vaage, district of Gudbrandsdal, amt of Kristian.	9.12	0.76	3,248, mean of six measures, Wergeland, 1841; 3,274, mean of five measures upon different days, Wergeland, 1843.
The Little Mjösen, or Vangs Mjösen, at Valders, amt of Kristian.	9.12	0.76	1,535, Hansten; 1,542, Naumann; 1,565, result of several measures, Wergeland; 1,532, mean of two measures upon different days, Keilhau, 1845.
Gjerrestads-vand, at Nedenæs, amt of Nedenæs.	9.12	0.76	No height given.
Strandsfjord, at Hallingdal, amt of Buskerud.	9.12	0.76	1,529, Naumann.
Tunsöen, at Numedal, amt of North Drontheim.	8.36	4.56	Height not given.
Tyen, mountain lake, at Filefjeld, inner sogn, amt of North Bergenhus.	7.6	3.04	3,623, vol. 2 of Budstikken, page 388; about of same height as Lake Bygdin.
Færensö, at Stördal, amt of North Drontheim.	7.6	3.04	Height not given.
Opstryn, at Nordfjord, amt of North Bergenhus.	7.6	1.32	82, by levelling, Krag.
Solensö, or Sörulensö, North Österdal, amt of Hedemark.	7.6	1.52	No height given.
Siljords-vand, at the Parish of Siljord, amt of Bratsberg.	7.6	0.76	Height not given.
Kalhovd-vand, on the lower Maar-vand, at Tind, Thelemarken, amt of Bratsberg.	7.6	0.76	3,500, Keilhau and Suhrland, 1842.
Feragen, east of Röraas, amt of South Drontheim.	7.	1.52	2,271, Hissinger.
Vansjö, near and east of Moss, amt of Smaalehnene.	6.08	6.02	62, Boeck and Keilhau, 1834.
Eina-vand, upon the Toten, amt of Kristian.	6.08	1.12	1,252, Esmark; 1,338, Suhrland, 1841.
Aursund-sö, or Aursuen-sö, at Röraas, amt of South Drontheim.	6.08	0.76	2,252, Naumann; 2,408, Hesinger, mean of six measures; 2,262, Hörby, mean of six measures

Length and breadth of the principal lakes in Norway—Continued.

Name and location of lakes.	Length in English miles.	Breadth in English miles.	Height above the sea in English feet.
Sjakervand, or Skjakervand, west of the Skjakerfjeld, amt of North Drontheim.	6.08	0.76	1,541, Schult.
Nordmandslaagen-sjö, the head waters of Logenelv, upon the mountain plateau of Hardanger, Hardanger, amt of South Bergenhus.	7.16	0.76	4,153, Keilhau, 1845.

THE SNOW-LINE.

Altitude of the snow-line above the level of the sea.

Upon the mountains south of Ottevand, in Gudbrandsdal, in about 62° latitude........................4,825 feet; Brock.
Upon the Dovre mountains in 61° of latitude..........5,353; Naumann.
At Gjardesdal, Mauranger, in the district of Hardanger, amt of South Bergenhus, in latitude 60°............3,575; Sexe, 1859.
At Lodalskaupe, at Nordfjord, in the amt of North Bergenhus, in latitude 62°..........................5,542; Von Buch; 5,414; Bohr.
In the mountains about Storhougen, between Lyster and Iustedal, Inner Sogn, amt of North Bergenhus, in latitude 61°.......................................5,353; Keilhau.
In the mountain range between Iölster, at Söndfjord and Invigsfjord, at Nordfjord, amt of North Bergenhus, latitude 61°.......................................4,117; Naumann.
Upon the west side of the island of Seiland, amt of Finmark, latitude 70° 5′..........................2,965; Keilhau.
The mean height of the snow-line upon the island of Seiland, latitude 70° 5′..........................3,088; Keilhau.

The height of the snow-line as established by Professor Leopold von Buch.

Commencing under latitude 60° 00′, at 5,661 feet over the sea.
Commencing under latitude 61° 00′, at 5,353 feet over the sea.
Commencing under latitude 62° 30′, at 5,012 feet over the sea.
Commencing under latitude 63° 30′, at 4,632 feet over the sea.
Commencing under latitude 67° 00′, at 3,705 feet over the sea.
Commencing under latitude 70° 00′, at 3,397 feet over the sea.
Commencing under latitude 71° 00′, at North Cape, 2,265 feet over the sea.

Explanation of terms and names of mountains.

Aas, common name of a lower mountain-back or ridge, often covered with timber. This term is rarely used in the north of Norway.
Berg, bjerg, mountain; the former more common.
Brat, a very steep mountain. This term is only used in the south of Norway. Sometimes it means a high mountain, seen some distance at sea; mostly used by seamen and pilots. In the north the word *roe* denotes the same.
Bræ, bre, fond, mountains covered by eternal snow or ice. Bræ mostly means ice-covered, such as Josteretatsbræen. Fond always means snow-covered. Folgefond.
Fjeld, mountain. Common term all through the country.
Fond, see Bræ.

Gust, light wind. Mountains are called so probably because the wind generally blows from the mountain. Vonsjogusten.

Haagn, a high but not cone-shaped mountain. Elgaahaagna.

Hammer, a steep, precipitous, and sometimes projecting mountain. This term is common throughout the country. Kampenhammerne.

Hat, *hette*, hat, or cap-formed shaped mountain, generally snow-covered Common throughout the country, but mostly from Dover northward. Skjækerhatten, Nehatten, Snehætten, Pighætten.

Haug, *Houg*, mostly in the south of Norway, signifying rounded, dome, or haystack-shaped mountain. Jemdalshougen.

Hei, *heid* mountain plateau. This term is common all through the country. Hogeheia.

Horn, common name of horn or alp-formed peaks. Skogshorn, Remsdalshorn.

Hovd, *hovde*, *hoved*. These terms are mostly used south of Dover, signifying large, projecting mountains. Synhovd, Reksjöhovd.

Hö, *hoe*, *hog*, *hogd*, *hoi*, *höide*, signifying large dome-formed mountains, sometimes cone-formed. Storhoi, Humdalshoe.

Jökel, *jukel*, common in the whole country, signfying glacier, or mountain covered with ice. Hallingjökelen, Jukeleggen.

Kaupe, common name of mountain covered with eternal snow. Lodalskaupe.

Kamp, common name south of Dover of dome-formed, often isolated mountain without peak. Askamoen, Svartekampen.

Kirke. This term is most common in the south of Norway, signifying high, precipitous, and projecting mountains. This name is also supposed to have been derived from legends of churches which before the reformation are said to have existed in the high mountains in the amt of Kristian. Synslaukjerken, Svarlhulkjerken.

Kjærring, mountain sometimes like the shape of an old woman in a sitting posture. Vehuskjærringa.

Kjöl, very common name of a mountain ridge dividing valleys. Roctingsjölers.

Klemp, a round dome-formed mountain. Bredklenklempen. This term is very seldom used.

Kep, *kleppe*, large and wide, but not alp-formed mountain. Blaahaaklep, Lenneklеppen.

Klet, common name, particularly at Österdal, signifying high, precipitous mountain. Solenkletten, Blaakaakletten.

Klub, *klump*. Klub is mostly used in the names of mountains situated upon the coast, signifying cone-formed, but in the interior more rounding or dome-formed mountains. Kjoihougklubben, Standaisklumpen, Kvitkjæmklubben.

Knip, *knippe*, common in the stift of Drontheim, signifying mountain appearing to be in many parts, or a bunch or cluster of mountain horns very close together. Grönsendknippen, Nysælerknippen.

Kolle, rounded or dome-formed mountain. Common term in all parts of the country.

Kule, only used in the south of Norway. A round or dome-formed mountain.

Kuv, in the interior signifying a mountain of considerable height, rising above others in the vicinity and not alp-formed; upon the coast, a round rock. Storkuven.

Kviting, light-colored high mountains, or sometimes when covered with snow. Gjönekvitingen, Kvitingen, Kvitingskjölen.

Naas nase nos, steep, precipitous, but not pike-formed mountain. Nybunaaset, Maarsnaas, Hermansnaasen.

Nut, *nad*, *knat*, *knap*, common name of a flat, rounding mountain. Sölenatten, Huledjurnatten.

Nut, high, but not alp-formed mountain. This name does not appear in the south of Norway. Baasnuten, Borgundnuten.

Pig, high and alp-formed mountain. Elgupiggen, Storhöpiggen, Guslepiggen.

Skaft, high mountain with extended base, which, when seen from the side, appears as a very gradual descent. Vegelskaflet.

Skag, a term not always of the same signification. In the south it means a steep and high-rising mountain; in the north a mountain wall. Skaget. On the north coast it often denotes a nearly perpendicular mountain wall. Lappeskagen. Also, a low, naked, extended cape. Hærneskagen.

Skar, *Skard*, common in the whole country, signifying a mountain in which appears to be a rift, or to be cloven, notched, or a narrow crevice. Dyrskardet, Gaakelskardet, Brudskardet, Skarifjeldene, Sælerskardet.

Skarv, a high, naked mountain, often covered with loose masses of rock, common in the southern amts of Norway. Hallingskarven, Söskarven, Storskarven.

Skavl, mountain with deep covering of snow. Vosseskavlen, Blaaskavlen.

Skolt, *pande*, name of broad, steep-rising mountains with rounded top.

Stöl, mountain meadow. Haastolen.

Stöt, a steep mountain with flat top. Havsterstöten. This term only appears near the Swedish boundary, and is believed to have been derived from the word Stötte, (mark,) from the presumption that at some former times there have been boundary marks upon these mountains.

Tind. This term is common over the whole country, signifying they upon larger mountain masses rising, cone-formed tops, alps, peaks. Naulgaardstinden, Suletind, Tessungtinden, Præstctind.

Top, *Tip*, *tup*, same signification as tind.

Varde. This term is often used in the south part of Norway, and denotes high mountain which might be seen a great distance at sea. Degervarde.

Vol is supposed to have the same meaning as Varde, but according to Hörby (New Magazine of Natural Science) it appears that this term, at least in some parts of the country, denotes a gradual ascending mountain, without having any mark or varde upon its summit. The term is common, particularly in the eastern portion of the country. Kvitvola, Bustvola, Finvola.

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CONTENTS.

REPORT OF THE SECRETARY.

GENERAL APPENDIX.

INDEX.